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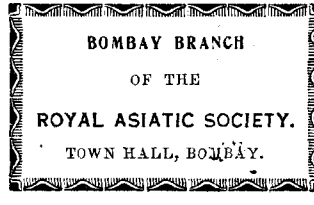

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Subinarine Telegraph

Committee Report

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REPORT

TO

THE LORDS OF THE COMMITTEE OF PRIVY COUNCIL FOR TRADE.

In compliance with the instructions given to us by your Lordships to inquire into "the "best form for the composition and outer covering of submarine telegraph cables," we have the honour to inform you that we have made numerous experiments and received the evidence of gentlemen conversant with the subject, and we beg to lay before your Lordships the following report.

In the first place, however, we must express to your Lordships how great was the loss we experienced soon after the commencement of our inquiry by the death of Mr. Robert Stephenson; his philosophical mind, his high scientific attainments, and his great practical knowledge peculiarly fitted him for directing an inquiry such as this, in which mechanical, chemical, and electrical science are combined; it was a subject, moreover, to which he had given considerable attention, and in which he took the greatest interest.

We have, however, had the benefit of his valuable assistance in originally devising the course of our proceedings, and in determining the general experiments which we deemed it necessary to make.

We propose to divide our report into three general heads, viz.:—

I. A short account of the principal telegraph lines which have been laid.

II. The construction and laying of submarine cables.

III. A summary of principles which we consider should govern these undertakings in future.

I.

An Account of the Principal Telegraph Lines which have been laid.

An Account of the Principal Telegraph Lines which have been laid.

We have appended to this Report a tabular statement, showing the principal submarine telegraph lines which have been laid. (Appendix No. 18.)

In 1840, Professor Wheatstone suggested to the Select Committee of the House of Commons on Railways the construction of a submarine telegraph between Dover and Calais, and subsequently further developed his plans; but the first efficient submarine telegraph which was actually laid was the line between Dover and Calais, projected by Mr. Brett, and completed in 1851.

The tabular statement shows that at the present time 11,364 miles have been laid, but of these little over 3,000 miles are actually working.

The following is a list of these lines:—

—	Length in Statute Miles.	Owner of Line.
Shallow Water Cables.		
Black Sea—Varna to Constantinople	172	Ottoman Government.
Black Sea—Varna to Balaklava	356	British Government.
Corsica and Sardinia	11	French Government.
Dacca-Pegu	116	Indian Government.
Dover and Ostend	80	Submarine Telegraph Company.
Dover and Calais (Grisnez)	25	Ditto.
Folkestone and Boulogne	24	Ditto.
England and Hanover	280	Ditto.
England and Denmark	350	Ditto.
England to Holland, Orfordness, and Schevening (four lines).	119¼ / 118¼	Electric and International Telegraph Company.
England to Holland, Mismeer to Zandvoort	136	Ditto.
Holyhead and Howth (1854)	73	Ditto.
Holyhead and Howth (1854)	73	Ditto.
Hurst Castle to Isle of Wight	1	Ditto.
Firth of Forth	5	Ditto.
River Tay	1	Ditto.

AN ACCOUNT OF THE PRINCIPAL TELEGRAPH LINES WHICH HAVE BEEN LAID.

	Length in Miles.	Owner of Line.
Shallow Water Cables—cont.		
Holyhead and Howth (1852)	73	R. and S. Newall and Company.
Portpatrick and Donaghadee	25	British and Irish Magnetic Company.
Portpatrick and Whitehead	26	Ditto.
Portpatrick and „ (1852)	15	Ditto.
Liverpool to Holyhead	25	Liverpool Dock Committee.
Majorca to Minorca	33	Spanish Government.
Denmark—across the Belt	18	Danish Government.
Denmark, Great Belt	28	Ditto.
Sweden to Denmark	13	Ditto.
Sweden to Gottland	64	Ditto.
Singapore to Batavia	550	Dutch Government.
Tasmanian, Bass Straits	240	Australian Government.
Prince Edward's Island to New Brunswick	12	
Whitehaven and Isle of Man	36	Isle of Man Electric Telegraph Company.
Weymouth to Alderney, Guernsey, and Jersey	93	Channel Islands Telegraph Company.
	3,074	
Deep Sea Cables.		
Athens to Syra and Scio	117	Greek Government.
Atlantic	2,200	Atlantic Telegraph Company.
Barcelona to Mahon	180	Spanish Government.
Corfu and Otranto	60	Mediterranean Extension Telegraph Co.
Dardanelles to Scio and Candia, from Scio to Smyrna	514	Levant Telegraph Company.
Iviza to St. Antonio	76	Spanish Government.
Iviza to Majorca	74	Ditto.
Newfoundland and Cape Breton	85	
Red Sea:—	Length.	
Suez-Cossire	294	
Cossire-Sunkin	545	
Sunkin-Aden	723	
Aden-Kooria Mooria	825	
Kooria Mooria-Muscat	559	
Muscat-Kurrachee	553	
	3,499	Red Sea and India Telegraph Company.
Sardinia and Malta, and Malta and Corfu	700	Mediterranean Extension Telegraph Company.
Sicily and Malta	70	Ditto.
Spezzia and Corsica	110	French Government.
Sardinia and Bona (Cagliari to Galita)	125	Ditto.
Toulon and Algiers	480	Ditto.
	8,290	

It will be perceived that of the lines, above 8,000 miles in length, which are not working, 6,949 miles belong to four undertakings only, viz.: the Atlantic, 2,200 miles; the Red Sea and India, 3,499 miles; the Sardinia, Malta and Corfu, 700 miles; and the Singapore and Batavia, 550 miles.

We have classed these lines under the respective heads of lines laid in shallow water and deep sea lines. Under the former we place all lines which are at such a depth as to be liable to injury from anchors of ships or dredges, or from strong tidal currents; these may be assumed to be in depths down to about 100 fathoms. Deep sea lines are those which are out of reach of those dangers, in depths considerably beyond 100 fathoms.

The particulars of the condition of the several cables are shown in the tabular statement before mentioned, and we therefore propose only to allude to a small number of the cables.

Shallow Water Cables.

Shallow Water Cables.

Dover and Grisnez Cable.

The line between Dover and Grisnez, laid in 1851 by the Submarine Telegraph Company, consisted of four copper conducting wires, each insulated with gutta-percha formed into a rope, covered with tarred hemp and protected with iron wires of No. 1 gauge. These wires were worked, with occasional injuries from anchors, till the spring of 1859, when extensive repairs were undertaken, in the course of which it was found that the gutta-percha covering of the copper wires was in as good order as when first laid, so complete had been the protection afforded by the tarred hemp and the immersion in water. The iron covering was, however, corroded in places, especially where it had lain on a bottom exposed to the action of moving water.

AN ACCOUNT OF THE PRINCIPAL TELEGRAPH LINES WHICH HAVE BEEN LAID.

Shallow Water Cables.

The Submarine Telegraph Company have four other lines between England and the Continent. One from Dover to Ostend with six conducting wires, laid in 1853, very similar to the Dover and Grisnez cable. In the others, instead of the single copper wire for a conductor, a strand of four copper wires was introduced. Of these one laid in 1858, between England and Hanover, is 280 miles long, containing two conducting copper strands, and weighing three tons per mile. The other two were laid in 1859—one, a very heavy cable, 24 miles in length, between Folkestone and Boulogne, weighing 9½ tons per mile, and containing six conducting strands of copper wire, insulated with gutta-percha and Chatterton's compound, twisted into a rope and served with tarred hemp, protected by iron wires of No. 0 gauge. The other is laid between England and Denmark, and is 350 miles long, weighing four tons per mile, and containing three conducting strands. The system of the Submarine Telegraph Company has thus been to place all the conducting wires they require for a particular route in one cable.

Ostend cable.

Hanover cable.

Folkestone and Boulogne cable.

Denmark cable.

The lines between England and Holland laid by the International Telegraph Company are five in number. The shallow sea which separates the English coast from Holland is navigated by numerous craft, which renders the submarine lines peculiarly liable to damage from ships' anchors. The shallow water, however, on the other hand, renders the repair or renewal of the lines a matter of little difficulty. Instead of the plan of the Submarine Company of placing several conductors in one cable being adopted in the first four lines laid by the International Telegraph Company between Orfordness and the Hague, four single wires were laid in four separate light cables, weighing about two tons to the mile: the four cables were all united to a large and very heavy cable for a distance of three miles from each shore, where ships were thought more likely to drop their anchors. The core is a double-covered gutta-percha wire, wrapped with tape and yarn, and externally covered with No. 8 galvanized iron wires laid spirally. The first cable was laid in a gale of wind, but this cable, as well as the three others, were all successfully paid out, and, setting aside the constant damage from ships' anchors, have generally worked well. The iron wire has, however, been seriously eaten away wherever it was exposed to moving water. It also rusts very fast in places where it rests in the mud on this side of the channel, but it has remained uninjured wherever it has been covered with the sand or silt at the bottom of the channel. The cables are sometimes found imbedded to great depths in the shingle on the English coast, and at other times entirely exposed. The lines were so frequently damaged, both accidentally, and occasionally wilfully, that their repair required the almost constant services of a vessel and crew, kept for the purpose; and therefore to put an end to so serious an expenditure, a large cable with four conducting wires has been laid down in their stead.

England and Holland cable.

The Electric Telegraph Company in laying these four cables left the entire responsibility of the design, construction, and laying of the cables to their own officers, and employed their own vessel, the contractor supplying the cable, and laying it under their instructions.

The Channel Islands Telegraph is worthy of notice. This cable, which is 93 miles long, was laid from Portland to Alderney, Guernsey, and Jersey in August 1858. It consists of a strand of copper wires, forming one conductor, covered with gutta-percha, and protected by iron wires, the weight being about 2½ tons per mile. The depth is nowhere greater than 60 fathoms. The cable is laid across a rocky bottom, traversed by a rapid tide; but in parts there is sand and shingle. At the landing place in Jersey the cable is laid among rocks, and the first accident took place there in the month of February 1859. The cable was not fixed to the rocks, and in a violent gale the waves caused it to strike repeatedly against the rocks, and broke it. A repetition of this injury was prevented by clamping the cable down to the rocks by means of iron forks leaded into the rocks. The next accident occurred eight months after the laying of the cable four miles from the island of Portland, where the tide, which runs at five and six miles an hour, had caused the cable to work upon a ridge of rock in 25 fathoms water, which had worn it through; in repairing this the cable was taken up and relaid over shingle in another place. In other parts, where the iron covering of the cable was exposed, it became corroded, the rust which formed being continually washed away; in other places, when it rested on cement stone the wire was decayed, and in some cases the cable sunk into the cement stone; another cause of corrosion is stated to be the attachment of zoophytes, and vegetation to the cable.

Channel Islands cable.

In one case the cable was so much abraded on rocks, that half the gutta-percha and three inches in length of copper wire was laid bare and worn away, leaving only an oxide of copper exposed to the water in the groove in which the copper had lain; and defective as this portion was it is stated that weak currents could still be passed through the cable

An Account of the Principal Telegraph Lines which have been laid.

Another fault appears to have occurred from lightning. A thunderstorm took place in Jersey, and the lightning is said to have struck the wire on the land; a portion passing into the office, and destroying the instruments; another portion passing out in the land wires and producing small punctures; the remainder travelling 16 miles along the cable to within two miles of Guernsey, where it appears to have met with a weak place, and passed out into the water, producing a fault.

Shallow Water Cables.

Further particulars of the other cables will be found in the evidence and Appendix No. 18; and we propose, therefore, only to allude to two others. One was laid between Singapore and Batavia, for the Dutch Government, which weighed 21 cwt. per mile, and is similar in construction to the Red Sea cable, a cable which had been devised for a deep sea line. (See Table in Appendix No. 18). This line has failed, partly in consequence of injuries from anchors, and partly from corrosion of the outer covering, which was too weak for a shallow water cable.

Singapore and Batavia cable.

Tasmanian cable.

The other was manufactured by Mr. Henley, and laid by the Tasmanian Government, across Bass's Straits in 1859, in three sections, and weighed two tons per mile. One section has failed, and is to be replaced by a stronger cable. This section was abraded by the rocky bottom on which it was laid. On the same section a seaweed called kelp grows profusely, which, it is stated, enveloped and floated the cable in some places. The new line is proposed to be laid on a good sandy bottom, in an altered direction, which, had a careful preliminary survey been made, would have been selected at first, and would have prevented the failure which has occurred.

Deep Sea Cables.

Deep Sea Cables.

Atlantic cable.

We now pass to Deep Sea Lines, and amongst the first, notice the Atlantic Telegraph. In 1851, Mr. Tibbet, of New York, and Mr. Frederick N. Gisborne, an English engineer, proposed to shorten the communication between America and Europe, by making St. John's, Newfoundland, a port of call for Atlantic steamers, and constructing a telegraph from thence to join the American lines. In 1851, they obtained an Act of the Legislature of Newfoundland, which gave them the necessary powers, and also conferred upon them certain exclusive privileges. These gentlemen were, however, unable to fulfil the terms of the Act, and they transferred their interest to a new Company chiefly promoted by Mr. Cyrus Field, Mr. Dudley Field, Mr. Peter Cooper, Mr. Chandler White, Mr. Moses Taylor, and Mr. Marshall O. Roberts, and called the New York, Newfoundland, and London Telegraph Company.

This Company obtained an Act of Incorporation in 1854, which conferred, amongst other privileges, the exclusive right of landing cables on the coasts of Newfoundland or on the islands or places within the jurisdiction of the Government of Newfoundland for 50 years, and no limit was assigned to the time within which they were to exercise this right. In 1856 Mr. Cyrus Field, Sir Charles Bright, Mr. Brett, and Mr. Whitehouse entered into an arrangement with the New York, Newfoundland, and London Telegraph Company, by which the privilege of laying a submarine line between Europe and the coasts of Newfoundland and Labrador was transferred to them; but these privileges were to revert to the New York, Newfoundland, and London Telegraph Company, if not exercised before 1862. These gentlemen formed the Atlantic Telegraph Company, and obtained a grant of 14,000*l.* per annum from the British Government, to be paid when and so long as the line was in working order, and the American Government gave a similar guarantee. Upon this, the Provisional Directors of the Company raised 350,000*l.*, in shares of 1,000*l.* each, and they undertook that the line should be laid in 1857. It was not until after this had been done that the permanent Board of Management was formed.

Some experiments upon forms of cable best suited to the purpose were made at Messrs. Glass and Elliot's works. The form selected was calculated to bear three tons. It consisted of a strand of seven copper wires, each No. 22½ gauge, weighing 93 pounds per mile, covered with three coats of gutta-percha, weighing 227 pounds per mile, served with thread jute yarn, saturated with a composition of tar and other materials, and coated with 18 strands of iron wire, each strand containing seven wires, each of No. 22 gauge; but it is to be regretted that there was no such thorough preliminary investigation as the importance of the subject demanded, because the undertaking given that it should be laid in 1857 did not afford the time necessary for such an inquiry. Mr. Whitehouse (Question 1838) states that Mr. Cyrus Field objected to further experiments because it would have put off the laying of the cable for twelve months, and other persons admit that additional experiments should have been made.

Messrs. Newall contracted for the manufacture of one-half of the cable, Messrs. Glass, Elliot, and Co., contracted for the other half; and the manufacture was commenced in

February 1857. Messrs. Glass, Elliot, and Co. had no covered works in which to stow it, and the summer being very hot, the cable was partially injured by the sun. A considerable variation in the conductivity of the copper wire was observed during the process of testing, and a standard for the conductivity of the copper was consequently adopted, but not till nearly the whole cable had been manufactured. The cable, 2,500 miles long, was finished in July 1857.

AN ACCOUNT OF THE PRINCIPAL TELEGRAPH LINES WHICH HAVE BEEN LAID.

Deep Sea Cables.

Atlantic cable.

The United States steam-ship "Niagara" said to be a vessel of 5,000 tons, received the portion manufactured by Messrs. Newall, and Her Majesty's ship "Agamemnon," a vessel of 3,200 tons, received the portion made by Messrs. Glass and Elliot. The apparatus for the laying of the cable was scarcely complete when the ships arrived at Valentia. In the first expedition it was settled that the ships should proceed together from Valentia to Newfoundland, and as soon as one ship had paid out the cable on board, a joint was to be made with the portion on board the other ship; many objections were raised to this course, because it was evident that if the weather should happen to be bad when the joint was to be made, half the cable would be inevitably lost. The expedition left Valentia on the 7th August 1857, and the cable continued to be successfully paid out until the 11th August, when it broke in 2,000 fathoms of water, after about 335 miles had been paid out. The dynamometer indicated a strain of 35 cwt. Upon one occasion, before this fracture occurred, the ship had been stopped, and the cable held by stoppers without being paid out, so that it was not the actual weight of the cable which broke it, but, as stated by Sir C. Bright, an omission to ease the break as the stern of the ship was raised by the waves. After this accident the ships proceeded to Plymouth, and the cable was coiled into tanks on shore at Keyham. Upon examination it was found that the cable was injured either by the coiling and uncoiling, or by the original exposure to heat; and several bad places were cut out where the copper wire had forced itself through the gutta-percha. The cable was tested at Keyham, and the leakage reported to be high. But as it was not placed in water for fear of corroding the outer wires, no very accurate data were obtained. In the numerous tests made at Keyham the cable was cut in several places, and it would appear that sufficient care was not always exercised in making good the joints afterwards.

In the spring of 1858, the cable was placed on board the ships, and after some preliminary experiments upon paying out and raising portions of the cable, in the Bay of Biscay, it was determined to commence to lay the cable from mid-ocean, between Ireland and America. Two unsuccessful attempts to lay the cable were made, and the vessels returned to Cork, but they started again on the 17th July and accomplished the successful laying of the cable between Newfoundland and Valentia, on the 5th August. The communication between the ships during the paying out was kept up by a prearranged series of signals, and doubts were entertained during the laying of the final success of the cable, from the serious faults which became apparent. On the first occasion a sudden cessation of the current was perceived, but the insulation was good, and after a time the currents came again as strong as before. This could only be accounted for on the supposition that the internal copper wire had broken from the strain, and that when the cable in which it was had reached the bottom the two ends had been brought together again by the elasticity of the sheath. A serious fault of insulation appeared in the cable at about 420 miles from the Irish coast, after which signals could be transmitted only by the use of Professor Thomson's very delicate marine galvanometer. Signals continued to be received, sometimes better, sometimes worse, from the 5th August till the 1st September, when they ceased to be intelligible. Attempts have subsequently been made to repair the cable, but the decay from rust of the outer covering, which, as before mentioned, consists of strands of very fine wires, has prevented the cable being raised without breaking. Mr. Varley in his evidence (Question 2935) states that the speed with which words could be sent through this cable was little over one word of five letters per minute by relay; on Professor Thomson's galvanometer two words per minute were obtained.

We attribute the failure of this enterprize to the original design of the cable having been faulty owing to the absence of experimental data, to the manufacture having been conducted without proper supervision, and to the cable not having been handled, after manufacture, with sufficient care. We have had before us samples of the bad joints which existed in the cable before it was laid; and we cannot but observe that practical men ought to have known that the cable was defective, and to have been aware of the locality of the defects, before it was laid.

The next telegraph to which we come is the Red Sea and India Telegraph.

Red Sea cable.

The Red Sea and India Telegraph Company was formed in 1857–58 upon certain concessions obtained by Mr. Lionel Gisborne from the Turkish Government, which authorized a line of telegraph to be carried across Egypt and down the Red Sea, thus giving the necessary powers for the construction of a line to India. These concessions

AN ACCOUNT OF THE PRINCIPAL TELEGRAPH LINES WHICH HAVE BEEN LAID.

Deep Sea Cables.

Red Sea cable.

give the Company full control over the line, and permit through messages to be forwarded, either in cipher or otherwise, without being subject to examination on the part of the Turkish or Egyptian Governments. It also grants sufficient land for stations, and the necessary connecting land lines. Mr. Lionel Gisborne was the engineer, and Messrs. Newall and Co. contractors, for the projected line. In June 1858, after two unsuccessful attempts to lay the Atlantic Telegraph had been made, and before the third and successful attempt, the Government gave an unconditional guarantee of 4½ per cent. for 50 years upon the whole capital required for the construction of the Red Sea and India line, viz., 800,000*l.*, thus relieving the shareholders from risk in the matter; they appointed an official director who had a general control over the proceedings of the Company.

The cable consisted of a strand of seven copper wires, weighing 180 lbs. per nautical mile, covered with two coats of gutta-percha alternated with two coats of Chatterton's compound, weighing 212 lbs. per nautical mile. This core was served with hemp yarn tarred, weighing 1½ cwt. per nautical mile, protected by iron wires weighing 16 cwt. per nautical mile. This cable had consequently the largest copper conductor and the best insulation of any cable made up to that date. The whole length of line is 3,043 nautical miles.

The first portion, between Suez and Aden, was finished on the 28th of May 1859. It was laid in three sections, exclusive of the land line between Alexandria and Suez. The first section is from Suez to Cossire, 255 nautical miles in length; the second, Cossire to Suakin, 474 miles; the third, from Suakin to Aden, 629 miles in length. The portion of cable laid between Suakin and Aden tested much less perfectly before it was laid than the other two portions. The section from Suez to Cossire was in good order when laid. There was a fault on the Cossire-Suakin section, 135 miles from Suakin, when first laid, but not such as to prevent the line working, and for several months it did not get worse; the line has, however, since failed.

In February 1860 the Aden Suakin section failed, and it was found to have several faults; one appeared to be caused by the gutta-percha having been softened by heat, and the yarn having cut into it and bared the copper wire, and there appeared to be a defect caused by the flange of a drum having rolled over the core and cut into the copper wire; apparently before the core had been covered with yarn and wire. The Company endeavoured to repair the section, and laid down for the purpose above 300 miles of new cable. The communication was restored in July. The section, however, failed again five days afterwards. The pieces brought up from this portion of cable showed numerous places where the wire covering was entirely corroded away. In other places, the wires were as good as when first laid. In many places the cable was entirely covered with shells and weeds, and thus protected from corrosion. This section was laid very taut, and some of the injuries have been attributed to tight paying out, but the corrosion of the outer covering would to some extent account for the appearance of the pieces which have been brought up. Faults have also appeared in the Suez Cossire section.

The second portion of line, between Aden and Kurrachee, *was completed in February* 1860, but remained only for a short time in working order. *A portion, of about 70 miles,* is laid in depths of from 1,900 to 2,000 fathoms. The sections on this line are, from Aden to Kurrachee, 718 miles; from Hallain to Muscat, 486 miles; from Muscat to Kurrachee, 481 miles. The Aden Hallain section has a fault, supposed to be about 230 miles from Aden. The Hallain Muscat section is stated to be in good working order. The Muscat Kurrachee section has a fault close to Kurrachee, supposed to be caused by injury to the shore end from waves; but it would appear also to have other faults.

The Company have had no means on the spot for the repair of this line, nor have they a staff competent to give the information required to enable the condition of the line to be ascertained with accuracy; but there can be no doubt that, with a moderate expenditure and proper appliances, it would be capable of being made some use of.

It appears that the contract provided that the cable should be maintained in working order for 30 days by the contractor, and that the several separate sections were worked for a longer period; but that the whole was not worked throughout for that period. The speed at which messages could be sent through the longest section, viz., from Aden to Hallain, is stated by Mr. Forde to be about five words per minute, but we believe that with proper appliances the line would give a higher speed. We consider that the failure of this line is attributable to the cable having been designed without regard to the conditions of the climate or the character of the bottom of the sea over which it had to be laid; and to the insufficiency of the agreement with the contractor for securing effectual supervision during manufacture and control of the manner of laying. It is, moreover, to be regretted that the contract for laying this line was entered into without a full investigation into the question, considering that the success of the Atlantic cable was at the time very doubtful.

In addition to these lines, various submarine cables have been laid in the Mediterranean, of these we would specially notice the following:—

AN ACCOUNT OF THE PRINCIPAL TELEGRAPH LINES WHICH HAVE BEEN LAID.

Deep Sea Cables.

In 1854 the Mediterranean Extension Telegraph Company laid down, under concession from the French and Sardinian Governments, a submarine cable with six conducting wires, from Cape Santa Croce, Spezzia to Cape Corse, in Corsica. From Cape Corse land wires were carried to Bonifacio; from Bonifacio a submarine cable of six conducting wires was laid to Santa Theresa, in the island of Sardinia, whence land wires were laid to Cagliari and Cape Spartivento. From Cape Spartivento a submarine cable was laid in 1855 to Bona in Algeria, a distance of 125 miles.

Spezzia and Corsica cable.

Mr. Brett made two unsuccessful attempts to lay the latter cable. The first attempt was made with a cable containing six conducting wires, and protected with strong iron wire; it weighed 8 or 9 tons per mile. The vessel in which it was stowed was a sailing vessel, towed whilst the cable was being laid by a steamer of small power. More slack was paid out than had been anticipated, and the cable broke during an attempt made to raise it by means of a windlass from deep water.

Bona and Cagliari cable.

A cable with three conducting wires, weighing nearly four tons per mile, placed in a steamer, was next attempted to be laid, but it ran short from an error in the course of the ship. The depth at the place was about 400 fathoms, and the steamer held on to the cable for four or five days, whilst buoys and assistance were being fetched by another steamer which accompanied the expedition. Rough weather, however, came on, and the cable broke before the assistance arrived.

Messrs. Newall made the third attempt, and laid a cable with four conducting wires, which weighed three tons per mile. Of the four wires, one appears to have been defective at first, one to have been faulty, and the other two capable of working. This cable has since failed, and it is stated that three faults were found in it; one was apparently caused by corrosion and another by coral dredgers, the third is in a depth of 1,200 fathoms, at about 40 miles from the island of Sardinia. The outer covering of the cable was so little corroded that it was able to be picked up in this great depth until within about a mile from this fault; the cable then became very much corroded, and it broke in attempting to raise it, before the fault was reached. The tests appeared to show that the cable was actually broken in this depth, but the cause of this fracture could not unfortunately be ascertained. Marine animals and vegetation were found adhering to parts of the cable at the above-mentioned depths. A more careful selection of the route would have enabled the faults which occurred in shallow water to be avoided.

Cagliari, Malta, and Corfu cable.

In 1857 the Mediterranean Extension Company, with a guarantee of interest from the English Government so long as the line remained in working order, laid lines from Cagliari to Malta, and from Malta to Corfu. The line consisted of a strand of copper wires, forming one conductor, covered with gutta percha, and protected by a serving of tarred yarn, covered with iron wires. The cable weighed 18 cwt. per mile. *The line between Cagliari and Malta* remained in good working order for twelve months, when a fault occurred. This fault was repaired, and the line worked again for several weeks, when it again failed in the same locality. An attempt was made to repair this second fault with an extra length of cable, and the cable was raised at some distance from the fault, and the new piece spliced in, but it proved insufficient to reach from the place at which the cable was raised to the fault. The line between Malta and Corfu is laid in depths extending to nearly 2,000 fathoms; it remained in good working order for about a year and three quarters, and then failed suddenly. The exact position of this fault is not known; but it is assumed to be from 20 to 40 miles from Corfu, and is reported as, continuity interrupted, but cable unbroken. The Company have subsequently, in 1859, laid a line from Malta to Sicily, but in comparatively shallow water. This cable contains one stranded copper conductor covered with gutta percha, served with tarred yarn, and protected by iron wires of No. 5½ gauge; it weighs three tons per mile. This line is in good working order.

Dardanelles, Candia, and Athens cable.

Mr. Newall has laid lines between the Dardanelles, Syra, Candia, and Athens, and he has three times failed in attempting to lay a line from Candia to Alexandria, where the greatest depth is 1,750 fathoms. In one instance a hemp-covered cable was used and failed in laying; it was also found that the teredo had eaten through the hemp and indented the gutta percha. Mr. Newall states that the hemp covering did not enable the cable to be raised from deep-water. In the last attempt a portion of the Red Sea cable was used, and after the deepest water had been passed a failure occurred, supposed to arise from a bad joint.

Spain, Majorca, and Minorca;

Submarine telegraph lines have been also recently laid between Spain, Majorca, and Minorca by the Spanish Government, manufactured by Mr. Henley, and the French

An Account of the Principal Telegraph Lines which have been laid.

Deep Sea Cables.

Toulon and Algiers cable.

Government have arranged for a direct line between Toulon and Algiers. This line has been manufactured by Messrs. Glass and Elliot. It consists of a strand of copper wire weighing 400 lbs. per mile, covered with four coats of gutta percha alternated with four coats of Chatterton's compound, also weighing 400 lbs. per mile, served with tarred hemp, and protected by steel wires covered with hemp, to prevent their corrosion. The specific gravity of this cable being small, and its strength considerable, it was able to be picked up for some distance in 1,600 fathoms of water. The first attempt to lay it failed from a fracture due to the occurrence of a storm when the ship from which the line was being paid out was half-way across; the end was consequently taken into Majorca, and communication established between France and Algeria by way of Spain. A subsequent attempt to complete the line failed from a collision between the vessel sent to assist in laying and the vessel containing the cable.

It will be observed that the failures of all these submarine lines are attributable to defined causes, which might have been guarded against. It is possible that as our experience progresses other causes of failure besides those already ascertained may be discovered, but we believe that there are no difficulties to be encountered in laying submarine cables, and maintaining them when laid, which skill and prudence cannot and will not overcome. Before passing to the next section of our report it will be interesting to describe briefly the arrangements which were made for the manufacture of the submarine cable originally intended to be laid from this country to Gibraltar, subsequently proposed for a line between Rangoon and Singapore, and now about to be laid between Malta and Alexandria.

Malta and Alexandria cable.

The manufacture of the core of this cable was contracted *for after the complete* failure of the Atlantic cable and before the experiment of the Red Sea telegraph had been decided. The core consists of a strand of 7 copper wires weighing 400 lbs. per nautical mile, covered with three coatings of gutta percha alternated with three coatings of Chatterton's compound, also weighing 400 lbs. per nautical mile, to be equal in conductivity and insulation to a previously constructed standard mile. This core is served with hemp saturated in tar, and covered with 18 No. 11 iron wires for the deep-water portion, the shore ends being covered with No. 0 iron wires. The line as originally devised was to have been laid for 300 miles in depths of from 1,500 to 2,500 fathoms, and the covering for this portion was to have been of steel wires, each coated with hemp. When the Gibraltar line was abandoned, the steel and hemp covering was given up, and the iron wire covering was adopted for the whole cable. The core was manufactured at the Gutta Percha Company's Works, and was tested in water in one of Reid's pressure tanks, up to a pressure of 600 lbs. per square inch, the air being exhausted from the tank before the water was turned in. This was the highest pressure to which the tank was adapted at the time; by recent arrangements, however, a much higher pressure can be obtained. The resistance and insulation of each mile of the cable were noted, and a careful system of comparative tests *framed, which are to be carried on* during laying, and after the cable has been laid, by which means the actual condition of the cable will be ascertained with great accuracy. These tests are described in Appendix No. 11.

The outer covering was manufactured by Messrs. Glass and Elliot. The contract provided that the cable should be kept under water from the time of its manufacture until laid, and that the electrical tests during covering and laying, and afterwards, should be entirely conducted by electricians appointed by the Government or their engineer, to ensure that the comparative tests deemed indispensable should be properly made. The water tanks constructed at the contractors' works leaked very considerably; and in consequence of some misapprehension the ships to carry the cable were not originally fitted with water-tight tanks to hold the cable. The result was, that the cable, at the contractors' works, was alternately wet and dry, and liable to rust; the corrosion of the large surface of iron in the outer covering, coiled in a compact mass, occupying a small space, rapidly generated heat, which could only be kept down by pumping water over the cable, in the absence of any means of permanent submergence. As long as this water was at a low temperature, so as to prevent the heat in the cable rising to above 50° Fahrenheit, no great mischief could result, as oxydation would not progress rapidly; but when the temperature of the cable increased beyond that amount the heating occurred in an accelerating ratio, reaching a mean of 86° Fahrenheit. This effect of wetting the iron covering of a cable had not previously been experienced, but it has a very important bearing on the selection of the form of outer covering of a cable. If the line had been laid in one length from Falmouth to Gibraltar, it was estimated that about five words per minute would have been passed through it; if,

however, it should be laid in three sections between Malta and Alexandria, the speed at which it can be worked would be materially increased.

II.

The Construction and Laying of Submarine Cables.

Construction and Laying of Submarine Cables.

A submarine cable consists generally of some insulated conductor, strengthened and protected by other surrounding materials to preserve it from injury, as well during the process of submerging it as after it is laid at the bottom of the ocean.

The whole subject of submarine telegraphy may be yet said to be in its infancy, and all that has been done has been rather the result of bold though successful tentative processes than of the application of any well ascertained data to the ends to be obtained. Under these circumstances the success which has attended many of these operations is a proof of the practical skill of the few talented individuals who have devoted their attention to the subject, and their advice and co-operation have been of the greatest value to the Committee in directing attention to their requirements.

The early history of submarine telegraphy resembles in a most striking manner the progress of land telegraphs, in which, as the requirements have arisen, such remarkable progress has been made; and there is no reason to doubt but that a similarly successful result will ultimately attend this new branch of the subject.

The early telegraphs began within the limits of a railway station, and almost insuperable difficulties attended their construction for a distance of even 20 miles. A further extension necessitated an entire change in the whole process. The wires, covered with cotton saturated with a solution of caoutchouc, and laid in metal tubes, were entirely abandoned, and the open air telegraph was adopted; but even with this improvement it was several years before it was found possible, even in fine weather, to work with certainty over a greater distance than 100 miles. Whereas, so perfect has the system of insulation now become, that no difficulty is experienced in communicating directly and instantaneously between London and any part of Great Britain.

It was unreasonable to expect that the progress of submarine telegraphy should be in any degree more rapid, and we ought rather to be surprised at the gigantic strides it has already made, than to be disheartened by the difficulties that have been experienced in the undertakings that have been attempted. It is a remarkable fact, and, as regards the science of the subject probably an unfortunate one, that complete success attended the laying of the first telegraph cables. They became successful precedents to appeal to; further investigation was thought unnecessary, and with no variation as regards the principles of construction, cable after cable was designed and laid down under circumstances and conditions having no resemblance to those originally encountered. The result was, however, far more encouraging than might have been expected. It is indeed, doubtful, whether the transmission of messages for even so short a period through such a cable across the whole width of the Atlantic be not a result worth all the expenditure that has been incurred.

Down to the date of our investigation, about 50 cables had been already laid down, and Appendix No. 18 contains a list of the cables, with the date of laying down, the particulars of the number of wires, the distance traversed, the company or government to whom they belong, their present condition, and in cases of failure, the causes of their failure, and the accidents to which they have been found liable.

In all these cables the same general principles prevail, viz.:—

1. The central conductor is a copper wire or strand of wires.
2. The insulating covering is gutta percha.
3. The external protection, when used, consists of hemp, or other fibrous material, impregnated with pitch or some other resinous substance, nearly in all cases covered with iron or steel wire, in the form of an ordinary twisted rope.
4. The cables so prepared have been paid out over the stern of ordinary vessels with a pressure break, to regulate the delivery according to the speed of the vessel, which has averaged from four to six knots per hour.

It will be convenient to retain these subdivisions in detailing the results that have been arrived at, briefly stating under each head the state of the subject at the commencement of the investigation.

1. *The Conducting Wire.*

1. *Conducting Wire.*

The material used for this purpose has in all cases been copper. Its durability and its high conducting power render it peculiarly applicable to the purpose. It was originally

CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

1. *Conducting Wire.*

used for land telegraphs, but its want of tensile strength, and especially its value to marauders, renders it inapplicable for open air lines; and though it is a far better conductor than iron wire, the extra size necessary when iron is used, is rather an advantage than otherwise, when the exposed situation of such work is considered; the conducting power of pure copper is to that of iron as one to eight, so that a copper wire $\frac{1}{10}$th inch diameter is equivalent as an electrical conductor to an iron wire nearly $\frac{1}{3}$rd inch diameter.

In the first telegraphs the conductor consisted generally of a No. 16 copper wire. This size gave abundant area, and the resistances, even when used in lengths of several miles, were not found to interfere seriously with the working. The conducting power of copper wire was taken to be directly as the area; there were, however, no precise data for determining à priori the size of wire requisite for any given length of circuit and speed of transmission. The wire was joined by being carefully lapped and soldered at the joint, and wrapped with smaller binding wire, which was also soldered with silver solder. The utmost care was necessary in the construction of these joints, and a large number of faults originated in their imperfection, and, indeed, the joint was always much more brittle than the wire itself, and liable to fracture, and a break at any single joint destroyed the value of a whole cable. Moreover, copper could not be procured of homogeneous texture, and not only did hard and soft places occur, but defects or the presence of foreign matter frequently rendered the wire so weak that it ultimately parted after being covered, thus entirely destroying the circuit by the separation of the broken ends in the elastic material with which it was surrounded. In other cases the defect, without causing fracture, reduced the area of the wire at these spots. It was also found that if a wire covered with gutta percha be excessively stretched, the copper wire on contraction was apt to knuckle through the covering of gutta percha, arising from the copper wire being capable of permanent extension, while the elastic covering of gutta percha regained its original dimensions.

Stranded wire.

To remedy these frequent cases of fracture bundles of smaller wires of similar area in the aggregate were adopted instead of a single copper wire. When the conductor is thus formed of a number of wires, the joints in the several wires are distributed; and the fracture or defect of a single wire does not therefore vitiate the whole cable; but it is objected to this arrangement that on the failure of any single wire the sharp fine broken end is liable to start out through the gutta percha, and then come into contact with the outer covering, or the water or damp earth on which the cable is laid, a defect not easily detected, and which can only be guarded against by close examination of the strand itself, and by the constant testing of the gutta-percha covering of the wire during its manufacture. It is also evident that in the form of a strand the bulk of the conductor is greater than with a single wire, and more gutta percha is required to obtain the same thickness of covering.

Want of solidity is urged as an objection to the strand form of conductor, *i. e.*, *it* is alleged that if water penetrates in any place to the wire it will pass along the wire as in a tube; the Gutta Percha Company propose to remove this objection by coating the central wire of the strand with Chatterton's compound, and bedding the six outer wires in it in the process of twisting. The compound, which passes out between the interstices of the wires, becomes firmly united to the first coating of insulating material, and the whole is so solid that a few inches of this strand will prevent the percolation of water at a pressure of 600 lbs. per square inch. Mr. Daft proposes to obtain the same object by bedding copper wires coated with brass in vulcanized india rubber. Mr. Clark has proposed to obtain this solidity by making the conductor in the shape of a solid wire, divided into three or four sections longitudinally, fitting closely to each other. Mr. Newall unites the several wires of a strand with solder.

To prevent the injury to one wire of a strand conductor from damaging the whole conducting capacity of the cable, Mr. Varley proposes to insulate separately what would otherwise be the several wires of a strand conductor, and to unite them at intervals and use them as one conductor. For instance, assume that the cable's conductor has three conducting wires, each surrounded with gutta percha; these three would be connected together at frequent intervals, as follows:—First, Nos. 1 and 2 would be connected together by soldering or otherwise; a little further on Nos. 2 and 3 would be connected, and further on still Nos. 1 and 3 are so joined, and so on, the different metallic junctions breaking joint, and no two of them being in the same spot. Excepting at these junctions, the separate conducting wires would be isolated from each other. Now, if the insulating covering were damaged, water would enter, and the wire become defective, strong positive currents could be applied to the

cable, which would eat away the conductor till its exposed ends retire inside the insulating envelope, these parts would then offer so much resistance that the line would be workable again. Thus a cable may be injured in several different places and yet be made available.

Construction and Laying of Submarine Cables.

1. *Conducting Wire.*

In the first telegraph wire that was made, the strength and toughness of the gutta percha was increased by combining it with sulphur, and this was thought to increase also its insulating properties; but the sulphur was found to act seriously on the copper, a sulphuret being formed at the expense of the copper. In the use of this wire it was discovered that in cases where the wire had parted in the interior of the sulphurized gutta percha, and the two ends had become slightly separated, the resistance offered by the lining of sulphuret of copper, produced on the passage of a galvanic current sufficient increase of temperature to ignite the gutta percha, and actually burst out into flame. So certain was this action, that charges of gunpowder were specially prepared by this means for submarine explosions and other such purposes; and this method possesses very important advantages from the smaller area of conducting wire that is necessary in comparison with the area requisite for the distant ignition of platinum for such purposes.

Quality of copper.

Professor Thomson and other experimenters have shown that the quality of the copper exercises an important influence on the conducting power of copper wire; but this question had not been fully developed when we commenced our inquiries; we consequently committed to Dr. Matthiessen the task of elucidating this question further.

We may observe, in the first place, that if the conducting power of silver be assumed at 100; that of copper would be 90: aluminium 34; and iron 13.

Dr. Matthiessen's valuable report will be found in Appendix No. 3. The following is a summary of the results arrived at:—

Table showing the Effect of Admixture of Copper with Specific Quantities of various Substances.

Effect of admixture of foreign substances with copper.

Substances alloyed with pure Copper.	Conducting Power of Alloy, pure Copper being 100.	Temperature Centigrade.
Carbon:		
Copper, with ·05 per cent. of carbon	77·87	18·3
Sulphur:		
Copper, with 0·18 per cent. of sulphur	92·08	19·4
Phosphorus:		
Copper, with ·13 per cent. of phosphorus	70·34	20·
Do. ·95 do.	24·16	22·1
Do. 2·5 do.	7·52	17·5
Arsenic:		
Copper, with traces of arsenic	60·08	19·7
Do. 2·8 per cent. of arsenic	13·66	19·3
Do. 5·4 do.	6·42	16·8
Zinc:		
Copper, with traces of zinc	88·41	19·0
Do. 1·6 per cent. of zinc	79·37	16·8
Do. 3·2 do.	59·23	10·3
Iron:		
Copper, with ·48 per cent. of iron	35·92	11·2
Do. 1·06 do.	28·01	13·1
Tin:		
Copper, with 1·33 per cent. of tin	50·44	16·8
Do. 2·52 do.	33·93	17·1
Do. 4·9 do.	20·24	14·4
Silver:		
Copper, with 1·22 per cent. of silver	90·34	20·7
Do. 2·45 do.	82·52	19·7
Gold:		
Copper, with 3·5 per cent. of gold	67·94	18·1
Aluminium:		
Copper, with 10 per cent. of aluminium	12·68	14·0

The addition of a small quantity of lead or of tin (0·1 per cent.) to copper containing suboxide obtains a purer metal, and, consequently, improves its conducting power.

The following table shows the conducting power of certain commercial coppers.

1. *Conducting Wire.*

Quality of copper.

Quality of Copper.	Conducting Power.	Temperature Centigrade.	Cause of Diminution of Conducting Power.
Pure copper - -	100 mean	15°·5	
Specimen furnished by Mr. Tenant, cut from a piece 1½ tons in weight -	98·78	15·5	Traces of silver. *No suboxide of copper.*
American (Lake Superior)	92·57	15	Traces of iron, silver (·03 per cent.) and suboxide of copper.
Australian (Burra Burra) -	88·86	14	Traces of iron and suboxide of copper.
Best selected - -	81·35	14·2	Traces of iron, nickel, antimony, suboxide of copper, &c.
Bright copper wire -	72·22	15·7	Traces of lead, iron, nickel, suboxide of copper, &c.
Tough copper - -	71·03	17·3	Traces of lead, iron, nickel, antimony, suboxide of copper, &c.
Russian (Demidorff) -	59·34	12·7	Traces of arsenic, iron, nickel, suboxide of copper, &c. The arsenic present may be considered the chief reason of the low conducting power.
Spanish (Rio Tinto) -	14·24	14·8	2 per cent. arsenic; traces of lead, iron, nickel, suboxide of copper, &c. The low conducting power is to be attributed to the arsenic present.
Gibraltar Core :			
Specimen No. 112 -	90·7	15·5	Traces of lead, suboxide of copper, iron and antimony.
„ „ 91 -	89·5	15·5	
„ „ 292 -	78·2	15·5	Traces of lead, arsenic (very small) iron, nickel,
„ „ 240 -	74·4	15·5	antimony, and suboxide of copper.

It appears from this table that Rio Tinto copper possesses no better conducting power than iron.

These differences of conducting power are caused by the impurities present in the specimens; the sub-oxide of copper appears to be most injurious, the conducting power of the copper being diminished by it in one case as much as 28 per cent., but there is no known accurate method of determining quantitatively the sub-oxide, and consequently the actual amount present in the specimen in question was not ascertained.

It is scarcely possible to obtain perfectly pure copper, and there is no substance which, added to pure copper, increases its conducting power; but it is of the utmost importance that the purest and best conducting copper should be used in submarine cables. The best way of ensuring this is by contracting for wire of a specified resistance per knot, because then what the copper wants in quality must be made up in quantity. It is, however, preferable to employ pure copper, because impure copper of such a diameter as to have the same conducting capacity, would give rise to greater induction.

Standard of resistance.

The specific resistance of copper and other metals to the galvanic current varies with the temperature of the conductor. In the contract for a telegraph cable, a wire affording a standard of resistance at a specified temperature should therefore be furnished. The resistance at different temperatures will vary with the quality of the metal; and copper is not a good metal for such a standard, because it oxydises easily and its conductivity varies so rapidly with temperature. The standard should be such that—

I. Its resistance will remain the same, whether it be made of absolutely pure or commercially pure metals; in other words, that such an alloy may be made by any chemist or assayer, and its conducting power will always be the same.

II. That its conducting power will not be altered by the process of annealing.

III. That its conducting power will not vary much with an increase or decrease of temperature.

IV. That the alloy will not alter by exposure to the atmosphere.

Mr. Varley uses for a standard iron wire boiled in oil and enclosed in a soft cement. Messrs. Siemens employ german silver for the purpose, which they compare with their mercury standard at zero centigrade. Dr. Matthiessen, whose experiments are published in the Philosophical Magazine for February 1861, considers that the alloy best adapted for a standard of resistance for this purpose is composed of

2 parts by weight of gold,
1 „ „ silver.

And he states that the variation in the conducting power of various specimens of this alloy, made by different persons and in different places, was very small.

With respect to the effect of temperature on this alloy and on copper and other metals generally, Dr. Matthiessen observes, that when a hard-drawn wire is heated to 100° a different conducting power is generally found on cooling; and to obtain concordant results it is necessary to heat the wire several times; but when once obtained, the values

found will remain the same, no matter how often the wire may be heated. Whether by letting the wire remain for a length of time it will gradually assume its original conducting power or not, is a question not yet decided. With annealed wires the same effect takes place, but in a much less degree.

CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

1. *Conducting Wire.*

The following table shows the differences in the conducting powers of some metals as compared with that of the alloy, between 0° and 100°, taking the conducting power at 0° = 100°.

Standard of resistance.

Silver	28·5 per cent. (annealed).
Copper	29·0 per cent. (annealed).
Gold	28·0 per cent. (annealed).
Mercury	8·7 per cent. (Siemens).
The gold-silver alloy	6·5 per cent. (hard drawn).
" "	6·7 per cent. (annealed).
German silver	3·8 per cent. (Siemens).

It would thus appear that, so far as variation of temperature is concerned, the alloys are better adapted for a standard of resistance than the other metals mentioned.* Moreover, they do not suffer much from exposure to the atmosphere.

With regard to the expense of the gold-silver alloy, the nine grammes alloy cost, drawn into wire, about 1*l.* 4*s.*, but the gold in it is always worth about 15*s.*; so that the real expense is very small. Care must of course be taken to prevent contact with mercury. The best way to prevent any such accidents is to varnish the wires. In having these alloys made it would be advisable always to have two specimens made by different parties, so as to avoid chances of error.†

It will be seen that the experiments above mentioned have cleared the question of the conducting power of copper wire, whether as regards its purity or its area, of all uncertainty, and that the size of a conducting wire necessary to fulfil any given conditions can in future be determined with absolute precision.

In these experiments new methods of testing and new instruments have been employed, which are fully described, and which cannot fail to prove of the highest scientific interest to the theoretical philosopher and to the practical electrician.

2. *The Insulating Covering.*

2. *Insulating Covering.*

The first land telegraphs that were constructed were in every respect analogous to submarine cables, and the wires were indeed frequently laid across rivers and canals beneath the water.

In the earlier experiments, which were confined to buildings or sheltered positions, the copper wires were insulated by a covering of cotton or silk laid spirally over the wire; the cotton was subsequently steeped in some resinous substance to protect it from damp, and ultimately the wires so prepared were immersed in a body of pitch or resin, and laid in wooden troughs or iron pipes. As many as 10 wires were thus laid along the Blackwall Railway in a wrought-iron gas pipe, two inches in diameter. In such pipes the wires were carried under ground or water.

It was, however, found almost impossible to maintain sufficient insulation by such means. In some places water penetrated through the pipe, in others the sun damaged the resin, and generally the resinous substance used, gradually decomposed or absorbed moisture. There are, however, still working in the Primrose Hill tunnel nine wires insulated by means of cotton and pitchy compounds enclosed in a lead tube, which have been down for 14 years. Moreover, although the utmost care was devoted to the subject, it was found impracticable to lay down any great length without serious faults occurring in the construction, and the system was hopelessly abandoned and entirely superseded by the open air wires. Covered wires, however, were indispensable for leading into stations, under bridges, and especially through tunnels, where the constant humidity prevented the use of open wires, consequently the greatest difficulty that has had to be overcome with the overland

* Dr. Matthiessen states that he has found, whilst making these experiments, that as soon as most of the pure metals are alloyed with traces of any other metal, the differences from temperature rapidly decrease, in fact almost in the same proportion as the conducting power of the metals themselves. Dr. Matthiessen has tested a commercial copper wire, whose conducting power only varied seven per cent. between 0° and 100°, whilst pure copper varies 29 per cent. Now suppose a wire of that copper, whose conducting power only varies seven per cent. between 0° and 100°, be compared with a standard at a certain temperature, and then with a pure copper wire at another temperature, say 20° difference, it is obvious that the pure copper wire will not have the same resistance as the original standard. It has as yet been generally assumed that the conducting power of all copper wire, whether pure or commercial, varies with an increase of temperature to the same degree, which, however, is far from true, and should be borne in mind when constructing a resistance thermometer, as proposed by Messrs. Siemens. (Phil. Mag., January 1861.)

† Dr. Matthiessen proposes that all those who study the electrical resistance of metals should compare one of their metals with this alloy, calling its conducting power 100 at 0° (hard drawn wire of one millim. length and one millim. diameter,) for then we should be able to compare the results obtained by different experimenters with one another.

CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

2. *Insulating Covering.*

lines has been the passage through long tunnels, and to this difficulty submarine telegraphy is deeply indebted for the progress it has made. It was in overcoming this obstacle that india-rubber was first successfully employed, and it is remarkable, that the first really efficient insulating substance that was used should, after falling entirely into disuse, be now again brought forward.

As in the case of copper for the conductor, so india-rubber or caoutchouc appeared almost specially intended for the purpose of insulation. This substance possesses insulating qualities of the highest order. It is tough, highly elastic, of less specific gravity than water, easily manipulated, extremely durable under water, nearly impervious to moisture, except superficially, and not excessively costly, and it appeared on its first introduction as though nothing further could be desired. One of the first and most important requirements in any insulating substance that may be adopted is that it should offer facilities for making the numerous joints required, either for the repair of lines when laid down or in their first construction. For this purpose india-rubber appeared also well adapted. If, *instantly* after being cut, two surfaces of this material are again brought into contact, they unite almost as perfectly as though no separation had taken place; moreover, being soluble in naphtha with a slight increase of temperature, two surfaces may be hermetically united by warming them and slightly moistening them with this material. The copper wire, previously covered with cotton and shellac, was covered by a thin strip of masticated india-rubber wound spirally on the wire, each turn overlapping the last, and several coatings were thus put on the wire, the union of the india-rubber being secured by slightly moistening it with naphtha. A degree of insulation infinitely superior to anything that had been before obtained was the first result, and the problem on which so much time and money had been expended seemed to be definitely and perfectly solved, and the new material came into rapid use. It required, however, but a very short experience to dispel all the hopes that had been raised. This gum resin, like all others of a similar character, slowly burns or oxydises even in contact with the air only, and in the dark; but in the light, and especially where exposed openly to the weather and the sunlight, the oxydation goes on with fatal rapidity; the wires hung out of doors soon became useless; the oxydised gum assumes a thick gummy character, and soon falls away from the wire. The union made with naphtha was found not to be durable; and after a short time, even in unexposed situations, the rubber was found loose upon the wire. Attempts were made to preserve it by enclosing it in grooved boards, and thus protecting it from the air, but this in dry situations was found to be of but little avail; and though in wet tunnels it was found to be more durable, yet here also it was ultimately abandoned. In other cases, the india-rubber assumed a liquid form from decomposition, and simply left the wire by flowing away from it.

To avoid these difficulties, gutta percha was proposed. Gutta percha is, when perfectly pure and at moderate temperatures, a remarkably good insulator, and capable of being kneaded and drawn solidly on the wire through dies, which avoids the joint which is necessary with india-rubber laid on in strips.

Some difficulties, however, rendered its first introduction not so successful as that of india-rubber. In the first attempts, the covering, which was single only, was not efficiently put on, and contained air holes and other defects. This was partly due to imperfections in the manufacture, but more particularly to the bad quality of the gum itself, which comes to this country mixed with a very large proportion of impurities, so large, indeed, that but a very small per-centage of the gum imported is fit for the manufacture of covered wire at all. In addition to the impurities already existing, fine sawdust and ground wood were mixed with the gum. It was also combined with sulphur, with the bad result already detailed under our last head. It was also found liable to oxydation when exposed, though this appeared to be avoided by covering it with Stockholm tar and immersing it in water, or otherwise protecting it from the air; it is not liable to assume the liquid form, but the making of an efficient joint offered several difficulties, as it was absolutely necessary for this purpose to remelt the material. These joints gave rise originally to a great number of faults, which, together with the general impurity of the gum itself and the imperfect manufacture, were the principal drawbacks to the more rapid introduction of gutta percha. Gutta percha, even moderately pure, however, has many advantages, and fell into hands far too enterprising and persevering to be rapidly abandoned. The manufacturers devoted themselves to the perfection of their machinery, sorted the gum with the most elaborate care, and soon produced a tolerably pure material. Improved methods of making the joints were introduced, fine ribbons of thin gutta percha being used instead of kneading over the joint with a mass of material; and above all, the system was introduced of

covering the copper wire with several independent layers of thin material, instead of one thick layer; the occurrence of air holes or defects from minute fibres left in the material were thus to a great extent obviated. The greatest care was, moreover, bestowed at the manufactory itself on the testing of the wire during the process of manufacture. Coincident with these improvements in manufacture, the Telegraph Companies endeavoured to protect the material from oxydation by covering the wires with tape and Stockholm tar, and enclosing them in wooden grooves, or covering them with yarn and tar, and laying them in underground pipes, the material being thus found to be tolerably durable, especially when immersed in water. In all cases, however, where these precautions were not taken, the wires rapidly became useless, and many hundreds of miles have thus been abandoned, while, on the other hand, with these precautions many lines which have been some years in use are still in good working order. It is, however, only since the commencement of our inquiry that the perfectly pure material has been produced in sufficient quantity to be practically useful; and that the high insulating properties of the gum in its pure state have been appreciated.

Construction and Laying of Submarine Cables.

2. *Insulating Covering.*

Professor Hofmann appears to have been the first to point out the cause of the decay of gutta percha and india-rubber; and at the commencement of our inquiry we requested Professor Miller to undertake the chemical investigation of this question. His report will be found in Appendix, No. 4.

Cause of decay of Gutta-percha and India-rubber.

It appears that pure gutta percha is a hydro-carbon, containing of—

Carbon - - -	88·96
Hydrogen - - -	11·04
	100·00

The gutta percha of commerce is mixed with resin, vegetable fibre, moisture, &c.; the moisture is mechanically diffused through the mass, influencing its pliability and toughness. Commercial gutta percha will remain unchanged for months in air, provided light be excluded, and provided the temperature be not raised very high, and it will remain unaltered for years in water, especially when coated with Stockholm tar, and when light is excluded. Alternate exposure to moisture and dryness, on the other hand, particularly if the sun's light has access to it, is rapidly destructive of gutta percha. All the deteriorated portions of gutta percha examined by Professor Miller were found to have undergone change from oxidation. The evidence we have taken shows that it has occasionally been injured in the sea by animals.

Caoutchouc consists of a hydro-carbon of definite composition, mixed with a small quantity of resin. It is liable to deterioration by exposure to the action of oxygen in the presence of solar light, but the gum is less rapidly injured if exposed to these influences in the native state than if it has been previously masticated. When subjected to the action of air excluded from light it does not experience any marked change even during very long periods. Further particulars will be found in Professor Miller's report.

We have made numerous experiments upon the effect of temperature and hydrostatic pressure on both gutta percha and caoutchouc. Such experiments occupy a very considerable time, and are otherwise difficult to perform. The general results appear to be that temperature has a very marked effect on gutta percha, but that pressure appears to consolidate the material and improve the insulation of both gutta percha and india-rubber.

The effect of temperature on the insulation of several insulating substances which we experimented on is shown in the tables of experiments in Appendix No. 6.

Effect of temperature on insulating substances.

The results may be briefly stated as follows:—With gutta percha in ordinary use for submarine cables, the insulation at 72° was not one-half as good, and at 92° not one-fourth as good as it was at 52°; and at 52° it was not one-third as good as at 32°. Perfectly pure gutta percha was a far superior insulator, and suffered little loss of insulation until it attained a temperature of between 72° and 92°. India-rubber and Wray's compound, which are very far superior as insulators to the gutta percha which has been ordinarily in use, exhibit very little loss of insulating power until they attain temperatures far above 92°.

The experiments in a very high temperature showed that whilst india-rubber withstood a temperature of 200° and Wray's compound a temperature of 152°, gutta percha covered wire was entirely spoiled at a temperature a little over 122°. At a temperature of from 90° to 100° Fahrenheit gutta percha will not change its shape; but at higher temperatures a conducting wire covered uniformly with gutta percha will easily become excentric by the mere process of coiling. Gutta percha covered wire should in no case be exposed to heat, the exact amount of which cannot be defined and regulated. This material is therefore not a desirable one for cables which have to be conveyed

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2. *Insulating Covering.*

Absorption of water by insulating substances.

through or laid in the tropics, unless means be found for ensuring that the cable be maintained at a low temperature.

When immersed gutta percha, india-rubber, Wray's compound, and Chatterton's compound absorb a portion of water. Professor Miller's experiments, in which gutta percha and india-rubber, were subjected to pressure of three tons per square inch for a period of six weeks, show that the absorption of water by gutta percha is very slight, almost *nil*, in sea water. In fresh water it is appreciable, but trifling. The absorption of water by caoutchouc is always sensible, the surface being rendered white and opaque, owing to the amount of water that has penetrated into the substance. The absorption, however, only reaches to a small depth, and does not destroy or in any way impair the insulating power of that portion of the caoutchouc which is beneath the moistened layer. The white aspect disappears as the substance is allowed to dry. The amount of absorption is dependent upon the extent of surface exposed to the water. The insulation of the specimens of gutta percha and of masticated india-rubber, experimented upon by Professor Miller, was in no way impaired by immersion under pressure, but the results with virgin india-rubber were not equally satisfactory. Messrs. Siemens' experiments on the immersion in water of gutta percha, india-rubber, and Wray's compound, give the following results:—

The materials examined absorb water more rapidly from pure water than from sea water, and more rapidly from sea water than from concentrated brine. In gutta percha this difference is very decided, the absorption being about three times as great in sea water, and five times as great in pure water as in brine. Caoutchouc absorbs water somewhat less rapidly than gutta percha at 39° Fahr., which may be taken for the temperature of the deep sea. Wray's mixture absorbs at the same rate from sea water as from pure water, that rate being nearly equal to that of gutta percha, at the lower temperature of 39° Fahr.

Increase of temperature affects the rate of absorption by gutta percha moderately with sea water, that rate being little more than doubled for an increase from 39° to 120° Fahr. In pure water the absorption at an increased temperature is greater. Caoutchouc absorbs from brine at about the same rate at 39° and at 120° Fahr. From sea water it absorbs at double the rate at 120°, and in pure water the rate of absorption is as much as eight times greater at 120° than at 39° Fahr. With Wray's mixture, the rate of absorption from sea water is not materially influenced by temperature; but in pure water the rate of absorption increases in a very extraordinary ratio with increase of temperature, being about 16 times greater at 120° than at 39° Fahr. Both caoutchouc and Wray's mixture change colour and lose firmness when exposed for many days to pure water of 120° Fahr.

The thickness of the sheets of material employed affects the absorption in a much higher ratio than can be accounted for by the simple increase of surface. The absorption of water by the thicker sheets appears to stop short at a limit which is rapidly exceeded by the thin sheets. The conductivity of these materials appears not to be materially affected by the absorption of two to three per cent. of water, which may be taken for a limit that will not be exceeded in sea water and at ordinary temperatures.

Increase of pressure within the limits of 50 lbs. per square inch does not affect the rate of absorption materially. The rate is somewhat higher under a vacuum, owing probably to the absence of condensed air upon the surface.

Effect of pressure on insulating substances.

Mr. Fairbairn (Appendix No. 5) made some experiments at Manchester under a pressure of 20,000 lbs. per square inch, equivalent to a depth of 8·72 miles. At that pressure a sheet of gutta percha $\frac{1}{16}$ inch thick absorbed ·05 per cent. of its weight of pure water, after an exposure of 65 hours. A further exposure of 23 hours did not produce any further absorption. A pressure of 5,900 lbs. per square inch, continued for a period of 450 hours, on a sheet of gutta percha $\frac{1}{64}$ inch thick, produced an absorption of ·2 per cent. of pure water; whilst with a piece of gutta percha 1 inch thick the absorption in a similar period was only ·08 per cent. Mr. Fairbairn's experiments also included trials of the absorption of india-rubber, Chatterton's compound, Wray's mixture, and other substances. As regards the effect of temperature the absorption with a pressure of 20,000 lbs. per square inch was for gutta percha six times as much at a temperature of 75° as at a temperature of 45°, for india-rubber two and a half times as much, and for Wray's compound seven times as much.

Mr. Fairbairn found that in pressures of 20,000 lbs. per square inch, the insulation was improved by consolidation, and that the bulk of the material was reduced.

We submitted wires (Appendix No. 6) covered with gutta percha, india-rubber, and Wray's compound, 110 yards in length, to pressure of three tons to the square inch for periods of a month. The wires were placed in iron pipes connected with an hydraulic press, by means of which the pressure was continually maintained. We had intended to obtain higher pressure, but we experienced so many failures in the attempt that we were

at last obliged to give up the endeavour. An examination of the tables will show that, except where specific faults occur, either from joints or from the pressure of the wire against the connexions in the pipes, the condition of the wire after the pressure was put on remained strictly uniform, the loss of insulation following very closely the temperature of the water in which the specimen wire was immersed. CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

The general conclusion at which we have arrived, from a careful consideration of these and other experiments, is, that the absorption of water by these substances is not to be feared as a cause of deterioration; and that pressure improves the insulation, whilst it is being applied, its effect being more obvious as the material is a worse insulator; and there appears to be less difference in india-rubber when subjected and not subjected to pressure than in any other of the materials experimented on. 2. *Insulating Covering.*

When the first submarine telegraphs were laid the size of the conducting wire or the thickness of the insulating material had not become prominent matters of inquiry. Some retardation of the electric currents in underground work had been observed; but in the first short submarine lines but little practical inconvenience resulted, and it was not until lines of considerable length had been laid down, that the subject of induction excited much attention.

It was early seen that the inductive action must be in some ratio proportionate to the thickness of the insulating medium, but the actual value of this ratio was unknown; the thickness of the gutta percha was accordingly increased with the feeling that some increase was necessary, but no real grounds existed for determining what that increase should be, nor were any clear notions entertained of the precise action of induction in causing the retardation observed. In 1854 Mr. L. Clark brought under the notice of Professor Faraday certain experimental results he had obtained in telegraph lines between London and Manchester, and Professor Faraday explained before the Royal Institution the general principles which govern these effects. Induction.

When a metallic wire is enveloped by a coating of some insulating substance, as gutta-percha or india-rubber, and is then surrounded by water or damp earth, the system becomes exactly analogous to a Leyden jar or coated pane; the insulating covering represents the glass, the copper wire the inner metallic coating, and the water or moist earth the external coating. The electricity with which the wire is charged, by bringing the pole of an active battery in contact with it, acts by induction on the opposite electricity of the surrounding medium, which in its turn reacts on the electricity of the wire, drawing more from the source, and a considerable accumulation is thereby occasioned, which is greater in proportion to the thinness of the insulating covering.

One mile of copper wire, one-sixteenth of an inch in diameter, presents a surface of 85·95 square feet, and the inductive circumstances being assumed to be the same, receives the same charge from a source of the same tension as a Leyden jar having an equal capacity. There is, however, one material difference between the two cases. Though both are discharged in a time inappreciably minute to the senses, the discharge from the wire occupies a comparatively much longer interval than that from the coatings of the jar. A wire on insulating supports, in the open air, when it is unconnected with the earth, receives also a charge, but very much smaller in amount, the inductive action of distant surrounding bodies exerting but little influence upon it.

Although certain general conditions of this inductive action in telegraphic wires had been made the subject of experiment, and were well known, when we commenced our inquiry, the knowledge of the subject still remained limited. It was therefore thought desirable that a series of experiments should be instituted, under circumstances as closely resembling those which take place in actual telegraphic lines as possible, in order to determine the amount of inductive discharge in wires of very considerable lengths, according to variations in the lengths of the wires, their diameters, the material and thickness of the insulating substance, and in the temperature and pressure of the medium by which they are surrounded (Appendix No. 1).

The results of these experiments may be briefly summed up as follows:—

Experiments on induction and insulation.

1. The amount of discharge of a battery is proportionate to the electro-motive force or tension of the battery.

2. The discharge is directly as the length of the wire.

3. When a discharge is effected simultaneously from a number of wires united at their discharging ends, the effect on a galvanometer is the same as when the wires are all united together to form one continuous length.

4. The conductivity of the metal, other circumstances being the same, does not influence the amount of discharge. Hence by employing a bad conductor the resistance of the circuit is increased, the induction remaining the same.

5. The theoretical formula given by Professor Thomson in his evidence for the electrostatic capacity of an insulated wire is as follows, viz.: If D denotes the diameter of a

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copper wire of circular section, symmetrically covered with insulating material to a diameter D^1, and I denotes the specific inductive capacity of the insulator, and log. denotes the Napierian logarithm of the number to which it is prefixed, the expression for the electro-static capacity of an unit length of conductor thus insulated, when the outside of the insulator is kept in communication with the earth, is $\frac{I}{2 \log. \frac{D^1}{D}}$, and this is assumed by many experienced experimentalists correctly to represent the phenomena. It appears from the experiments above mentioned that the amount of discharge from wires of different diameters with coverings of various thicknesses of the same insulating material may be assumed to be for practical purposes, directly as the square root of the semi-diameter of the wire and inversely as the square root of the thickness of the insulating envelope.

2. *Insulating Covering.*

Experiments on induction and insulation.

Hence by increasing the diameter of the wire and the thickness of insulating covering, in the same proportion, the amount of inductive discharge remains the same. The force of the current in a voltaic circuit increases as the square of the diameter of the wire (the length of circuit being constant and the resistance of the battery being inconsiderable as compared with that of the metallic portion of the circuit), consequently if it be found inconvenient to increase the wire and the insulating covering proportionately, greater advantage will be obtained by increasing the diameter of the wire than the thickness of insulating covering, for whilst the covering remains the same the induction discharge increases only as the square root of the diameter of the wire, whilst the force of the current increases as the square of the diameter; and if the insulating covering be varied the conducting wire being constant, the strength of the current will remain the same, but the induction will only decrease as the square root of the thickness.

6. *a.* That india-rubber surpasses all other materials in the smallness of the amount of its inductive discharge and the perfectness of its insulation. In the former respect a coating of india-rubber is fully equal to a coating of ordinary gutta percha of double its thickness.

b. That Wray's compound, an artificial composition formed by the addition of other highly insulating materials to india-rubber, and the newly manufactured pure gutta percha, closely resemble india-rubber in both particulars.

c. That the mixture of imperfectly conducting materials with gutta percha, as carbon in Mr. Radcliffe's composition, or pounded cocoa-nut shell as in Mr. Godefroy's, has the disadvantage of greatly reducing the insulation and increasing the induction.

d. That the interposition of cotton thread between the wire and an india-rubber coating, as in Hall and Wells' preparation, also considerably increases the induction and diminishes the insulation. The induction is augmented because the cotton thread, which is a bad insulator, increases the surface of the conductor; and the insulation is impaired, not only because the insulating coating is diminished, by the thickness of the cotton, but probably also in consequence of the greater inductive action. *The interposition* of cotton between two layers of gutta percha *is equally disadvantageous, as is* proved by the experiments on Mr. Hearder's short line of 450 yards.

e. That the interposition of a viscid insulator between two coatings of gutta percha neither decreases the induction nor improves the insulation of the line. If Mr. Hughes' process should possess any advantage, it will be found in the tendency of the viscid fluid to fill up air holes or flaws in the gutta percha coatings.

f. Generally speaking the more perfect the insulating property of the material is, the less is its inductive capacity. There are, however, several apparent exceptions to this rule among the experiments made, but there are so many causes to affect the experiments on insulation, that we are not warranted to infer, from these exceptions, the entire independence of the two properties.

7. Temperature affects the discharge only in so far as it affects the insulation.

8. It does not appear that pressure exerts any influence on the amount of induction discharge.

Several other interesting results will be found in Appendix No. 1.

The velocity with which electricity travels through a metallic wire is exceedingly great; indeed, for practical purposes, instantaneous, when there is no induction. The well-known experiments of Professor Wheatstone, which were made with electricity of high tension, proved that this velocity is not inferior to that with which light travels through the planetary space; and the most reliable experiments which have since been made with electricity of comparatively low tension, which is more affected by adventitious circumstances, still give a very high speed. Messrs. Fizeau and Gounelle ascertained the velocity of electricity generated by a voltaic battery to be in copper wire 111,834 miles per second, and in iron wire 62,130 miles per second; and the more recent

2. Insulating Covering.

Experiments on induction and insulation.

experiments of Messrs. Guillemin and Burnouf give the velocity in iron wire to be 111,847 miles per second. Messrs. Fizeau and Gounelle inferred from their experiments that the velocity of electricity is independent of the strength of the current and the section of the conductor, though it varies with the material.

Other experiments have been made, from which a much lower velocity has been concluded; but these have generally been on subterranean lines, in which the retardation has been proved to be owing to the inductive action of the surrounding medium, from which they are separated by an insulating covering. In these cases it is not the velocity of the electricity in the metal which is measured, but the time which is required for a certain amount of effect to be produced at the remote end of the wire, or, in other words, the time necessary to charge the wire to a certain amount. Most electricians who have considered this matter in a theoretical point of view concur in stating that this time is proportional to the square of the length; many attempts have been made to prove this law, but satisfactory experiments, free from error or ambiguity, are still wanting.

Dr. Faraday, from some experiments made by Mr. Clark, gives two seconds for the time which the current requires to appear at the remote end of a subterranean telegraphic wire 1,500 miles in length, whilst in the same length of air wire the time was almost inappreciable. Mr. L. Clark states the time at which the current appeared at the distant end of a subterranean wire 788 miles in length was three-eighths of a second, whatever was the battery power employed; and, with regard to the Atlantic cable (before it was submerged), Mr. Walker states the interval between the electric force being communicated at one end and arriving at the other to be about two seconds. It is stated that Professor Thomson's delicate reflecting galvanometer showed a sensible current in less than one second.

If a measured charge, after its transmission through the wire, were discharged at the remote end in an equal interval of time, this retardation would be of little importance, as the charges at one end and the discharges at the other would succeed each other in the same order and with the same intervals between them; but as the discharge in great lengths of wire occupies a much greater time than the charge, if a new charge is given before the wire is completely cleared of the former, although the charges may be separated by strongly marked intervals, the discharges would be confounded into one, and it would be impossible to distinguish the various succession of currents, which represent the alphabetic characters, from each other. There is thus a limit to the number of signals which may be made in a given time, dependent on the inductive action brought into play on the line.

Various expedients have been resorted to, to increase the rapidity with which currents may be made to succeed each other; the one most generally employed is to discharge the near end of the line immediately after it is disconnected from the battery, before another current is sent into it. Mr. Varley and Messrs. Siemens, instead of discharging the return current into the earth, neutralize it by bringing the wire into contact with the pole of a weak battery, which produces a weak current in the opposite direction. Professor Jacobi employs a secondary battery, which, becoming polarized or charged by the transmitted current, occasions a current in the opposite direction, which acts during the cessation of the primary battery current and neutralizes the residual electricity in the wire.

Professor Hughes has made a number of experiments on submarine cables (before they were immersed) to ascertain the number of alternating currents he could pass through them in a given time, so that the signals should not be confused at the opposite end; he calls the succession of two alternating currents a wave. The results he obtained are stated in his evidence.

With respect to the number of words per minute transmitted through various submarine telegraphs in their submerged and working state, we collect the following particulars from the evidence:—

		Words per minute.	
Varna and Balaklava	310 nauts	5	A. Varley.
Varna and Constantinople	150 nauts	15	Ditto.
Red Sea	480 nauts	12	Forde.
Ditto	730 „	5 or 6	Ditto.
Atlantic	2,500 miles	2½	Thomson, maximum.
Ditto	„	2	Ditto, verified by himself.
Ditto	„	1·1	C. F. Varley, by relay.

It seems to be generally admitted by experimentalists that the rapidity of succession of the signals is not affected by varying the number of elements of the battery.

CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

2. *Insulating Covering.*

Experiments on induction and insulation.

Mr. Varley. To increase speed.

The most important problem to solve in submarine telegraphy undoubtedly is, how the signals may be made to follow each other with the greatest rapidity; and this resolves itself into two questions, how far can the circumstances upon which inductive action depends be removed, and what are the best means to lead away or destroy the residual charge of the wire after the original current has ceased to act. In the course of this inquiry we have obtained some valuable information with regard to the former, but much remains to be done before the latter question can be fully answered. We would, however, draw attention to the remark made by Mr. Varley (Quest. 2909), that in the practical working of an underground or submarine line, the available speed for communication can only be taken at one-third of the attainable speed.

One of the most fatal things that can happen to a submarine cable is the occurrence of faults by which a conducting communication is established between the internal wire and the surrounding water; the smallest and almost imperceptible fissure is sufficient to effect this, and the utmost care should therefore be taken in the manufacture to avoid them. When gutta percha is the insulating substance, they arise most frequently from air-holes produced during the preparation of the material, and occasionally from badly centering the wire, by which it is brought into close proximity to the external surface of the covering, so that a slight abrasion may occasion it to protrude. Both these defects may be in a great measure avoided by covering the wire with successive coatings. The wire, originally properly centered, may also become displaced in consequence of the softening of the material when exposed to a high temperature. In wires covered with masticated india-rubber these particular defects do not occur to such an extent, but on the other hand it is necessary that the joints of the spirally twisted slip should closely adhere, otherwise in every place of imperfect contact a fault would be generated. The masticated india-rubber when warmed is softened, and will yield to constant pressure; in this way wires so covered have failed. Another cause of defective insulation is the presence of some foreign substance or impurity. Very efficient tests have been devised to ascertain the existence of faults in a covered wire during or after the process of manufacture, and we do not doubt that a cable may be delivered by a manufacturer entirely exempt from faults.

But faults may originate during the process of laying, and even after this operation, in consequence of previous or subsequent injuries. Such injuries are sometimes mechanical, as from abrasion or other causes, and sometimes electrical, as for instance at places where the wire is eccentric or where the insulating covering is less perfect than in other parts of the line, the electricity escaping at the weak place may gradually injure the insulating covering so as at last to create a serious fault; this effect will be more likely to occur in long submarine lines, where strong currents of electricity are employed. In lines which can be worked with very weak currents such injuries may sometimes be postponed for a a long time These faults are difficult to discover and repair; but certain testing operations founded on the laws of the voltaic current have enabled the place of a fault to be determined within certain limits, which can be the more accurately ascertained according as the insulation of the line is more perfect. Several of the modes of testing actually employed, are described in the evidence.

At the period of the commencement of our inquiry the insulating properties of the gutta percha then in use had been found to be such, that improvement was not deemed absolutely necessary. But we have shown above that the leakage of electricity through gutta percha, as then prepared, is very large, and that if a foreign conducting substance should have found its way into the gutta percha covering of a wire, or if an air bubble should occur in the covering and when immersed become filled with water, or if the conducting wires become excentric, the electricity will leak out at these weak places, and when currents of any strength are used will gradually destroy the insulation at the spot; and the failure of the line will only be a question of time. We have, moreover, shown that perfectly pure gutta percha, prepared by means of new processes, is far superior as an insulator to that in use at the time when we commenced our inquiry; and we have also shown that other substances are vastly superior to ordinary gutta percha in insulating power and less liable to injury from heat. Sufficient time has not, however, yet elapsed to submit these new substances to the test of experience, and the past history of the submarine telegraph shows that the greatest precaution is necessary in employing any new material until its durability under all circumstances, as well as its electrical properties, have been ascertained. Amongst these substances we have mentioned india-rubber, which formerly failed and has been again introduced, in consequence of attempts having been made to use it, so as to avoid the defects to which it was originally found liable. For instance, it is necessarily laid on in ribbons, and it is proposed to effect the union of the edges of the ribbon without the use of any injurious solvent. In one case it is proposed to place the surfaces of newly cut material in immediate juxtaposition, by which means

they unite; in another case the ribbon wound spirally on to the wire is covered with vulcanized india-rubber, and the whole is subjected to a high temperature and pressure to compel the union of the edges; in the third case the ribbon, after being wound spirally on the conducting wire, is passed through hot water; another suggestion is that the liquid milk as it comes from the tree should be brought over and be applied directly to the wire so as to solidify for the first time upon the wire, but this has never yet been tried. Caoutchouc is always much more free from impurity than gutta percha, its properties are therefore far more constant, and the little experience which has been obtained as to its durability shows that there is no ground for apprehension on that score. Of the other compounds experimented upon by us the one proposed by Mr. Wray possesses remarkable powers of insulation; but how far the absence of experience of the durability of these new materials may warrant the adoption of any of them in lieu of gutta percha is still a subject requiring great care and consideration. If, however, it should be thought unadvisable at once to adopt them, gutta percha is now a well-tried material, the newly introduced pure gutta percha has remarkable insulating properties, and the perfection with which copper wire can be coated with it renders it an efficient insulator under ordinary circumstances; and it is established beyond a doubt, that, although instances have occurred in which it has been damaged by marine organisms, and been found liable to decay under rare and peculiar local circumstances, this gum will last for many years when used in moderate temperatures, coated with Stockholm tar, protected from light and covered with water.

Construction and Laying of Submarine Cables.

2. *Insulating Covering.*

Experiments on induction and insulation.

The general result of the experience thus gained is, 1st, that no material, however promising, can be relied on until it has stood the test of time; and 2nd, that too much care cannot be given to the manipulation of the insulating covering of a conducting wire, which is composed of substances so delicate in texture, and employed in such a manner that if a single defect, however slight, escapes detection during the process, the whole result is vitiated, as is the case with a chain when a single link is faulty. Perfection will be most likely to be secured by carefully testing the core in defined lengths, in water of a specified temperature, and causing every particular of the results of the tests to be carefully noted for future reference in case of faults.

3. *The External Protection.*

3. *The External Protection.*

As we have already seen, all the means that had been adopted for preserving the insulation of telegraph wires under ground or through tunnels had been found, more or less, inefficient, except in cases where the pipes which contained them were saturated with moisture or filled with water. This was an important consideration in favour of the durability of submarine telegraphs, but the preservation from decay was only one of the objects to be attained by the protection of the insulated wires of a submarine cable. Gutta percha is an extremely tender material, and is peculiarly liable to injury from the slightest cause. It requires, therefore, to be protected, even in the process of manipulation, and during its transport from place to place. Nothing has been found more effective for the first protection of gutta-percha wire, preparatory to any other covering, than wrapping it, immediately after it is made, with an ordinary tape wound spirally about it, the tape being steeped in tar or some other similar substance, to ensure its preservation and protect it from the air. A submarine line is, moreover, exposed to a very considerable strain during the act of paying it out of the vessel while laying it down in the ocean. It is necessarily subject to much handling during this process, and to a considerable amount of tensile strain, as it has to pass over "drums" through a break, and lastly, over the stern of the vessel into the sea. In heavy weather the motion of the vessel as it rises and falls throws considerable stress on the cable, and in deep water its actual weight becomes the most important element of strain, especially in cases where cables have been paid out in depths of between two and three miles, and when it is remembered that iron wire of the length of little over three miles will barely support itself. The first cable was designed for shallow water, and a covering of iron wire adopted for its protection. The wire was laid on spirally, and the weight of the cable was, by this addition, made so great that it was not found liable to injury from the anchor of any ordinary coasting vessel. As it would evidently have been imprudent to place such a material as iron in immediate contact with gutta percha, the core was first surrounded with a considerable thickness of hemp steeped in Stockholm tar and tallow. This compound was found to have no destructive effect on the gutta percha, and formed a safe material, in which the wire strands became embedded. The result was most successful; it was peculiarly adapted to the circumstances in which it was placed; and no cable has been more durable. Subsequent cables were constructed upon the same plan, and, as long as the circumstances were similar, with similar success. The most important

CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

3. *The External Protection.*

innovation consisted in this, that, instead of laying several insulated wires together in one rope, each was enclosed in a separate cable. Thus, between England and Holland, four separate wires are enclosed in four separate cables. This system was adopted because in shallow water, where so large a number of vessels, are constantly passing, the cables were peculiarly liable to being damaged by anchors; and if one or more of the wires were damaged, the constant communication could be kept up by means of the other wires, and repairs were much facilitated by having so light a cable to deal with. These repairs involved the cost of the constant maintenance of a vessel for that purpose, the expenses of which caused the system to be abandoned, and a large and heavy cable (as heavy indeed as it was practicable to make it) has been laid down in lieu of the separate cables. It requires considerable power on board a vessel to be able to lift a heavy cable to the surface, and if a small vessel is so unfortunate as to foul her anchor, the anchor is invariably lost; but of course as the position of such a cable is well indicated to navigators, care should be taken to avoid such contingencies. In dealing, however, with great distances and depths of water, heavy cables become impracticable, unless special vessels of enormous size be constructed for conveying and laying the cables. The Atlantic cable, small as it was, and though laid down in two halves, required a ship of no less than 3,200 tons burthen to carry each half. It is evident therefore that no vessel could have been found capable of carrying a much heavier cable. It was also thought that in extremely deep water the cable would practically be so completely out of the reach of accident, that any further protection than was necessary for its mere manipulation in laying down was unnecessary; but although in the case of the Atlantic the use of a small cable was perhaps unavoidable, it is a remarkable fact that in almost all cases small cables have been found liable to mishaps, while the heavier a cable has been the greater has been its durability. So long as iron wire is used for covering the cable, the strain occasioned by paying-out in deep water is but little affected by the weight or size of the cable. Wire, however small, will break with its own weight at a length of or about three miles, and an iron rod, however large, also breaks at the same length, the strength being of course in proportion to the area, and therefore in proportion to the weight. In dealing, therefore, with deep sea cables, it is evident that the strain cannot be varied by a change in size of the cable itself, but by the alteration of the specific gravity of the substance employed. An iron cable we have seen breaks with its own weight at three miles in length; but a small gutta-percha wire being but of little greater specific gravity than water itself, might be paid out in any depth without any increase of strain. Indeed, one inventor has proposed that for great depths a cable lighter than water should be used, and anchored by weights at a given distance from the bottom, so that all abrasion would be avoided. These considerations led to the abandonment of iron wire, and the adoption of hemp as an external covering. The specific gravity of hemp is little greater than that of water, and consequently no strain occurs with hempen cables, whatever be their length. But the experience with hemp has not been satisfactory. It has been found too weak, after long immersion, to enable cables to be raised from much depth, and has been frequently eaten away by marine animals and injured by marine vegetation; besides which it is very liable to abrasion from rocks or gravel; and its absolute durability, even when free from these dangers, is very doubtful. In order to increase the strength of the cable for laying or for raising for repairs, and to increase the specific gravity of the cable, the hemp has been combined with iron or steel wires, which have been embedded in it. The union of hemp and steel has been found to give much greater tensile strength than would at first sight appear probable from such a combination.

In all cases in which small wire has been used without other protection, the rapid corrosion of the wire has been a source of great inconvenience. The iron wire in the early cables has generally been galvanized, and its durability has been increased to a moderate extent by this process; but it is by no means a durable article, even with this precaution, and its durability depends on its local position. When covered with sand or mud, provided there be nothing in the mud itself destructive to iron, the wire appears to last a considerable time under water; but in certain localities, where the cable lies upon rocks, exposed to the constant flow of water, or where the mud or sand contains peculiar constituents destructive to iron, a very few months of submersion have completely destroyed the material. To avoid this corrosion the iron has, as we have mentioned above, been coated with hemp, and this again covered with tar or some other protecting material, and its durability has been undoubtedly increased by this process. The portions of the Atlantic cable covered with tarred yarn picked up in Trinity Bay, Newfoundland, were bright and free from rust; in other cases the hemp has been abraded from the wire, the durability of the cable then becomes merely a question of the durability of the hemp or tar covering

The most simple and practical way of covering a rope, and that which consequently first

CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

suggested itself, was to lay the iron wire spirally on the core, but this method was not free from objections, among the greatest of which was the liability of such a rope to form itself into kinks, which depend to a great extent on the lay of the spiral covering. These kinks are apt to occur, whenever sufficient slack accumulates to allow them to form. If, in paying out a line, slack be allowed to run overboard whilst the vessel from any cause is laying to, it almost invariably becomes entangled and twisted at the bottom of the sea in the most remarkable manner, especially when there is a strong tide. Or if, in paying out the cable from the hold of a vessel, where it is laid in large carefully arranged coils, these coils become entangled in the hold, a large quantity of slack is rapidly drawn into the breaks, the paying out suddenly stopped, and the cable sometimes damaged before the speed of the vessel can be checked. If the ordinary twisted rope could be placed on a drum as it is manufactured, and the drum carried on board ship, and the line paid out directly from the drum into the sea by the rotation of the drum on its axis, no liability to kinking could arise; but such a process is evidently impracticable, the weight of the drum and its coil being far too great to be dealt with by any mechanical means. If such a rope were slipped off the drum, and in this state laid in the hold of the vessel, every coil that was paid out from the mass would throw a "turn" into the outlying rope, and these twists accumulating would rapidly tend to the destruction of the cable either by untwisting it, or tightening it to such a degree as to throw it into kinks. When the cable is laid in the vessel a turn is laid in with every coil; in paying it out the reverse operation undoes the turn which has been laid in, in the first instance. It is important to avoid coiling or uncoiling a cable, when finished, more often than is absolutely necessary; and even its transference from the factory to the ship, and from one ship to another, is hazardous. To remedy the inconvenience from kinking many devices have been suggested, such as plaiting the hemp or wire on the cable or laying them in parallel layers, and afterwards binding them all over with a spiral wire. Lines so constructed with parallel wires or strands possess this additional advantage, that in case of great tensile strain the parallel wires tend directly to protect the inner core from strain without in any way increasing the pressure upon it, whereas if great tension be put on a twisted cable the elongation of the cable tends to the compression of the inner core, and that to such an extent as sometimes to cause considerable damage. Cables have been thus taken up in which the insulated wire has been greatly reduced in diameter by this tension, and the gutta percha itself nearly destroyed. It also frequently happens, on the relief of this tension, that the contraction of the outer covering and of the gutta percha causes the copper to knuckle through and come into contact with the strands of iron. Or in other cases, when the cable has been elongated, the core of copper wire covered with gutta percha has been permanently elongated, and forced itself through the strands of iron wire; this effect is shown in Appendix No. 9, Plate No. 9. It is an important consideration in the outer covering of a submarine cable that the delicate insulating material which covers the conductor is liable to numerous injuries during the process of laying on the outer covering, and that although the core may have been originally perfect, these injuries will sooner or later prove the destruction of the cable after it has been laid. The plan which has hitherto been adopted of covering the core with a serving of hemp or tape, saturated with some insulating material, enables any injuries which have occurred during the process of laying on the outer covering to be concealed until after submergence; it has therefore been suggested that it would be desirable to saturate the serving of hemp or tape with a conducting material, instead of with a non-conducting material, and to keep the cable in water and under electrical tests during the whole time of manufacture and until submerged. One mode of obtaining these tests would be to stow the cables in defined lengths, in a series of insulated tanks; and to keep a constant current on the cable; the proportion of the current which escapes from each tank would show whether the portion in one tank was worse than that in other tanks. Another method is that which was adopted by Messrs. Siemens and Halske for testing the Malta and Alexandria cable and described in the Appendix No. 11.

3. The External Protection.

We have had laid before us numerous specimens of proposed cables for deep sea lines, as well as a series of experiments upon different forms of cable, made for the Treasury, by Messrs. Gisborne and Forde, for determining the form of the Falmouth and Gibraltar cable. (See Appendix No. 10.)

We have ourselves made experiments upon several forms of cable specially devised by various persons for deep sea lines, information respecting which will be found in Appendix No. 9. The following table gives a general view of the results. As one of the main objects to be attained is a minimum of extension with a maximum of breaking weight during the process of laying, we have compared the results by showing for each cable the equivalent length in water due to a definite elongation and to the breaking weight.

CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

3. *The External Protection.*

Cables in which the Strength is placed in the Outer Covering, by means of Hemp or Wires, laid on spirally.

Name.	General Description of Arrangements for Strength and Protection.	Specific Gravity.	Length of Cable in Water in Fathoms equivalent to 0·5 per Cent. of Elongation.	Length of Cable in Water in Fathoms equivalent to 1· per Cent. of Elongation.	Breaking Strain.
			Fathoms.	Fathoms.	Fathoms.
Clark, L.	Steel wire, with a slight spiral, covered with tape, shellac, and marine glue. *See* Plate, No. 5	1·92	1,610 1,119	Not reached Not reached	Not broken. 3,376
Gisborne and Forde (Gibraltar Deep Sea).	Steel wires, coated with hemp, laid spirally. *See* Plate, No. 2	1·9	1,176 2,440	1,947 3,924	Not broken. Not broken, with a weight = 5,683 fathoms in water.
Siemens	Strands of hemp, laid spirally, protected by thin copper sheathing. *See* Plate, No. 4	1·5	1,342 2,046 1,694	2,552 3,672 3,284	Not broken. Do. Do.

Cables in which the Strength is placed in the Outer Covering, by means of Hemp or Wire, laid on longitudinally.

De Bergue	Tarred hemp lines served round with cord of hemp. *See* Plate, No. 5	1·26	5,300 —	8,124 5,908	Not broken. 16,982
Godefroy	Steel wires, coated with india-rubber, and covered with india-rubber canvass. *See* Plate, No. 3.	1·9	2,653	Not reached	3,424
Do.	Do.	1·4	2,596 2,596	3,500 Not reached	Not broken. 3,309
Hall and Wells	Hemp lines kept in place by plaited hemp. *See* Plate, No. 4.	1·35	760	1,126	4,420 4 per cent. extension.
Do.	Steel wire and hemp lines kept in place with plaited hemp.	1·6	2,499	Not reached	2,729
Do.	Do.	1·9	2,437	4,525	5,597
Silver	Steel wires, covered with plaited hemp. *See* Plate, No. 2	2·8	1,642 1,832	2,260 Not reached	Not broken. 3,213
Sinnock	Tarred hempen lines, served in a close spiral, with hemp string and iron wire. *See* Plate, No. 4	1·4	622 —	1,088 463	Not broken. 2,855
	Do.	1·7	997 —	1,782 636	— 7,132

Cables in which the Strength is placed next the Copper Conductor inside the Insulating Covering.

Allan	Steel wires, laid spirally on copper conductor, outer covering plaited jute, saturated with marine paint. *See* Plate, No. 3.	1·6	1,519 1,283	Not reached 2,258	Not broken. 2,936
Do.	Do., outer covering india-rubber canvass.	1·38	3,405	5,555	7,484
Do.	Do., outer covering Godefroy's compound.	1·3	3,835	6,340	6,348

As regards the liability of these cables to kink, we would observe that the Messrs. Gisborne and Forde's (Gibraltar) cable was easy to kink, and was set in kinks by tension. Messrs. Silver's cable was disposed to kink. Messrs. Clark's, De Bergue's, Godefroy's, Hall and Wells', Sinnock's, and Allan's were difficult to kink, and the kinks generally unfolded by tension.

A cable made by Mr. Hooper was submitted after our experiments were concluded, and we had no opportunity of testing it. The strength is given by steel wires laid on in a slight spiral, protected from corrosion by means of vulcanized india-rubber. See Appendix No. 9, Plate 6, Fig. 21.

The results shown in the above table for Mr. Allan's cables are remarkable. In these cables the strength is given by steel wires laid spirally round the copper conductor, thus using the strength giving material to form part of the conductor; the area of the conductor is therefore increased; and there is no danger of the conductor knuckling through the gutta-percha from the resiliency of the latter. This addition to the conducting area

increases the induction of the cable in the full ratio of the increased area, but it only increases the conducting power of the cable in the proportion which the conductivity of steel bears to that of copper, which is about one-seventh. To obtain the same capacity for the transmission of messages in a cable of this kind as in a cable in which copper forms the sole conductor, the dimensions of the conductor must be increased to give the same ratio of conduction to induction. The insulating material of course requires protection in these as in other cables.

3. The External Protection.

All the cables in the table in which iron is used for strength have been furnished with some protection against corrosion. This is afforded in some cases by hemp, in another case by tape, shellac, and marine glue, in another by india-rubber and india-rubber canvass, in another by a mixture of cocoa-nut and gutta-percha covered by india-rubber canvass.

In the cables in which hemp is used for giving strength, the hempen strands have been protected in one case by a serving of hemp, in another by a combined covering of hemp and iron wire, and in another by a sheathing of copper. The object of employing copper being, that it is not so subject to corrosion in the sea as iron.

We consider it essential that in iron or steel covered cables the iron or steel should be protected against corrosion, both as a preservative before laying and as a security when laid, to enable them to be raised in case of injury. The protection of the iron or steel covering by hemp alone cannot be durable; nor indeed are we satisfied that any of the materials submitted for protecting the iron or steel covering would be durable if exposed to abrasion. It has been suggested that for cables in shallow water where a solid covering is wanted, the outer covering should be sought in some hard metal not acted on by sea water—probably some alloy. We believe that a coating of tin upon steel or iron wires, with a further protection by means of hemp saturated with tar or some more durable compound, or covered with a cheap form of gutta-percha would be advisable. Cables in which the strength depends on hemp will not be able to be raised for repairs, and unless the hemp is protected will be subject to destruction by marine animals and vegetation, by decay, and by abrasion on hard surfaces. The durability of hemp, however, may be much increased by coating it with some compound of gutta-percha or with marine glue. Whatever, however, be the outer covering, it should prevent any strain being brought on the inner core; the specific gravity should, therefore, be adapted to the depth and be such as to ensure the cable sinking evenly. We understand that the specific gravity of the Toulon and Algiers cable (1·9) recently submerged in depths of from 1,600 to 2,000 fathoms has proved satisfactory in practice. It is also to be observed that the necessity for joints to be made in a cable whilst laying and after it is laid down is sure to occur, and must be provided for. The difficulty of making a joint even in gutta-percha wire has already been alluded to, and whatever be the outer covering, it is evident that it will often be found necessary to make joints in it at sea under circumstances of considerable difficulty. In the ordinary twisted cable a splice can be made almost as secure as the original cable. Longitudinally covered cables will necessarily present greater difficulties, and the fact that joints will have to be made must be borne in mind in the selection of the outer covering of a cable. When three or four wires are included in the same covering, a defect in any one of the wires renders it necessary to cut the entire cable, and this difficulty is no doubt an objection to this kind of cable.

4. *Laying and Maintenance of Submarine Cables.*

4. Laying and Maintenance of Submarine Cables.

We have only incidentally considered the question of laying submarine cables.

All the evidence which we have collected on the subject tends to show that no cable should be laid without detailed survey being previously made of the bottom of the sea where it is to be laid, in respect both of the irregularities of the surface and of the material of which the surface is composed. These materials should be subjected to analysis, to ascertain whether they are likely to produce any chemical action detrimental to the cable, and the course in which the cable is laid should be selected with a view to avoid, as far as possible, any bottom upon which either mechanical or chemical injuries are to be apprehended.

The Channel Islands telegraph, to which we have already alluded, affords a strong instance of the necessity of such a survey. The Red Sea Telegraph, which has been entirely corroded where it lies over the Dhalac Bank, is another; the Bona and Cagliari cable, in which corrosion and fracture have occurred in deep water, is another; and the Cagliari-Malta telegraph, where the failures have occurred in comparatively shallow water, on a bottom strewed with rocks, or, as some persons have suggested, subject to volcanic action, is another.

Any ground liable to anchorage should be carefully avoided, and also water so shallow as to be affected by surface currents, yet whenever it is practicable the depth should

CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

4. *Laying and Maintenance.*

not be greater than to allow the recovery of the cable and its repair when necessary. It has been found almost impracticable to recover cables that have been laid in greater depths than from 300 to 400 fathoms, nor, except in very rare cases, have any under water currents been found to exist beyond a depth of from 60 to 100 fathoms. This, therefore, should form a criterion of the depth to be selected, provided such depth and a favourable bottom can be found. Silt or mud has in some cases been found to corrode the iron covering of cables very rapidly, in other cases to preserve them, according to the constituents of the mud. All uneven, rocky bottoms should be avoided, especially in the neighbourhood of volcanic action. The surveys that have been made have been far too superficial in this respect. It has very properly been observed that if the land between London and Brighton were under water, and were surveyed from the surface at distances of 10 and 15 miles only, the soundings might accidentally so occur as to indicate nearly a level bottom, whereas we know how undulating the country really is. So important is the selection of a proper site for a cable that soundings should be taken at least every two or three miles, and much more frequently when any great irregularity is indicated by those soundings. In this way dangerous reefs and precipitous spots might be avoided, and generally, with but little diversion, favourable ground might be found.

We trust that no telegraph cable will be laid in future until the best course has been selected by detailed investigation of this nature.

With respect to the mechanical operation of laying, we believe that many improvements have yet to be made. The first requisite is that the ship should have a large amount of surplus power, so as to be able to make way against contrary winds and seas.

The paying out a long cable from a vessel, however well prepared for the purpose, offers greater difficulties than would at first sight be imagined. The cable is laid in the hold of a vessel in as large a coil as possible, and for this purpose there are but few which will allow space enough, hence all ordinary vessels require considerable modification. The space must evidently be perfectly clear, no cross beams from side to side of the vessel can be allowed, and no perpendicular supports for the upper deck can be permitted throughout the whole of the area occupied by the cable. It is especially in steamers where so much of the hold is occupied by engines, that the difficulties in this respect are experienced. The cable must be so placed as to load the vessel evenly, and must be so paid out that she shall preserve an even keel. Consequently, when, as in some cases, the cable has been laid in two separate holds at the head and stern, in a vessel not properly adapted, portions from one and the other must be alternately paid out, and the cable be, therefore, so laid that this shift may be made with facility, and without fouling any portion of the apparatus or of the rigging. But with ships arranged for the purpose, and provided with large water ballast, this difficulty is not experienced. Thus the Zandvoort cable paid out from the "William Cory" was coiled in two large coils, and the paying out of one coil was completed before the second coil was commeneed upon, water being admitted as required to keep the vessel in trim. When the velocity with which a cable is paid out is considered, say from four to six knots in the hour, the change above mentioned in a heavy sea is an operation involving considerable risk, and it should, therefore, if possible, be avoided. Moreover, vessels loaded with cable are liable to roll to an inconvenient extent, especially screw steamers, which are particularly applicable for this purpose. A cable stowed in a sailing vessel and towed by a steamer is unmanageable in a heavy sea, the steering is extremely difficult, and in case of accident, it is impossible to check the progress of the sailing vessel containing the cable. It is therefore imperative, especially with a long cable, that the vessel employed should be a steamer of sufficient dimensions to contain coals, and the cable for the whole voyage. A vessel capable of laying a cable, weighing three tons per mile, across the Atlantic will have a load of 6,000 tons besides coals, and the clear space in her hold must be of sufficient dimensions for coiling this enormous length of cable. The hold must also be completely protected from the heat of the boilers. It is evident, therefore, that for such a purpose a vessel would have to be specially constructed, as no vessel existing, except the *Great Eastern*, would be adapted to the work. The labour attending the paying out of a cable is far greater than would be at first sight imagined, several relays of hands are necessary. The cable is coiled in the hold with as much regularity as possible, and each layer of coils is kept in its place by lashings of hemp, and sometimes by packings of wood for that purpose. The hands employed in freeing the coils have a difficult duty to perform. When going at full speed the coils have to be handed out with great rapidity, and yet with great regularity, to prevent their being thrown into the breaks more speedily than required; while the lashings and packings of wood have to be carefully removed, so as to liberate only so much of the cable as is required. The guides through which the cable

passes must be carefully contrived to avoid the possibility of the cable getting out of the sheaves on receiving any check. It next passes through the break, which requires the most delicate handling to ensure the proper strain. If sufficient pressure be not put on the break the cable runs out from its own weight with a far greater velocity than is due to the speed of the ship, and a large per-centage is wasted.

4. Laying and Maintenance.

The speed with which a cable is paid out should evidently precisely correspond with that of the vessel, and in deep water the greatest care is necessary to obtain such a result. In shallow water less difficulty is experienced in this respect; and so accurately were the steerage and regulation of the breaks under command in the first cable laid across to Holland, that though laid during a gale of wind, only 7 per cent. of cable beyond that required for the direct measured distance was laid. This operation was performed, too, in a rapid tideway, the tide being transverse to the course, first on one side of the vessel and then on the other. The steering of the vessel was never, therefore, in a direct line, but at a considerable angle to the direction it was intended to go. When it is remembered how seriously the drag of the cable over the stern of the vessel interferes with the steerage, the great care that is necessary in the navigation will be appreciated. Another difficulty occurs from the derangement of the compasses of the vessel by so large a quantity of iron in their vicinity; for although the compasses may have been first adjusted for the deflection due to this disturbance, yet as the cable is paid out, its amount varies very considerably. To meet this difficulty a separate vessel is required to guide the vessel containing the cable. In the case of the Dutch cable two tugs were employed always ahead of the vessel containing the cable. A straight course was made by these tugs alternately, and buoys were placed to make for during the day, while at night Bengal lights were exhibited from the tugs. It was, no doubt, owing to these precautions that so small a per-centage of waste took place.

The cable, after passing through the break, runs over the stern of the vessel into the sea—in some cases over a sheave, and in others through a hawse pipe. But the steerage of the vessel is far less interfered with if the cable is allowed free lateral motion over the stern of the vessel. For this purpose the rail round the stern is protected by a strong plate of cast iron, and on this plate the cable is shifted from side to side of the stern, as the steering renders it necessary. Its position requires constant changing, for the friction soon cuts deep grooves into the cast-iron plate, if allowed to run long in the same spot. The break usually employed has been a large drum of cast iron round which three or four turns of the cable are laid, the surging being affected by means of a cheek which guides the cable on to the drum. The break is checked by an ordinary friction strap. Nothing has been found more effectual than constant personal superintendence for the regulation of this pressure, and to facilitate this operation many ingenious mechanical arrangements have been employed. The construction of the break is, indeed, a matter of the greatest importance. It is a machine which must be absolutely free from failure of any kind during the whole of the voyage, as any accident happening to the break endangers the safety of the whole operation. The laying of the cable should be uninterrupted from the beginning to the end of the journey, and with this view no derangement should be possible either in the machinery connected with the break or in the engines of the vessel. The speed, also, should be maintained as uniform as possible, and some speed always ensured, whatever may be the weather. To ensure this end a vessel with abundance of power is absolutely indispensable, for even the choking of a pump, the heating of any part of the machinery, the derangement of a valve, or indeed the slightest mishap in any part of all this complicated machinery, may prove the loss of the whole cable. If in spite of weather or tides the vessel could infallibly preserve a moderate speed across the Atlantic, the laying of the cable would involve but little risk. As long as the weather is calm, and all in good order, the operation is an extremely simple one, requiring only proper care and attention. But in heavy weather, when the vessel is pitching and rolling to such an extent that the men can scarcely keep their feet in the hold while unlashing and freeing the cable, the cable when the surging of the vessel throws sudden and unequal strains on the break, and more especially during dark nights when the breaksman himself can scarcely keep his place, and can see nothing of what is going on around him, the difficulty is of no ordinary kind. Paying out cable from the hold is much facilitated by an arrangement invented by Mr. Newall, technically termed the cone and rings, and described in the evidence appended. Several self-acting breaks have also been proposed and used, but these have met with only partial success.

Proposals for alterations of form from the break-wheels hitherto used will be found in the Appendix; for instance, Mr. Longridge proposes that the cable should pass in a spiral groove round a cone, and that the friction on the cone should afford the necessary retardation to the passage of the wire; this friction being varied as occasion arises by

CONSTRUCTION AND LAYING OF SUBMARINE CABLES.

4. *Laying and Maintenance.*

varying the length of groove which the cable is made to traverse. Another plan is to pass the cable between friction rollers. Captain Selwyn, R.N., proposes to obviate the liability to kink which a coiled cable possesses by placing the cable on a large floating cylinder, which is intended to be towed by a steamer, and which would roll over in the water and uncoil the cable as it progressed. We have great doubts as to the practicability of this plan.

The question of the paying out of submarine cables and the mathematical laws to which they are subject in paying out have been already fully investigated in numerous well-known publications.* We would, therefore, only observe that whilst we have described the arrangements in general use, the form of paying-out apparatus to be adopted will necessarily depend upon the cable to be used and the depths at which it is to be laid, and must therefore be determined by the engineer in each case.

* A considerable amount of information will be found in the Appendix on the general arrangements which have been adopted for manufacturing and laying submarine cables.

The contracts that have been entered into have always necessarily presented considerable difficulties, differing entirely, as they do, from any ordinary contract, in which the work, when completed, can be easily measured, examined, approved, or rejected. If, on the one hand, the contractor is made solely responsible and liable for the protection of the line, and is to furnish a guarantee for his sufficient performance of the contract to make or lay it down, then it becomes a matter of great importance to determine for what period of time such a guarantee should be required. Moreover, if this responsibility is laid on the contractor, it is evident he ought to be subject to little interference from other parties, as any guarantee he may give will evidently be useless if, in carrying out the work, he is simply to obey the orders of others. On the other hand, if the control over the work is kept entirely out of his hands, it is difficult so to arrange the terms of the contract as to give him any important interest in the success of the undertaking, and without such interest it is absolutely impossible, in work of such a character, to ensure that diligent, constant care and attention to every detail which is so essentially necessary to its perfection. Again, the risk attending, more especially, upon laying down submarine cables in deep water, is evident from past experience, and is so great that no contractor can undertake the work without a very large margin for profit, which consideration, while it seriously increases the cost of such an enterprise, does not in practice secure its being properly carried out. The Electric Telegraph Company have preferred leaving the construction of the cable to the contractor, but the manufacture in every detail is under the personal superintendence and direction of their own officers, who are, in fact, responsible for the quality of the workmanship. In such case, no extravagant profit need be paid to the contractor, who incurs but little risk. In the other cases, where the whole matter has been left in the hands of the contractor, who, having only in view the laying down of the cable, which should be protected for a limited length of time, does what he has agreed to do in the most economical manner possible, and without regard to the ultimate durability of his work. All such contracts have been proved to be objectionable, and telegraph companies have acted, in respect to the manufacture of telegraphs, in a manner totally different from that in which they would have acted in any other operation. They begin by simply asking the contractor for what amount he would be prepared to lay down a cable in a given position, guaranteeing his performance for a limited time, as though a cable, unlike a railway or any other means of communication, were a thing which would necessarily, when once laid down, last for ever. No greater error could be committed. Nothing can be more precarious than the life of a cable. The company which, without regard to its future maintenance, provides capital for merely one cable must be fortunate indeed if such a course should not prove its ruin. The future repair, maintenance, and restoration of a cable is as important an item as the maintenance of a railway.

Capital for all these purposes should be provided, as well as for the mere construction of the cable itself. With our present experience no company would be justified in relying upon the successful action of one single cable in deep water, and two at least should be provided for in the original estimates, to meet the chances of accident in laying down. And beyond this, arrangements should be made that a sufficient reserve fund should be available for constant repairs, and if necessary renewal. This is also a most important consideration in regard to the route that is selected, for although the protection afforded by deep water is evidently very great, the chances being that the life of a cable will be much longer in deep than in shallow water; yet, on the other hand, faults do occur at the greatest depths, and in such case the cable is necessarily abandoned, as no repair is possible.

III.—Summary of Principles which should govern the Construction and Laying of Submarine Telegraphs.

Summary of Principles which should govern the Construction and Laying of Submarine Telegraphs.

The materials of which a telegraph cable is composed, and the dimensions and form in which these materials are to be combined, necessarily depend upon the conditions of position, depth, nature of the bottom of the sea, and the climate in which the cable is to be laid. Each case will require special consideration and arrangements peculiar to itself, and we can only point out the general principles to which regard must be had by an engineer, in designing and laying a submarine telegraph cable.

We have already stated these general principles, and we propose now, therefore, only to give a short summary of the main points which it is necessary to observe.

The following are the points to which we would invite attention:—

A.—*Construction of the Cable.*

Construction of Cable.

1.—The Conducting Wire.

The Conducting Wire.

The conductivity of the metal, other circumstances being the same, does not influence the amount of induction; a bad conductor, therefore, increases the resistance, the induction remaining the same. Hence the conducting wire should be formed of the material which possesses the highest conducting power which can be selected. To ensure this it is desirable in contracts to provide that the conductivity of the wire shall be equal to that of a standard wire at a specified temperature, and then what the wire wants in quality must be made up in quantity at the contractor's expense. It is, however, better to obtain the material with the highest conducting power, because the larger diameter of the inferior conductor would give rise to increased induction. The standard wire should be of some metal or alloy not liable to oxydise, and not subject to rapid variation in conductivity from change of temperature. The metals and alloys we have described in an earlier part of the report appear well adapted to the purpose.

The conductor should be formed of a strand of wire, or of some modification of the strand form, as before mentioned, so as to prevent the fracture of one of the wires rendering the whole cable useless.

2.—The Insulating Covering.

The Insulating Covering.

Of the materials which have been submitted to us the best insulator by far is india-rubber; Wray's compound and pure gutta percha, nearly resemble india-rubber in its insulating properties.

The induction discharge is directly as the length of a wire. The amount of induction discharge from wires of different diameters with coverings of various thicknesses of the same insulating material may be assumed to be, for practical purposes, directly as the square root of the diameter of the wire and inversely as the square root of the thickness of the insulating envelope.

Hence by increasing the diameter of the wire and the thickness of insulating covering in the same proportion, the amount of inductive discharge remains the same. The force of the current in a voltaic circuit increases as the square of the diameter of the wire (the length of circuit being constant and the resistance of the battery being inconsiderable as compared with that of the metallic portion of the circuit), consequently if it be found inconvenient to increase the conducting wire and the insulating covering proportionately, greater advantage will be obtained by increasing the diameter of the wire than the thickness of insulating covering, for whilst the covering remains the same the induction discharge increases only as the square root of the diameter of the wire, whilst the force of the current increases as the square of the diameter; and if the insulating covering be varied the conducting wire being constant, the strength of the current will remain the same, but the induction will only decrease as the square root of the thickness.

India-rubber surpasses all other materials in the smallness of the amount of its inductive discharge and the perfection of its insulation. A coating of india-rubber is fully equal to a coating of the gutta percha hitherto in use of double its thickness. Wray's compound and the recently manufactured pure gutta percha closely resemble india-rubber in both these respects. The mixture of imperfectly conducting materials with gutta percha has the disadvantage of greatly reducing the insulation and increasing the induction. The interposition of cotton thread between the wire and an insulating coating considerably increases the induction and diminishes the insulation. The induction is augmented because the cotton thread, which is a bad insulator, increases the surface of the conductor; and the insulation is impaired, not only because the insulating coating is diminished, by the thickness of the cotton, but probably also in consequence of the greater inductive action. The interposition of cotton between two layers

SUMMARY OF PRINCIPLES WHICH SHOULD GOVERN THE CONSTRUCTION AND LAYING OF SUBMARINE TELEGRAPHS.

of insulating material is equally disadvantageous. The interposition of a viscid insulator between two coatings of insulating material neither decreases the induction nor improves the insulation of the line, but the viscid fluid has a tendency to fill up air holes or flaws in the insulating coatings. Generally speaking the more perfect the insulating property of the material is, the less is its inductive capacity.

India-rubber and Wray's compound are not perceptibly affected by any ordinary increase of temperature, but increase of temperature has a very decided influence in diminishing the insulation of gutta percha. This substance is, therefore, not well suited for cables to be laid in tropical regions. Temperature affects the induction discharge only in so far as it affects the insulation.

Construction of Cable.

The Insulating Covering.

India-rubber and gutta percha are subject to deterioration by exposure to the action of oxygen in the presence of solar light; but when light is excluded gutta percha will remain for months, and india-rubber for a considerable period unchanged in air, and both will remain unaltered for years in water when light is excluded; indeed sea water is peculiarly favourable to the preservation of gutta percha, especially when coated with Stockholm tar. As regards Wray's compound, we have seen a specimen of his No. 2 material which it is stated has been exposed to alternations of temperature and exposure during two years, but sufficient time has not elapsed since its introduction to enable us to express a definite opinion as to its durability.

India-rubber, gutta percha, Wray's compound, and Chatterton's compound all absorb water; the absorption is more rapid from pure water than from sea water, and more rapid from sea water than from concentrated brine. The thickness of the material affects the rate of absorption in a peculiar manner. The absorption of water by thick sheets stops short at a limit which is rapidly exceeded by thin sheets, pressure does not appear to increase the amount of absorption; and it does not appear that this absorption is to be feared as a cause of deterioration in submarine cables.

Pressure greatly improves the insulation, whilst it is being applied, and this effect is more perceptible as the substance is a worse insulator. But it does not appear that pressure asserts any influence on the amount of induction discharge.

The manner in which gutta percha can be manipulated and placed on the wire by being forced through a die, renders it less liable to flaws when laid over wire than india-rubber, but india-rubber is generally more free from impurity than ordinary gutta percha; the more perfect insulation afforded by india-rubber, pure gutta percha and Wray's compound enables flaws to be detected, which would pass unnoticed with ordinary gutta percha. The occurrence of flaws in the insulating covering of conducting wires can be best guarded against by laying on the material in several coats and by testing each of the coats under water at a specified temperature. These electrical tests should be continued during the covering of the cable and whilst it is being submerged, in order that comparative results may be obtained throughout.

3.—The External Protection.

The External Protection.

The form of the outer covering must in each case depend on the local circumstances affecting the site in which the cable is to be laid; and in selecting it, regard should be had to the fact that it is necessary to provide for the repair of the cable everywhere, except in depths which interpose a limit to the possibility of raising it.

It should be such as to protect the internal core against injuries or strains in laying or in raising for repairs, against the attacks of marine animals, and against abrasion upon a hard bottom; and it must be capable of having joints made in it with ease. It should be such as to give the cable sufficient specific gravity to ensure its sinking evenly. The material by which strength is given to the cable should be effectually protected against corrosion, and should be arranged so as to furnish the required strength with the minimum amount of elongation.

The suggestion that the materials used in the external covering should not be sufficiently good insulators to prevent the detection of injuries to the internal core during the process of covering, is one which deserves consideration.

4.—General Conclusion as to Form of Cable.

General Conclusion as to Form of Cable.

The construction of a shallow water cable must, of course, differ from that of a deep sea cable. The shallow water cable will not be much subject to injury in laying from the strain brought upon it, but it will be liable to abrasion and injury from currents, anchors, and other causes, and must, therefore, be constructed with a view of being raised frequently for repairs. For this class of cable the preservation of the outer covering from corrosion is of the first importance. The most desirable covering would, if it could be found, be a strong metallic covering not liable to corrode in sea water, rather than iron,

wire covered with hemp or other material. For cables beyond the reach of anchors, and even of strong currents, it may be necessary to employ iron or steel wires to obtain the necessary strength for raising for repairs, either during laying or after they are laid. In this case the danger from abrasion is less, and the iron or steel wire must be protected from corrosion by means of some outer covering. We think such an outer covering is to be sought in tarred yarn, protected by some cheap compound of gutta percha or india-rubber. The wires by which strength is given should be laid on longitudinally, or with a very slow turn, and must be kept in place by a special binding, or by means of the covering compound. Cables of this general form may also, we believe, be made applicable to the greatest depths which will be met with. In any case the outer covering should be so devised as to prevent a strain coming on the core; and the specific gravity should be adapted to the depth and be such as to ensure the cable sinking evenly.

Summary of Principles which should govern the Construction and Laying of Submarine Telegraphs.

Construction of Cable.

With our present experience we believe the safest core for a submarine cable is a strand of purest copper wire, in which solidity is obtained by one of the arrangements before mentioned, and coated, according to the locality in which it is to be employed, with purified gutta percha or with india rubber, protected by a covering of gutta percha, and the whole served round with tarred hemp to form a bed for receiving the protecting external wires; each coating of insulating material should be tested in water of a specified temperature to ascertain that its insulation was equal to a specified standard. The size of the conductor and the thickness of the insulating material should be such as to allow of the line being always worked with moderate currents. A long cable must, however, insulate sufficiently to resist the tension arising from currents termed "deflections" or "earth currents," which Mr. Varley states have sometimes, in a distance of about 70 miles, a tension of more than 100 cells of Daniel's battery. See Question 2914.

B.—*Laying and Maintenance of Submarine Cables.*

Laying and Maintenance of Submarine Cables.

1.—Preliminary Survey.

Preliminary Survey.

Before the route, in which a submarine telegraph cable is to be laid, is decided on, a careful and detailed survey of the nature and inequalities of the bottom of the sea should be made, and that line selected where there are fewest probabilities of injuries from mechanical or chemical causes, and where (if possible) the depths are such as to allow of the cable being raised for repairs. In such a survey it is of more consequence to ascertain the relative differences of level of the bottom of the sea at every step than the actual depths; and it would be of great advantage for this purpose if some instrument could be devised which would enable the actual outline of the bottom of the sea to be traced. The actual position in which a cable is laid should be defined with the greatest precision attainable, to facilitate future repairs.

2.—Apparatus for laying Submarine Cables.

Apparatus for laying Submarine Cables.

The failures which have occurred in laying submarine cables are mainly attributable to the employment of ships which have not been constructed for the purpose, and of defective paying out apparatus. This latter must depend so entirely upon the form of cable to be used, and the depths in which it is to be laid, that it must be left to the engineer to devise in each case. In depths where repairs are possible the amount of slack paid out should be sufficient to enable the cable to be raised without injury for repairs.

The question of the ships to be used is one of great importance, to which sufficient attention has never yet been paid. The ship should be of large capacity, to admit of the cable being coiled easily without injury, and great care should be taken to isolate the hold from the engine room; it should be of a form to allow of the cable being paid out without any material alteration in the trim of the ship taking place; it should have sufficient power to enable it to maintain a speed of from four to six knots per hour in the direction in which it is proceeding, in any weather; and it should be very steady in a rough sea. These qualities point to the necessity of special ships being built for the purpose. There are difficulties in the way of this being done by contractors for laying cables, because they cannot afford to have ships idle on their hands, and they consequently either hire a ship, or build one to serve afterwards for other purposes. But we believe that a ship of the construction we have shown to be necessary for laying a cable would, when not employed in laying cables, be found extremely useful for the ordinary purposes of commerce.

3.—Contracts for laying Submarine Cables.

Contracts for laying Submarine Cables.

If the contractor is made solely responsible for the safe laying of the line, he cannot be made subject to much interference by other parties: but he will put on a large additional price to cover the risk he incurs, and the extra cost to be paid does not secure the success of the enterprise. If, on the other hand, he is not made responsible,

SUMMARY OF PRINCIPLES WHICH SHOULD GOVERN THE CONSTRUCTION AND LAYING OF SUBMARINE TELEGRAPHS.

Laying and Maintenance of Submarine Cables.

Contracts for laying Submarine Cables.

he will have little interest in the success of the undertaking, and without such interest it is difficult to secure that close supervision to the details of the manufacture which will alone obtain perfection.

The question, no doubt, to some extent, depends upon who the contractor is. But in any case we believe that a divided responsibility would be fatal to this, as it is to any other enterprise. Hence we should suggest that a defined, but limited, pecuniary responsibility should be placed on the contractor, who must therefore be allowed a certain liberty in his arrangements, but the specification for the manufacture should be drawn up by the engineer, and include clauses reserving to the engineer the right of approving of all the arrangements to be made. At present the manufacture of the core, consisting of the conducting wire with its insulating covering, is necessarily in the hands of different persons from those who contract for the manufacture of the external protection and for laying the cable; the core would therefore be delivered to the latter contractor in a specified electrical condition, for which he would be held liable if accepted by him.

With respect to the period for which the guarantee should be made, we believe that the best plan would be that the contractor for laying the cable should contract for its maintenance for a certain number of years, and that the arrangement should be such that whilst he received payment for the actual cost of laying, he should receive his profit, after the cable was laid, in the shape of an annual sum for maintenance, by which means the greater the number of years the cable lasted without repairs the larger would be his profit.

In providing for a telegraphic line no company can rely on the successful action of the cable; capital should be provided, not only for laying the line, but for properly constructed ships and other appliances for raising and repairing it in the depths which would admit of it, and for laying a second line to be used whilst repairs are in progress; and there should be provided out of revenue a reserve fund for the repair, maintenance, and renewal of the line.

Conclusion.

We have thus endeavoured to lay before your Lordships in a condensed form the results of our inquiry. We are much indebted to the various persons who have given us information, and for further details we beg to refer to the Appendix and Minutes of Evidence attached to this Report. We desire, however, in conclusion, to observe that we are clearly of opinion that the failures of the existing submarine lines which we have described have been due to causes which might have been guarded against had adequate preliminary investigation been made into the question. And we are convinced that if regard be had to the principles we have enunciated in devising, manufacturing, laying, and maintaining submarine cables, this class of enterprise may prove as successful as it has hitherto been disastrous.

DOUGLAS GALTON,
C. WHEATSTONE,
WILLIAM FAIRBAIRN,
GEO. P. BIDDER,
} Committee appointed by the Board of Trade.

EDWIN CLARK,
CROMWELL F. VARLEY,
LATIMER CLARK,
GEO. SAWARD,
} Committee of the Atlantic Company.

BOARD OF TRADE,
WHITEHALL, *April* 1861.

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MINUTES OF EVIDENCE

TAKEN BEFORE

THE SUBMARINE TELEGRAPH COMMITTEE

AT THE

OFFICE OF THE BOARD OF TRADE.

Thursday, 1st December 1859.

PRESENT:

The Right Hon. J. Stuart Wortley.
Captain Douglas Galton.
Professor Wheatstone.
Mr. Varley.
Mr. Latimer Clark.
Mr. Saward.

Captain DOUGLAS GALTON in the Chair.

Lionel Gisborne, Esq., examined.

L. Gisborne, Esq.
1 Dec. 1859.

1. (*Chairman.*) You are a civil engineer?—Yes.

2. You have had, I believe, considerable experience in laying down submarine cables?—I have been connected with the laying down of a good many submarine cables, but I have never personally superintended the laying down of more than one.

3. What submarine cables have you been connected with?—I have been connected with the line from the Dardanelles to Candia, which it is intended shall go from thence to Alexandria. I had originally the concession for that line, and I was engineer to the company who undertook to lay it. That company was dissolved, and it was placed in the hands of other parties, and my partner, Mr. Forde, was present during the laying down of a portion of the line. I am at present engineer to the Red Sea and Indian Telegraph Company, and to the Falmouth and Gibraltar Telegraph on account of the Government. I specified the class of cables to be laid down, assisted in making the contract, and superintended all the experiments during the manufacture of the cable, and up to the time that it went on board ship. I was present during the laying of the line from Suez to Aden, which is now in perfect working order.

4. I thought that there had been some fault in that line, and that you had not been able to certify it?—There is a question between the contractor and the company as to a fault between Suakin and Cosseir. That fault is one of insulation measurable by an instrument, but not practically appreciable in speaking through; so that, although as a scientific question, there is a fault in that cable, as a practical question, on the working of it, there is none. It is, however, intended to lift the cable where the fault exists, and to cut it out.

5. Is that fault in deep water, or is it in shallow water?—In 300 fathoms of water. I will give the Committee a short summary of the state of the line. I will commence at Suez. In paying out the cable from Suez to Cosseir no fault of any sort or description ever went overboard that we could find out, nor did any stoppage occur during the paying out. The line was landed at Cosseir, a distance, I think, of 272 knots, and up to this time there is no fault upon that line that is measurable by the finest instruments that we possess, supplied by Mr. Siemens.

6. At what depths was the line?—At 300 and 400 fathoms; the whole line lies in water not more than 400 or 420 fathoms perhaps at one spot; generally, about 300 fathoms, and seldom less than 150; the formation of the Red Sea being steep at the edges, and with a plateau at the bottom.

7. Did the gutta-percha show no loss whatever?—The gutta-percha showed the loss due to itself as material, but no more loss than it showed out of the water. I can produce to the Committee a detailed report and sections showing the relative conductivity of the copper and the gutta-percha, all referred to one unit. In the Suez and Cosseir line the loss by insulation of the gutta-percha is only what is due to it as a homogeneous mass being a slight conductor of electricity; there is no such thing in existence as a perfect non-conductor, the perfection of non-conduction being always the imperfection of the instrument. In going from Cosseir to Suakin the cable was laid down successfully; but a fault went overboard, a slight one; the vessel was slowed for a short time, and in about two hours the fault completely disappeared. We went on and the cable was laid successfully to Suakin, and for the first 10 days no fault, irrespective of the different conducting powers of the two materials, could be appreciated. In about 10 days a fault manifested itself, and as a scientific question increased rapidly for 10 days, and then it became stationary, and that fault still exists. The last reports that we have are that the fault is, if anything, better than it was, certainly better than when it was at its maximum, and that it is improving rather than getting worse. The position of that fault has been carefully determined by measurements made from both Suakin and Cosseir, and it is known to be at the same place where the fault went overboard; the position of that place is accurately marked on the charts, it is close in to a little islet within 150 yards in about 300 fathoms of water. It can be lifted and mended, if required. That line is 534 knots in length.

8. Is the resistance of the fault known?—Yes.

9. Does it vary under testing?—It does not; it varies a little from day to day, but it is scarcely appreciable. The magnitude of the fault is measured according to a standard, which I can give the Committee when I send in the report, if they should desire it.

10. What is the maximum depth at which the

L. Gisborne, Esq.

1 Dec. 1859.

cable is laid in the Red Sea?—I think 420 fathoms, certainly less than that.

11. (*Mr. Varley.*) Is no variation at all perceptible when the fault is being tested?—I cannot answer that question so well as Mr. Siemens; I do not pretend to be an electrician, and never did. I am an engineer, and I think I understand the manufacture of cables and the principles of them.

12. (*Chairman.*) What is your opinion as to the practicability of lifting a telegraphic cable successfully in 400 fathoms of water?—My opinion is founded upon the fact that they have been already lifted in that depth. I may proceed to state that we started again from Suakin and laid the line down to Aden, and in the laying down of that line we got a fault overboard, and it was a serious one; it was caused by the jamming of the cable at the cone against one of the rings; we were in between 300 and 400 fathoms of water, and we laid to on that cable for, I think, 18 hours. In the meantime we had been steaming on slowly to keep up a sufficient strain on the cable to prevent its forming kinks, and we picked up between seven and eight miles of that cable and found the fault, and the whole loss when cut out was found to exist in that fault, proving the cable behind us to be perfect. A splice was made and we continued it on to Aden; there was a fault close to Aden in the splicing of the shore ends to the main line, but that was corrected immediately, and when the line was landed at Aden we found it practically perfect, and it has remained so ever since.

13. Have you found no difficulty from the shore ends lying in the hot sand?—Considerable difficulty from the hot sand, and we had to take up the shore ends, that is of the land line, (which was buried in a trench, not in the water) at Cossier, twice, in consequence of faults occurring, and eventually we had to lay the shore work at night.

14. How are the shore ends covered?—Generally speaking, the cable itself is connected.

15. Is there no protection against the heat?—No; we buried it three feet under the sand. I may mention as a question of heat, that when we first tested the cable on board the "Imperador," about 900 miles of cable, at Suez, the insulation was so bad that we could not speak through it on board ship.

16. You attribute that to the temperature?—Yes; as we laid down the cable and it got into the water the insulation improved steadily. We were able before we had gone many miles to deduce a rough law, by which we could ascertain the proportionate state of insulation of the cable according to the temperature. We found the temperature of the sea in the Red Sea generally 81° a foot below the surface; I have seen it as much as 90°, and at 300 fathoms about 73°.

17. What was the temperature in the hold?—It was sometimes 92°.

18. At what temperature does gutta-percha become plastic?—It is a difficult question to answer; it depends very much upon the position it is in. I have hung up a piece of gutta-percha wire over the awning exposed to the sun, I have had it hung up for an hour, and it did not become plastic in the sun in a current of air; I have laid a gutta-percha wire covered with iron upon the bulwarks, with the thermometer alongside it, and when the sun was 110° on that thermometer the gutta-percha oozed out like sealing-wax.

19. (*Mr. Saward.*) Were the bulwarks painted black?—Yes.

20. (*Chairman.*) The thermometer ranged 110°?—Yes. I have had a piece in the engine-room, where the thermometer was generally 100°, and it has not been affected in the least, but the lead covered wire carried along the decks as a connexion from the hold to the instruments melted away though covered with wood.

21. You mean that the gutta-percha melted?—Yes.

22. (*Mr. Saward.*) Would that be due to radiation?—I think it is due to the conducting power of the heated metal. When you have the gutta-percha in a current of air, the heat does not affect it so much as when it is lying out of a current of air and exposed to the sun. The question of temperature was very much elucidated by our proceedings in the Red Sea, in consequence of the cable getting into a cooler medium as it was laid, and so much was that the case that in one instance, when we were laying the cable, we had to connect one hold and another hold together, so as to work a great deal more cable than the length we were going to lay, and we had to insert a pricker, and make a temporary connexion with half the cable in one hold, to enable us to speak through with the station we were corresponding with from the ship. During all the time of the laying experiments were registered constantly, and the testing of the cable was continued throughout all the time it was being laid.

23. (*Chairman.*) Was the weather calm during the time it was being laid?—Most of the time; we had strong breezes part of the time.

24. Did the vessel pitch at all?—Enough to make me sea-sick; we had no rough weather. The paying-out machinery worked admirably when properly watched. We never had any accident in the paying-out machinery, the watching being more necessary to control the amount of slack being paid out than from any danger to the cable itself.

25. Can you describe in a few words the general nature of the machinery?—The machinery consisted of a drum, round which the cable took four or five turns, with a friction strap on it; the friction strap was worked by a lever, in which various weights were placed, and the weight upon that lever represented the strain upon the drum. The cable, after passing over the drum, passed under a sheave working at the end of a short lever, which we called the "indicator;" this short lever and the sheave were quite disconnected with the friction strap, so that they had no effect upon it; but by weighting this short lever and sheave to an amount equal to the weight upon the friction strap you got a sort of balance, so that when the weight upon the cable became greater than the weight upon the short lever the lever rose, the cable being at an angle under it, and as soon as that lever rose the man at the break would then take the pressure off the strap, and thus compensate for the extra strain that was coming upon the cable, from whatever cause. The great object of this was not so much to prevent the cable breaking as to allow the man at the break to know that he was keeping a uniform strain on the cable, the uniformity of the strain depending upon the uniformity of the depth; and to such accuracy did we get after some practice, that we used often to signal to the "Cyclops" beforehand what depth of water we were running in when we had come to a change of even 50 fathoms; and so perfectly did that machinery work, that with the last 254 miles of the cable which Mr. Newall laid from Aden towards India, we covered 252 miles of ground in 300 fathoms of water, and as to the cable which was laid down from Suakin to Aden, which is 637 miles long, the loss by the slack is only three per cent.; but before we knew the machinery well we lost as much as 16 per cent. during 24 hours. The average loss upon the whole cable is under 10 per cent.

26. Has the break much inertia when the vessel pitches, and is there a sudden strain brought upon the cable by that means?—I never saw these vessels pitch violently; but I think that the pitching of a vessel has very little effect upon the paying-out machinery if properly attended to, the length of the cable behind the vessel is so great in anything like deep soundings, and the small angle of 10 or 11 degrees which the cable makes with the stern of the vessel, gives it such enormous elasticity, that the rise and fall of the stern of the vessel in pitching is practically nothing compared to the length that you have in the elasticity of the cable.

27. You do not think that there is much danger to be apprehended from the inertia ?—There is no doubt that with a very light cable and a heavy vessel that rolls very much and pitches and does not steer straight, you can bring a larger amount of inertia upon the machinery; but with an iron-covered cable that weighs anything like a ton to a knot, and in moderate soundings of a thousand fathoms, or which a cable can easily attain by its own strength, I do not think that, generally speaking, the pitching of a vessel seriously affects the working of the machinery.

28. What was the weight of the Red Sea cable ?—Twenty-one hundredweight per knot.

29. (*Mr. Stuart Wortley.*) Is not the angle less at the stern of the vessel the lighter the cable is ?—The angle depends partly upon the specific gravity of the cable, but also upon the speed of the vessel.

30. Assuming a given speed, is not the angle smaller in proportion to the lightness of the cable ?—Yes; the Red Sea cable was payed out generally at an angle of 11 degrees.

31. (*Chairman.*) You mean with the horizon ?—Yes; we had a stern sheave, the upper periphery of which was fixed at an angle of 11 degrees above the bottom of the jaws over the stern, through which the cable payed out, so that when the cable was paying out and we saw it working in the jaws, we knew that the angle was about 11 degrees; when we saw the cable stationary in the bottom of the jaw, we knew that the angle was greater, without taking the trouble to measure it; and when we saw the cable, on the contrary, very much about the jaws and paying out nearer to the top, we knew that the angle was too small, or, in other words, that the strain upon the cable was too great.

32. Then you ascertained throughout the strain upon the cable by the angle ?—Yes, that was one means of estimating it; I never felt during all the time we were paying out the cable the slightest doubt or fear as to its breaking in the machinery. Our whole object was to lay as little cable as possible, allowing sufficient slack for the depth we were in.

33. Could you measure the strain upon the cable in deep water ?—We measured it the whole time. There was a regular log kept, in which was registered constantly what the cable was doing, and that log has been filled up, and calculations made since I came home, and the whole history of the cable during its paying out is now a matter of figures.

34. What was the greatest depth that you passed in laying down the cable?—420 fathoms.

35. (*Mr. Varley.*) Do not you imagine that in so great a depth as 1,600 or 2,000 fathoms, where you say that the strain would be greater, that the inertia of the machine would play an important part on the pitching of the vessel in a very rough sea ?—Of course; but generally speaking, I do not think that if the construction of the cable is properly apportioned to the depth, that the inertia of the machinery then need play any important part in laying a cable. If you tried to lay a cable weighing 10 tons to a mile in soundings of 1,000 fathoms with light machinery, the machinery would not, of course, answer. If you laid it, and it only weighed 10 cwt. to a mile, in 50 fathoms, with very heavy machinery, the machinery would not answer; but I think that if the specific gravity of the cable is proportioned to the depth at which you are laying it, that then machinery can be designed so as not to have any dangerous inertia.

36. Did you find that the strain varied exactly as the depth of the water in which you were laying the cable ?—When we came to know it, it did it perfectly; but it is entirely a question of the eye, the ear, and the judgment at the moment. Mr. Newall laid 600 miles of cable in soundings up to 400 fathoms depth, losing only 3 per cent. slack, and the last 254 miles of the cable we laid at a speed of 8 and 9 knots, and we covered 250 miles of ground in about 300 fathoms of water with a monsoon in our favour.

37. (*Chairman.*) Of the other cables with which you have been connected, did any fail ?—There has been one cable which has failed three times, the cable from Candia to Alexandria.

38. What were the causes of the failure ?—When the company that I was connected with took up the Indian line, they gave up the Dardanelles and Alexandria line, which they had only taken up because the Turkish government insisted upon its being made, and when they so gave it up, my connexion with it practically ceased, but still I advised, and was more or less mixed up in the matter. An iron-covered cable had been designed, something like the Corfu and Malta cable, and it was intended to lay that cable, but Messrs. Newall, who had undertaken to lay this cable at their own cost, as a matter of economy tried a hemp-covered cable. Here is a piece of it (*producing the same*) that was picked up in 400 fathoms of water, and this hempen cable had to go down in depths 1,600 and 1,800 fathoms, and as much as 1,900 fathoms; they paid out 129 miles of this cable, and they covered 128 miles of ground, and it was blowing extremely hard, and they hung on to that cable for 36 hours, blowing so hard that boat communication between the man-of-war and themselves was impossible. At the end of that time a fault was found to exist—a fault as to insulation; they tried to pick up the cable, and it broke, and they were then, I think, in 1,500 fathoms. Since that cable failed, experiments have been tried by me and others upon hemp-covered cables. There is one cable which has been in the water for some months, and the Committee will see that the hemp has shrunk so as to force the core right through, and in many places forced the copper through the gutta-percha, showing that hemp alone is not a proper material to cover a cable with. First of all, it is much too elastic; this cable will stretch 4 or 5 per cent. before it breaks; when it is once got into the water, the tendency of it is to shrink, the outer covering will shrink; the gutta-percha will try and shrink, but being held back by the copper, it is prevented, unless it forces the copper to shrink with it, and then the copper knuckles out, so that this hemp-covered cable has been proved to be perfectly useless for the object for which it was intended; the failure must be entirely attributed to the want of proper materials having been used in the manufacture of the cable, and not to any fault inherent in the core itself as a core, or in the manner of laying it, as laid down by Mr. Newall.

39. (*Mr. Stuart Wortley.*) Was there any symptom, when that small hempen cable was laid down in 1,800 fathoms, of any effect from the pressure of the water upon it ?—Not that could be ascertained; it broke during the paying out.

40. (*Chairman.*) Do you know at what depth the fault was ?—I think it was in 1,500 fathoms of water, and the fault came on gradually.

41. Was the nature of the fault ascertained ?—The cable broke where the fault was supposed to exist. I have no doubt that the fault occurred to the cable because it can stretch 4 per cent. before it shows any fault of insulation, and that is a very dangerous thing. If the strain upon the cable had been throughout perfectly uniform, still the slack would be less than one per cent., and it is fair to assume, in so light a cable going to a great depth, that the strain was sometimes unequal; in that case the cable would stretch, the effect of which upon the conductivity of the cable would not be felt till some time afterwards, when the hemp began to shrink. My belief is, that the fault that occurred was due to the stretching and shrinking of the materials of which it was composed.

42. What length of time does it take before hemp saturated with tar begins to shrink in the water ?—As soon as it is saturated; under pressure within a few hours.

43. (*Mr. Saward.*) With regard to the hemp that was used in your experiments, was it first shrunk or saturated with any fluid, or was it wound spirally round the core in a dry state ?—It was put on saturated with Stockholm tar.

44. (*Mr. Varley.*) I think you stated that the fault

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appeared gradually, and went on augmenting till it became so large as to stop the communication?—Yes.

45. Do you not think that if the fault had arisen from the knuckling of the copper that it would have arisen suddenly, and after a certain amount of tension had been accumulated, sufficient to cause the copper to go; when once it started, it would go suddenly through the gutta-percha?—No; I find the contrary to be the fact from the experiments I have made. I can show the Committee pieces of core which have stretched 25 per cent., and are electrically perfect, and these pieces of core left to themselves for some weeks gradually became electrically imperfect; the copper and the gutta-percha are in a constant state of antagonism, and any weak place that shows itself is gradual, whether in the copper or in the gutta-percha, and the electrical connexion becomes imperfect.

46. But invariably gradually?—Not invariably; but I can state a case in which 25 per cent. of elongation was given to a piece of core without any electrical fault, and after a few weeks there was a serious one, and now the copper has bulged up.

47. That fault came on gradually?—Yes.

48. Have you met with any other faults of a similar nature that have come on suddenly, or any more faults which have come on gradually of the same kind?—I have never stretched but one piece of core to that extent; stretching core six or eight per cent. does not do it any harm at first.

49. The copper does not knuckle out?—No, not at first; I have tried the Gibraltar core, which is very thick.

50. That is not the same core as the Dardanelles line?—No.

51. (*Chairman.*) What is the thickness of that core?—About the same size as the Atlantic cable as to thickness.

52. How many coverings of gutta-percha has it?—Three, I think.

53. Have you considered whether, if the hemp had been previously saturated in any kind of suitable fluid, it would have been possible to prevent the shrinking of the hemp cable?—I have tried experiments which are embodied in tables which the Board of Trade have, and upon almost every sort of hempen cable which has even been proposed. I have refused nobody to try an experiment upon any class of cable, some of the samples are in this box, and the results will speak for themselves; but they are that in every case the cable elongates when hemp covered in a manner which eventually injures its electrical state.

54. Have you ever tried Rowett's cable?—I think so, and without success.

55. (*Professor Wheatstone.*) Has this specimen been actually taken from the bottom of the sea?—Yes.

56. I observe that the wire is very eccentric; did this occur in its original manufacture, or subsequently to its having been laid?—I think that is accidental where it has been cut.

57. To what do you attribute that eccentricity?—To imperfect manufacture.

58. Not to any effect as having been laid down at the bottom of the sea for any length of time?—I think not.

59. Or the action of heat?—No.

60. Do you not think it impossible to get any very great length of wire absolutely centred; will not a certain amount of eccentricity always be found in gutta-percha covered wire as at present manufactured?—I see no reason why perfectly central wire should not be manufactured. In all manufactures the chances of faulty places are great, but I see no more reason why you should not get practically perfectly manufactured core than why you should not get a perfect casting.

61. (*Mr. Saward.*) Are you familiar with the invention of Professor Hughes for applying a semi-fluid substance?—Yes.

62. Do you perceive any mechanical difficulty in the application of it to a cable, and if not, do you think it would be an advantage?—I am now getting a mile of cable made with Professor Hughes's application in it, and until that cable has been experimented upon I should not like to give a definite opinion; but my own impression is, that, professionally, we should be wrong to trust to any self-acting system of mending a fault, the great point being to prevent it beforehand.

63. If it be true, that it is a useful substance, and of easy application, would it not have a tendency to make the centreing of the wire more perfect?—No, I do not think it would have a tendency to make it more perfect. Perfect centreing can only be attained by putting on a succession of extremely thin coats of gutta-percha. The experimental committee have a piece of cable with 20 coats of gutta-percha round the core, and it is impossible that that should be badly centred; there are 20 courses in that cable (*pointing to the same*).

64. (*Mr. Stuart Wortley.*) Did you say that in the case you have been alluding to, which was a case of faulty construction, the cable in its character was left to the choice of the contractor?—Yes; he took the whole responsibility of making it, laying it, and paying for it.

65. Did you not also say that you thought the choice of the contractor was influenced to a great extent by a desire to economise the expense?—Certainly; and still more so after the contractor had failed in laying this cable (*pointing to the same*), the manufacture of which never was looked after, and which, I believe, never was properly tested beforehand, and was made as cheap as possible; after having had this experience, he took the rest of this hemp-covered cable and covered it with iron, but did not put iron enough upon it so as to prevent the hemp being visible between the strands; and he took that same cable out and tried to lay it, and in laying it an accident happened to the machinery. In altering the weight upon the lever, one of the men in charge of the machine turned a screw, which required very delicate handling, to lift the lever; he suddenly jammed it, and the cable broke in the machine, and was lost in 1,200 or 1,400 fathoms of water. Up to that time the insulation of the cable was good, although during its manufacture it had never been properly tested so as to ensure its being good. That is failure No. 2. Now comes failure No. 3: Mr. Newall having failed in laying this cable twice, or rather Mr. Liddell, his partner, having failed twice, Mr. Newall, his partner, came to the Board of the Red Sea and Indian Telegraph Company, and asked permission to take some of their cable to lay between Candia and Alexandria; they gave him permission, and he took it; but instead of taking all the 420 miles he required, he only took about 315 miles; he put it on board ship and sent it to the Mediterranean, and commenced laying it at the eastern point of Candia, and he laid down all the 315 miles of Red Sea cable which they had lent him perfectly, with practically no loss of insulation at all, and then attached to it the remainder of this iron-covered cable, which had been originally a hemp-covered one; he tacked on this old iron-covered cable, and they had not paid out five miles when a fault occurred. The fault was a slight one at first, but it went on rapidly increasing, and by the time they had paid 25 miles they were obliged to stop. They picked up 20 miles of this improperly manufactured iron cable in a depth of from 1,200 to 1,500 fathoms of water, and at the end of 20 miles it broke, and it broke apparently from having got round a rock; but the conclusion that I would draw from these three failures is, that they are all three attributable to the want of care during the manufacture of the cable, and not to anything that is intrinsically due to the cable as a submarine conductor of electricity, or to the means adopted for laying. You will remark that the gutta-percha on this cable is thin for a depth of 1,200 or 1,500 fathoms of water,—the Red Sea cable; the core is too thin for a depth of 1,500 fathoms of water, still in both cases have those cores answered

their purpose in that depth where they were properly manufactured. In the last instance, the whole of the very deep water was passed when the fault occurred. With regard to the Red Sea cable, which it is proved by the result has been carefully looked after during the manufacture, a portion of that cable was laid in as much as 1,900 fathoms of water, and laid perfect; the imperfection only took place when the attempt was made to lay another cable imperfect *per se*, which neither I as an engineer nor Mr. Siemens would have allowed to be right, had we been responsible for the expenditure upon it.

66. Do you say that you are of opinion that if that choice of the cable had not been left to the contractor, and its election had not been so much influenced by economical considerations, and it had been selected by competent authorities, and manufactured according to their directions, there is no reason why that cable should not have been successfully laid?—Not the slightest; on the contrary, I can quote the case of a cable that was laid in the same sea, the Cagliari and Malta and the Malta and Corfu, which is as slight as this is, and it was laid in 1,500 fathoms water perfectly successfully, the cables having failed since from causes quite irrespective of the manufacture of the article and its laying.

67. (*Chairman.*) What was the cause of the failure of the Corfu and Malta cable?—Mr. Siemens will give you the best information upon that subject.

68. And what was the cause of the failure in the Cagliari and Corfu cable?—The original fault, as far as I can learn, was caused by the cable being grappled by fishermen; the second fault was caused by the first being improperly mended, and the third fault was caused by the cable being lost in 300 fathoms. I do not give this as information of my own knowledge, but from what I have gleaned.

69. You have no doubt that cables can be safely laid in deep water, say 2,000 fathoms?—I have no doubt that you can lay a cable in almost any depth of water, if you apportion its specific gravity and its strength according to the depth at which it is being laid. You will see by the tables the results of the experiments made upon the strength of cables by Mr. Siemens and myself. There is a column in which is run out the ratio between the specific gravity of the cable and its breaking weight, and the depth that that breaking weight represents, and you will find in this box, No. 9, specimen formed of steel and hemp, and the breaking strain of the cable represents 11,000 fathoms of water, with an elongation of less than one per cent. So that I think the question of laying cables in deep water, as a question of laying, is not more difficult to solve than the question of laying cables in shallow water, if the proper mode of construction is adopted.

70. (*Mr. Saward.*) Do you perceive any engineering or mechanical reasons for doubting the permanency of cables properly constructed and laid in deep water?—The only experience that we have of any length of time is of the cable between Calais and Dover.

71. I speak of causes existing particularly in deep water, which do not exist in shallow water, and which would affect the permanency of telegraphic cables laid in deep water?—There are extreme developed causes, either natural disintegration or chemical action. If you have very shallow water the cable will be subject to rubbing and to wear and tear on the outside, or being injured by an anchor. If you have enormously deep water, your cable will be subject to very great pressure, a pressure, perhaps, of two and three tons to an inch; and there is no doubt, under such a pressure, with such an active agent as electricity passing through the heart of your cable, there is a tendency to the disintegration of the material, both by chemical action and from natural causes, much greater than there would be in a similar cable laid at a depth where the pressure upon it would be only a few hundredweight per inch. But I do not think that a cable in 3,000 fathoms of water, properly manufactured and once laid, is more likely to injury than a cable in 500 fathoms of water. We know that gutta-percha, properly manufactured, under a pressure of five tons to an inch does not absorb water to any amount which could possibly injure it. I do not mean to say that a very thin coating of gutta-percha over a very large conductor, with very intense electricity passing through it, and all under a pressure of five tons to an inch, the water would not penetrate to that conductor, but it would only be from imperfection in the manufacture; but I do mean to say that, with a properly apportioned conductor, with a covering properly manufactured and properly laid on, there would not be much more chance of its going wrong in 2,000 fathoms of water than in 500, assuming the manufacture to be proportionately good.

72. (*Chairman.*) Have you made any experiments showing the result of a pressure of five tons per square inch upon gutta-percha?—The only experiments that I have seen are those of Mr. Mackintosh.

73. Have you seen anything to indicate that the insulation or the conductivity of the wire is in any way influenced by extreme pressure?—I cannot say; I have not subjected any length of it to great pressure; I am now putting up a pressure tank, by which I shall be able to use a pressure of 1,000 pounds to an inch, and I can put into that two knots of cable. Upon that we shall make experiments which will test that question; experiments upon conductors covered with one covering of gutta-percha to one covered with 20 coatings.

74. (*Mr. Varley.*) You stated, I think, that you thought it not unlikely, when a cable was submitted to a pressure of five tons to an inch, the electricity passing through the conductor would cause more chemical action on the gutta-percha under such pressure than in an ordinary state?—I think it is more likely to do so.

75. How do you suppose the chemical action would take place, unless the gutta-percha conducted the electricity?—All gutta-percha conducts it, to a certain amount.

76. Do you think that it conducts more electricity under pressure?—No, but the particles are brought nearer together.

77. Chemical action is due to the amount of electricity conducted invariably; therefore, unless more electricity were conducted, it would appear that no more chemical action would take place at such a pressure than in the ordinary state?—I think you will find chemical action in many cases very much developed by pressure. You will find sometimes a complete loss of action in a vacuum. Electricity is no doubt one of the many excitants of chemical action where it takes place, and it will take place under some circumstances when once excited. My argument is, that chemical action is more likely to be excited from whatever cause, under certain circumstances under great pressure, than in a vacuum.

78. You spoke of intense currents of electricity, are the Committee to understand you to mean that the kind of action that you would expect to take place would only take place under the influence of very intense currents, but that avoiding the use of that intense electricity, you would to a large extent avoid the injury you would contemplate?—Yes; the excitement due to electricity is measurable by the intensity of it. You may subject two materials to a large quantity of electricity at a low tension and no chemical action will take place, and if you subject those same quantities to a small amount of very intense electricity you will cause electrical action immediately, the ultimum of electrical action being the spark.

79. (*Mr. Saward.*) Then the element of destruction which you apprehend, in cables laid in deep water, would be very much mitigated if the mode of passing the electricity through it were properly managed?—I am comparing the same intensity of electricity passing under 500 fathoms of water to the same electricity passing under 2,500 fathoms, taking the same length, and assuming that for a length of 500 miles you would require electricity of a cer-

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tain intensity to do your work; I say that if those 500 miles were all in shallow water, the chance of disintegration or chemical action is less in 500 fathoms than in 2,000.

80. (*Mr. Varley.*) This is merely your idea of what would take place; you have no evidence to prove it?—We have no evidence in 2,000 fathoms of any cable, for none has been laid.

81. Your experience has been principally gained in the Mediterranean and in the Red Sea?—Wholly so.

82. (*Mr. Stuart Wortley.*) What is the greatest depth at which any cable lies, either in the Mediterranean or in the Red Sea, which is now in efficient order?—I will take the Bona and Cagliari cable, that lies in 1,600 fathoms.

83. Is that at the present time in good working order?—In as good order as any cable of its class could be under the circumstances. It consists of a very slight conductor covered with a thin coating of gutta-percha.

85. But it is in working order?—Yes.

84. How long has it been down?—Three years.

86. Has it ever been interrupted?—It has been constantly interrupted, as the loss of insulation is so very great, but it is a permanent loss of insulation due to the imperfection of the manufacture of the cable.

87. It has not become deteriorated since it was first laid down?—I believe not.

88. It suffered great deterioration at first, but since that it has gone on pretty steadily?—Yes.

89. (*Mr. Varley.*) Is it not true that two of the wires are wholly useless, that the third wire is scarcely ever available, and that the fourth one is only partially workable?—My information is derived only from public sources; I do not know personally whether the wires are good or not, but I have understood that three of the wires can be worked through, but there is one of them which is a good deal better than the other three.

90. (*Mr. Stuart Wortley.*) If I understand you correctly with regard to the Bona and Cagliari cable, if it be a fault, the fault is such as you would attribute to the badness of the manufacture and not to any natural causes which necessarily accompany the laying of any deep sea cable?—Certainly; I should have attributed the same faults that now exist to the cable when being first shipped; the faults ought to have been foretold by anyone understanding the subject.

91. (*Mr. Saward.*) Have you formed any opinion as to the general form and specific gravity of a cable suitable to lay in depths of $2\frac{1}{2}$ miles?—Yes. Anybody who looks over this book of experiments, of which there have been a great many, will see the different results. These are—"Experiments for the Strength of Cables, conducted by Messrs. Gisborne and Siemens for the Government." I have run out in a column in red ink the equivalent in depth of half the breaking strain of a cable, two-thirds of the breaking strain, and the total breaking strain, and alongside of that is the per-centage of the elongation; and, as a general principle, I find that where the elongation at half the breaking strain exceeds one per cent., the cable is not suited for the depth represented by half the breaking strain. For example, I will take sample 7 in this box, and I find that the elongation at half the breaking strain is 2·20 per cent.

92. Are these steel wires or iron wires?—Iron covered with hemp. The elongation at half the breaking strain represents a depth of 2,755 fathoms; from that, I should say that that cable is not safe to be laid in 1,000 fathoms of water.

93. Will you now take No. 9?—That is composed of steel and hemp. I will take the mean of the experiments, and I find, in round numbers, that half the breaking strain is 0·75 per cent., at what represents a depth of 5,000 fathoms. On that cable the total breaking strain gives an elongation of about two per cent., representing nearly 1,100 fathoms. Every 100 feet stretches two feet. I may mention that I expect to attain results better than these, because I am now conducting a series of experiments which are not yet entered in this book.

94. (*Mr. Stuart Wortley.*) At what angle do you expect cable No. 9 to go out from a ship moving, say, at six knots an hour?—At 10° or 11°.

95. In any depth of water?—Yes, above 100 fathoms.

96. Say 200 fathoms?—Yes.

97. (*Mr. Varley.*) Do you think it possible to get in sufficient quantities, in the present state of the market, steel wire for such a cable as the Atlantic cable, and are there any difficulties in the manufacture of a steel-covered cable which render it unfit for deep sea purposes?—The quantity of steel can be obtained. I have now offers from one steel manufacturer to deliver me 40 tons per week.

98. Are there not serious difficulties in the way of welding steel wires, especially of such a small calibre?—I have made a very large number of experiments upon steel welded and unwelded, from different manufacturers, and upon steel hemp covered, and the results are extremely gratifying. I find that in a cable of that sort, assuming the welds to be fairly or skilfully distributed over the cable, the strength is diminished only about half the breaking weight of the one wire where the weld is; that is to say, if this cable has 12 wires in it, and one of those wires is welded in a length of 100 feet, that the loss due to that weld is half the breaking strain of that one wire, $\frac{1}{24}$th of the whole strain, and that I can get rid of that altogether, and make the steel stronger than itself, counteracting the effect of the weld through other means: thus if I take one of these steel wires and break it by tension and then take a certain quantity of hemp, say, a pound, and put it into a strand and break it, and add the sum of these two together, they make say a ton; I can then put that hemp upon the wire in such a form that the hemp and the wire together will bear more weight than the sum of what they will separately. It may appear a paradox, but I will explain it. The result that I arrive at broadly is this, that the hemp and the iron together, in certain proportions, will bear more weight than the sum of what they will separately; and I account for that in this way:—If you take this piece of iron, and lap the hemp round it with a certain lay, in the quantity which experiment has proved to be the best, when the strain comes upon it, the first thing that stretches is the steel or the iron; supposing that there is a weak spot in that steel, before that weak spot breaks it will elongate to a certain extent, and then the hemp clasps that weak spot, and gives it the strength that it would have lost were it alone; so that when this steel alone is tested, the breaking strain of that piece of steel is its weakest point; but now I have overcome that weak point by bringing into action the force of the hemp, so that the next time any portion of the materials is giving way it will not be the weakest point of the steel, but the next weakest point, and so the next weakest point will be supported by the hemp, and so on, until the breaking strain of the compound hemp and steel wire is not the weakest point of either of the materials, but it is the mean of both; by that means I get a combination of greater strength than I can obtain separately. I have now explained the apparent paradox; as I have stated, you get with the materials together a breaking strain the mean of both materials, instead of, when separate, getting the breaking strain of the weakest part of each material.

99. (*Mr. Saward.*) Have you exposed this hemp and wire-covered cable to water for any lengthened period of time?—For some weeks.

100. Does it undergo any alteration of shape?—None. The steel wire in the hemp controls it and prevents it shrinking.

101. The shrinking of the hemp in this case would be an advantage, as it would surround the steel closely?—There must not be too much shrinkage; if

there is, the first strain will come upon the hemp and it will part before the iron has got the strain upon it. There is a certain lay and a certain proportion of the materials of steel and hemp by which you can add a very large amount of strength, and for that reason I state that I think that the specific gravity and strength of a cable and its elongation can be apportioned so as to ensure the laying at very great depths, and the question of depth becomes to an engineer simply one of the mechanical adaptation of the materials.

102. (*Mr. Stuart Wortley.*) What do you think is the greatest amount of strain that you may safely approach to in paying out a cable, considering that its breaking strain as one, how near may you approach to that?—One-half I think. I think that you may calculate your cable at half its breaking strain, with the probability that during the paying out you may sometimes attain a higher ratio from some slight imperfection or disturbance in the paying-out machinery.

103. Does not that greater strain risk the cable?—No, not if you have the means at hand to relieve the strain upon the cable, which means ought always to be at hand.

104. Do you think that those means cannot be made self-acting, so as to avoid the danger which you have before mentioned, and which occurs when it is left to the care of a man, that you are liable to get an unequal strain put on?—No. My opinion is that if you have anything self-acting, it will probably act badly. The principle that I acted upon was this, that when my own hand was not upon the lever of the break, Mr. Newall's was. A great many things may be made self-acting in the paying-out machinery, but after all you must depend upon the judgment and the knowledge and wakefulness of the man who is handling the break, and I believe with judgment and watchfulness and care you can handle a break in such a way as to know to within ten per cent. what the cable is doing, which of course is very far under its breaking strain.

105. With regard to the means of communication through the cable, will you state what number of words you are able to send through the Red Sea cable?—The Red Sea cable is working from Aden to Suez by translation at eight words a minute.

106. You have described that as being in perfect order, and therefore the Committee may take that as being the ordinary working of that cable?—I think, with time and a little more practical manipulation of the instruments and a little more care, we shall be able to work 10 or 12 words a minute; 10 words a minute for 500 miles was the amount that was expected when the contract was let, and it was to that amount that the cable was desirable; we find that the cable will work 10 words a minute through 750 miles at one stretch, and thus my anticipations have been fully carried out.

107. I understood you to say that your belief is that any cessation or interruption of the action of any of the cables in the Mediterranean is attributable not to any permanent natural causes or permanent difficulties, but rather to occasional accidents or a fault in the manufacture?—Certainly; I know of no instance in which a cable can be proved to have deteriorated electrically or mechanically of its own accord.

108. (*Chairman.*) Can you give the weight of the copper and gutta-percha in a mile of any cable that you have referred to?—The Red Sea cable has 180 lbs. of copper and 212 lbs. of gutta-percha.

109. (*Mr. Stuart Wortley.*) From your knowledge of what has taken place in the Mediterranean, are you of opinion that there has been any imprudence in the character of the contracts which have been entered into?—Yes, I think that the contractors have been left a great deal too much to themselves.

110. Are you of opinion that it is safe for the Government or for any commercial body to contract with a manufacturer to lay a cable at his own risk, leaving him to the choice of his materials and of the form of the cable?—If you leave the whole risk to the contractor, you must leave the contract in his hands, and I think it is unadvisable to leave all the risk in his hands; nineteen-twentieths of the risk consists in the imperfections which is due to the manufacture of the gutta-percha and the copper, with that the contractor has nothing to do. Hitherto the contractors who have laid cables have gone to one firm, the Gutta-percha Company, for their core. Private individuals or the Government can deal with the Gutta-percha Company as well as the contractor can. Now, if the contractor takes all the risk of laying a cable, he not only takes the risk of the loss of the money which he spends himself in covering that cable, in fitting out ships, and putting up machinery, but he also runs the risk of losing the core and having to replace it, the main part of his risk being due to that core. I do not see why you should throw upon the contractor the onus of replacing an article over which he has no control. If you supply the core to the contractor and tell him to cover it in a certain manner, and you lay it,—as to any cable that may be lost, you will replace the core and he will replace the covering,—I think that is a principle which is a fair one, dividing the responsibility, because then the perfection of the core becomes a matter of serious moment to you who have to replace it if you lose it. You will then take care to look after the Gutta-percha Company, as no contractor can or will.

111. In these contracts of which you are cognizant, has not the risk of the contractor, where it has been left to him, been confined to only a short period after the laying?—A month.

112. Do you think that that gives sufficient security for their laying them efficiently, and not giving them too much influence with regard to considerations of economy?—The risk is a commercial element, as much as an engineering one. If you think it worth your while to pay 25 per cent. to an individual, and you choose to trust to his skill to secure your property, that is your business; but if you think you can do it better by employing professional men at so much a day or so much a month, you can do so; my own opinion is that the contractor should run a considerable risk, limited to what he has actually expended himself. Take the Gibraltar core or the Red Sea cable; the price of the core of the Red Sea cable was in round numbers about 50*l.* a knot, the price of the covering was about the same; then the contractor has not only to pay for the core and to pay for the covering, but to supply ships to take it out and lay it, and he would lose all that if he lost the cable; whereas if you supplied him with the core, all that he would lose would be 50*l.* a knot upon the covering, and upon that 50*l.* a knot is his profit.

113. Is it not the case that in nearly all, or in by far the greater proportion of cables lately, the contractor has dictated the kind of cable to be laid?—Not in those that I have had to do with.

114. Was it not so in the Mediterranean?—All that I know about the Mediterranean cables is from hearsay, and what one sees in the journals.

115. I understood you to say that with regard to the one to Candia, the cable was entirely left to the contractor?—After my connexion with it ceased. My original specification was for an iron-covered cable about the same size as the Corfu.

116. And subsequently your advice was departed from, and they took their own course?—Yes; I did not know that they were going to lay a hemp-covered cable until my partner wrote to me to say that it was hemp covered. I can give the Committee another instance, in which a cable is being laid under my advice by the Dutch Government, between Singapore and Batavia. The specification was made out by me, and the contract given through me to Mr. Newall, and the cable is being laid. The manufacture of it was under my control. That cable has gone on board ship entirely perfect.

117. Where is it to be laid?—In very shallow water only 50 fathoms.

L. Gisborne, Esq.

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118. Is that any thicker than the Red Sea cable in the outer covering?—Slightly thicker.

119. If it were the case that in a large number of instances the contractors dictated the kind of cable that was to be laid, should you not consider that a vicious principle?—That depends upon whether the contractors know more about it than those who are consulted.

120. Is it not opposed to their interests to some extent; in other words, would not their interest be conflicting with those of their employers to a large extent, with regard to the permanence of the cable more particularly?—As a rule I think it wrong to leave to the contractor the choice of the specification for a cable; but if commercial people will throw the whole risk upon the contractor's shoulders, he must have the control of everything. If you say to a contractor, you will manufacture a cable for me, to be laid from such a point to such a point, and you shall not be paid until that cable is laid, or if you are, you will give security for it, and after that cable is laid it must work at ten words a minute. Then a contractor must design a cable capable of doing the work required, and he then assumes all the responsibility, and ought to have the whole control. I think that is a vicious system.

121. If that sort of contract is made with a condition that he shall only be responsible for a month after the cable is laid, does not that expose him to temptation to carry it out as cheaply as he can if his responsibility is only to hold for a month?—I think that if a cable will last for a month, it will probably last for years. By lasting a month, I mean that it shows no sign of weakness in a month; if after a month you cannot detect any electrical fault in it more than is due to the materials you have used, I think that that cable is as likely to last years as days after the expiration of that month.

Mr. C. William Siemens examined.

Mr. C. W. Siemens.

122. (*Chairman.*) You are a civil engineer?—Yes.

123. You have devoted, I believe, considerable attention to electric telegraphs?—Yes, I have, in connexion with my partners in the electrical business.

124. You and your partners, I believe, have the superintendence of a large number of telegraphs on the continent?—Yes, my partners have.

125. Who are your partner?—My two brothers and Mr. Halske.

126. You have charge of the Russian telegraphs?—We have.

127. And of the Austrian telegraphs?—Not the charge of it.

128. Of the Prussian?—We have constructed many lines in Prussia and Austria and other parts, but we have only charge of the Russian lines.

129. You have been concerned in the electrical testing of several cables during the progress of laying?—We have had charge of the electrical conditions of the cable during the laying of a number of cables; most of the cables in the Mediterranean, including the Bona Cagliari, the Channel Islands, the Cagliari-Malta-Corfu, the Canea-Syra and Pyreus, and several other small cables, besides the Red Sea cables.

130. There are some points connected with the electrical testing of the Red Sea cable as to which Mr. Gisborne stated that you would supply some evidence, as that department was in your charge; will you be so good as to give us an account of the laying of that cable?—We had several gentlemen from our establishment, amongst them my brother, Werner Siemens, who superintended the electrical conditions of the cable. The cables were first tested on board ship, in order to ascertain that no deterioration in the insulation had taken place since they had left the works.

131. You had tested those cables, I suppose, at the works previously?—Yes.

132. During the manufacture?—We had not an official testing, but we tested the cable before it left the works. During the paying out of the cable, we had established a system of connecting the cable on the shore where it was laid to a clock arrangement, by which three connexions were made alternately. One is a connexion of a core of the cable to the earth; another, a connexion of the cable with the instruments; the third is the insulation of the cable at the end. By this arrangement we can, at known periods, submit the cable on board ship to different tests, in order to ascertain its condition, whether the continuity is perfect, whether the resistance of the gutta-percha does not fall below a mean, and thirdly, what is the charge that this gutta-percha covering will hold. During the progress of the laying of this cable several interesting facts or phenomena presented themselves. The temperature on board ship was exceedingly high, as much as 92°; the temperature at the bottom of the sea averaged 72° or 73°. During its transit a considerable improvement in the insulation took place, as will be observed on inspection of our diagrams, which Mr. Gisborne has presented to the committee. There were other phenomena; and first in laying the section from Cossier to Suakin, a sudden fault appeared during the paying out; this fault disappeared again after the lapse of two hours. It wholly disappeared, but four days after the completion of the line the fault reappeared, which, by calculation, was found to be in the exact spot where the fault had appeared during the laying. It is as yet a subject of conjecture what the nature of that fault can be; evidently the fault was perceptible during the time that the cable was in suspense, or had a strain thrown upon it, but disappeared when this faulty place had securely reached the bottom, and was relieved of this strain. This proves, I think, that there was in the gutta-percha some fissure or faulty place, which by a slight extension of the cable could admit the water near enough to the conductor to produce defective insulation, but which closed again by the elasticity of the gutta-percha, until by the continued action of the electric current a partial disintegration of the protecting film of insulating medium was produced. Another, and larger fault, appeared in the next section from Suakin to Aden, during its submersion at seven miles distance from Aden, but which was rectified; that was a fault in the gutta-percha, produced evidently on board ship. In the Mediterranean cables several faults appeared, both immediately in paying them out, and after they had worked for some time satisfactorily. They occurred for the most part in the Canea-Syra line, which, like the first unfortunate Canea-Alexandria line, consisted of a hemp-covered insulated conductor. These faults consisted either of cavities in the gutta-percha covering filled with water, or of places where the copper conductor did lay so very eccentric as to reach very nearly the surface of the gutta-percha. The slight film of insulating medium in these plans afforded a sufficient protection to the conductor during the process of testing, but in allowing a portion of the current to pass, electrolysis appears to set in, which rapidly changes the appearance of the gutta-percha in those places, and destroys its insulating property. In addition to these faults, which must be attributed chiefly to faulty manufacture, this cable, which was laid in 1858, and taken up again last summer, was found to be beset by another enemy in the shape of millions of small shell fish or snails, accompanied by small worms, which had completely destroyed the unsheaved hemp, and eaten some circular holes into the gutta-percha. Professor Ehrenberg, to whom we submitted specimens of those animals, is of opinion that the shell fish only attacks the gutta-percha, but that the little worms accompanying it is

really the friend of the cable, in destroying the former.*

133. Have you any specimens of those shells ?—Not with me ; but I can furnish you with them. I have brought a piece of the cable itself, showing the ravages of the animals.

134. (*Professor Wheatstone.*) Was the hemp that was used tarred ?—Yes.

135. Well saturated ?—Yes, as much as usual.

136. Did these animals make their way through to the copper wire ?—No.

137. And this was after 10 years' immersion ?—No, after one year's.

138. (*Mr. Stuart Wortley.*) Do you know upon what kind of bottom it was where this occurred ? Was it rocky, or sandy, or muddy ?—Mostly rocky; in the seas between Candia and the other Greek islands there is a great deal of rock and sand.

139. Was this piece near the shore ?—No, it was in mid sea.

140. In what depth of water ?—I cannot give the depth at the spot. In taking up the cable in the Mediterranean, which had formerly been laid from Cagliari towards Bona by Mr. Brett, it was found to be covered with corals to a depth of 700 fathoms, after which no coral appeared. I was present during the expedition to Gibraltar, which was a sounding expedition, and there we found small animals, apparently of a similar description, at a depth of 900 fathoms.

141. Was that on the coast of Portugal ?—It was off Cape Finisterre. The faults which were found in the Candia cable were probably caused to a great extent by elongation of the core, owing to the insufficient strength of the hemp covering ; but proofs are not wanting that the conductor frequently almost touched the circumference of the insulating gutta-percha. A striking instance of this description occurred only lately during the paying out of a cable from Otranto to Avlano, when a great fault was suddenly indicated ; the cable was taken back, and the copper wire, for about 12 inches, was found almost to be at the surface—it could be seen in places.

142. Who was the manufacturer of that cable ?—The Gutta-percha Company manufactured the core, and supplied it to Messrs. Newall and Co., who laid this cable.

143. (*Mr. Saward.*) Was that cable in the course of its manufacture passed through tar in a heated state, or a mixture of any kind, to preserve it from rust ?—It was tarred in the usual manner.

144. Was it an iron-covered cable ?—Yes.

145. (*Mr. Varley.*) Did you examine the gutta-percha in that part of the cable where the wire was exposed ; did you cut it across, and look at a section of it ; and did you find the marks which indicate the different layers of the gutta-percha perfect, or that the wire was out of the centre ?—I have not seen that particular piece of wire, it has not come yet ; but I have seen pieces of cable so eccentric, where the coating seemed to have been disturbed, as though the gutta-percha had been in a semi-fluid condition. I have seen pieces of that description, but in most cases the fault appears to be one of imperfect manufacture. There are sometimes air-bubbles in the gutta-percha when it is not made with sufficient care, which will allow the cable to resist perfectly, until it is put to a severe working test. These causes should be examined more closely into, and I think there is no difficulty in manufacturing a cable without any such holes and eccentric places, if the cable be properly tested, both at the gutta-percha works and at the works of the manufacturers of the cable.

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146. (*Chairman.*) You would propose to apply peculiar tests during the manufacture for the purpose of discovering eccentricities, would you not ?—I have suggested a mode of finding places of slight insulation by charging the cable with static electricity, and allowing the discharge to take place across those places, in anticipation of what the continued working of the cable would produce ultimately.

147. You imagine that those weak places, in the first place, are liable to allow a current of electricity at a high tension to pass through, and in the second place are liable to gradual deterioration by the chemical action which is taking place by the escape of the electricity at those defectively insulated parts, is not that the case ?—There is evidently great chemical action going on in the cable by the passage of a current, the proof of it is the current of polarization; when the cable has been charged for some time by the current, a return current will flow out. This return current is greater in badly insulated cables than in well insulated cables, and proves clearly some change or some chemical action having taken place. It is therefore highly probable, judging from the altered appearance, that in those places of thin insulation an excessive chemical action will set in which will disintegrate the gutta-percha, but it is equally certain that where a sufficient wall of insulated gutta-percha intervenes, no perceptible destruction of the gutta-percha takes place, otherwise it would have appeared in those cables which have been worked for some years, especially in the Dover and Calais or the Malta and Corfu line, which latter has been worked for several years, and which has not failed till lately, as it appears, by lightning.

148. Have you any evidence as to that being an accident ?—I have no positive evidence, but I have a piece of the same core which was acted upon by lightning, and it speaks for itself (*producing a piece of telegraphical cable*).

149. Where was this acted upon ?—In the Mediterranean ; it was at the Corfu end, I believe.

150. Was that on the shore or in the sea ?—Near the shore.

151. (*Mr. Varley.*) How was it struck ?—In the land line. I find that a piece of the Gibraltar core will retain the static charge for a considerable time, disappearing only gradually. I also find if the thickness of the wall is materially diminished, a discharge takes place across the gutta-percha, which establishes a passage for a galvanic current from the conductor to the surrounding water. Now such a discharge cannot, I conceive, injure the remaining part of the coating, and altogether the static electricity would not act in a sufficient quantity to produce any chemical change upon the mass of the insulating medium. I therefore am of opinion that in order to ensure the safety of a cable, it is advisable to find out the places where the insulating medium is not of a suitable thickness, and rather to destroy that portion of the cable than allow it to be submerged.

152. Do not you find that when in an electric cable the static discharge is considerable at any opening made by any weak places, the larger the opening the more liable it is to admit water, consequently making the discovery of the defective place more certain than when you are operating on shore lines ?—No doubt; we want to make as great a discharge as possible, therefore the longer the cable is, the greater

* The Government School of Mines, Jermyn Street.

MY DEAR SIR, January 4, 1859.

THE specimens you have sent me remove all doubt as to the nature of the mischief-maker in the cable. It is a bivalve shell fish, the *xylophaga*, closely allied to the shipworm (*teredo*), but distinguished from it, among other peculiarities, by not lining its burrow with shelly matter.

The *xylophaga* turns beautiful cylindrical burrows, always against the grain, in wood ; and I have no doubt it perforated the hempen coating of the cable in the same way. On meeting the gutta-percha it seems not to have liked it, and to have turned aside, thus giving rise to the elongated grooves which we see.

Nothing is known, so far as I am aware, of the range in depth of *xylophaga*, so that I cannot answer your inquiry as to whether it is probable that cables immersed in 600—2,000 fathoms water would be attacked or not.

The best way would be to determine the point experimentally by sinking bits of the cable at various depths, and letting them lie for a few months.

One thing appears to be consolatory, viz., that the *xylophaga* is evidently repelled by the gutta-percha ; for had not such been the case, you would have had that substance perforated by continuations of the cylindrical burrows which traversed the hempen part of the cable. Except by destroying the outer covering, therefore, I do not see that the *xylophaga* is likely to do much damage to the cable.

If the hemp were well soaked in creosote, it *might* stop the destroyer ; but hardly anything except a metal sheathing has been found permanently efficient in checking the ravages of *xylophaga* and *teredo* in wood.

Yours, &c.
(Signed) T. H. HUXLEY.

Captain Galton, R.E.

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the discharge we shall have, and the surer will be the indication of those faulty places.

153. (*Mr. L. Clark.*) Does not a 10-feet Leyden jar attached to a piece of cable give a very strong indication when you have passed the faulty places?—Yes, it probably would.

154. (*Mr. Varley.*) I should like to ask you for a little more explanation about the return current that you spoke of, from badly insulated cables. I did not clearly understand what you meant by that?—When the cable is badly insulated, I find the current proceeding from the cable, after it has been subjected to the current from the battery, is more powerful than a return current would be in a well insulated cable.

155. Do you mean a cable containing a defect in it, which renders it badly insulated, or do you mean a cable which is merely covered with an insulating material of a less insulating power; do you mean to say, that a cable having a defect that admits water would give more return current than a cable that has no such defect?—If the fault has become a great one, then the return current will be lessened; but if the cable is simply defective, from containing a number of eccentric places, or if the cable is honeycombed, as it has been found to be to a great extent in some of these cables, then this return current is more powerful and more enduring than it would be from a thickly insulated core.

156. You mean soundly insulated?—Yes. There is another proof of chemical action in cables. If there is a point of bad insulation, it is found that by passing a positive current for a length of time through this cable, it can be restored to a tolerably good condition of insulation. This circumstance was first taken advantage of by my brother, to maintain the early underground line wires in Prussia in working condition, and has also been resorted to to improve the condition of submarine cables; but it would be very dangerous, I consider, to trust to such means, because this re-establishment of insulation is effected at the expense of actual decomposition of the copper conductor. We find that the copper has been oxidized away in some instances, and the insulation has been partially restored for a while on account of this oxidation; but where such places exist, the cable cannot be of a permanent description.

157. (*Professor Wheatstone.*) Are you aware that Mr. Hip, the superintendent of the Swiss telegraph, made an experiment of that kind, and that it has remained two years since the cure was effected in that manner?—Yes; but in the course of time the copper will be eaten off. I think it is not a safe mode; it is an effectual mode of patching the cable, but I think a cable so patched would not be fit for continued use, and it would not be a cable that should be accepted as perfect.

158. (*Mr. Varley.*) Supposing a cable has a defect in it, and consequently cannot be worked, excepting in that way, you may then get a considerable amount of workout of it by that process?—No doubt.

159. Have you any information as to any case besides that one which has been mentioned, in which a cable has been restored to working order for any length of time?—The Bona cable has been restored to a very good working order in that way, but it can only be restored for a time; the effect of those positive currents is lost after it has been worked in the usual way.

160. (*Chairman.*) What is the exact chemical action which takes place from the positive current?—The effect is the oxidation of the copper. I have seen pieces of copper so acted upon.

161. Then the copper becomes its own insulating material, from the oxide of copper outside of it?—That appears to be the effect, and it shows clearly that decomposition of the water must have taken place, in order to produce oxide upon the copper.

162. With reference to Mr. Gisborne's evidence, what is your opinion generally as to the risk of laying cables in deep water, if properly made as to strength and specific gravity?—I can only corroborate on principle, and as the result of past experience what Mr. Gisborne said, that if a cable is properly made, and properly designed for its work, it can be laid with perfect safety in deep water. The strain upon the cable bears a proportion to the depth of the water in which it is laid, and therefore it is simply a question of calculation whether a cable can be produced that can be laid in a given depth without bringing any regular strain upon it exceeding a certain safe amount, which should certainly not exceed one-half the breaking strain, taking care that the cable does not elongate with that strain beyond the limit of elasticity of the copper conductor, or say one-half per cent. What you, I suppose, wish me more particularly to refer to is the electrical condition; there I really see no reason why a cable in deep water, say 2,000 fathoms, should not last as long as in shallower water. It is true that under such pressure a change may occur as to the degree to which chemical action extends, or the rate at which it operates upon the gutta-percha. It is possible that under this heavy pressure some of the water, which we know is contained in gutta-percha, may be set free, so that the copper can more freely act upon it. In cables of sufficient insulating medium the chemical action appears to throw itself upon weak places only, and if a cable in deep sea has a thickness of insulating material proportioned to the greater facility for the chemical action that may exist at the greater depth, I cannot see any danger in having deep sea cables. Experience, I think, proves rather the comparative safety of deep sea cables. In the Mediterranean no cable has lasted better than the Corfu and Malta cable, and if reports are true, it is now only injured by atmospheric influence, not by any cause proceeding from the depth of the water. The Cagliari cable has remained, so far as I know, in the same state as it was in the beginning. One of the wires was defective when it was first laid; it was found that the splice had not been well made. The fourth wire was restored to working condition, but the cable has never been in first-rate condition; it never has been well insulated, owing to the extreme thinness of the insulating medium. I think it has two coverings, but they are exceedingly thin.

163. (*Mr. Stuart Wortley.*) Who manufactured that cable?—The core was manufactured by the Gutta-percha Company, and the cable was laid by Newall and Company.

164. (*Mr. Clark.*) Have you any information as to the state of insulation of the Sicily and Cagliari cable?—I have not.

165. (*Mr. Saward.*) With regard to the injury occurring to these cables by lightning, is it not possible to make such arrangements on shore as to avoid the possibility of those injuries, by means of lightning conductors?—I think there are several means by which those dangers might be avoided. The first is, to put a good lightning discharger near the place of the landing of the cable. It is not sufficient, I consider, to place such a lightning discharger near the station, because lightning, striking the suspended land wire, will not find sufficient discharge at the station, but will discharge in both directions, and the cable may be injuriously affected by such discharge. A very safe way of avoiding the lightning discharges upon the cable, where there are short land lines connected with them, is, in my judgment, a wire suspended in contact with the earth over the land wire in connexion with the cable. Such an arrangement would be very inexpensive.

166. (*Chairman.*) You mean close to it?—Over it.

167. At no great distance?—The distance is immaterial; two or three feet over the insulated wire.

168. And a larger wire than the insulated wire?—I should prefer a larger wire, in order to make it capable of carrying off the lightning discharge; and I would connect this wire with the earth either at every post, or at least at frequent intervals. This wire would require no insulator, and would act simply as a safeguard; it would have an additional

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advantage in preventing slight fluctuations of atmospheric electricity from affecting the working of the instruments. The submarine instruments are naturally of a very delicate description, and it is advisable to avoid disturbing influences as much as possible. An arrangement of this description would place the land wire, as it were, between two earths, and remove from it all atmospheric electric influences.

169. (*Mr. Varley.*) Do not you think, if a wire were put deeply underground, covered with gutta-percha, that it would remove all the difficulty at once, and more efficaciously than that plan of placing one wire over the conducting wire?—No doubt a deep underground wire would effectually remove the difficulty; but underground wires in connexion with the submarine conductors for some reasons are objectionable, and sometimes even impracticable. The shore in many cases is rocky and precipitous, and it is desirable to test the submarine line as much as possible without reference to any other disturbing causes. If a fault exists, we can determine, no doubt, where that fault exists, whether in the land line or in the sea line; but if several faults exist, it is more difficult to determine with precision where the fault lies; so that, in order to keep an exact record of the condition of the submarine line, I think it desirable to divest it as much as possible of underground lines.

170. Do you consider that an underground line would be more liable to irregularity than an overground line?—It would be more liable to changes of insulation; an underground line of wire is considered not so safe as a submarine line; it is more liable to disturbances by land slips, by attacks from animals, and by accidents of various descriptions.

171. (*Chairman.*) Do you know of any cases of underground wires being injured by lightning?—I am aware of instances in which an underground line has been injured by lightning.

172. (*Mr. Saward.*) Have you given any special attention to the law which regulates the speed of currents through long insulated conductors?—I have paid attention to that subject.

173. Have you formed any opinion as to the ratio in which the retardation of the current increases with the increased length of the conductor, as to whether it increases as the square, or in what ratio?—This question is one of considerable intricacy. There are certain laws regulating the flow of electricity in the conductors; there is one law on the subject known by the name of Ohm's law, there is another law regulating the induction or the charge of the insulating covering which regulates the resistance of conductors. Now the retardation is the product of those two laws operating one against the other. If the cable is merely extended in length, without increasing either the conductor or the insulating covering, the power of speaking through this cable will decrease very much in the square ratio of the length; but it is possible to construct a cable for any reasonable speed of communication through it, if scope is given to the proportion of the conductors and the thickness of the insulating medium to the length.

174. Then it is a question of the size relatively of the conductor and the insulator?—Quite so, provided the insulating material remains the same.

175. (*Mr. Stuart Wortley.*) Are there any submarine cables in the Russian dominions?—There are some short lines; one from Cronstadt to Oranienbaum.

176. In what depth of water?—In very shallow water. That cable was laid by the firm with which I am connected in 1853.

177. Is there any deep sea cable at all in the Russian dominions?—There is not.

178. (*Mr. Clark.*) Have you had any experience of insulation by india-rubber?—My experience of india-rubber, as an insulator, is very limited. I have seen some of the earliest india-rubber coated wire; it was laid in Russia by Jacobi, and it appears that the india-rubber has not changed its nature materially by the working and the time it has been exposed. Those wires are partly under water and partly under ground. I have seen some tests lately of india-rubber-coated wire, which certainly speak very much in favour of that substance; and it may be a question whether it could not be advantageously applied in many cases, either separately or in conjunction with gutta-percha. It would certainly be free from many of the difficulties under which gutta-percha has hitherto laboured. It would be free from cavities, and it would not be affected by heat and elongation of the conductor in disturbing the centre; but whether its present state of manufacture is sufficiently perfect to warrant its application on a large scale, I am not prepared to say.

179. (*Mr. Varley.*) Supposing an india-rubber-covered wire to be bent sharply, and left in a warm temperature for some time, do you find that it maintains its centre as well as gutta-percha covered wire, or do you find that it comes out of the centre? Have you tried any experiments, with a view to ascertain whether an india-rubber covered wire would resist a kink in which the cable would be turned round sharply better than a wire covered with gutta-percha?—I have not carefully made the experiment, but from the observations I have made, I consider that india-rubber being much more elastic than gutta-percha will not yield to the action of continued pressure in the same way that gutta-percha does.

180. I ask that question, because my experience has gone rather in the opposite direction. Mr. Gisborne stated that while the Red Sea cable was at Suez, its insulation at the temperature was so imperfect before it was submerged, that they could not work through it. You stated that you could give some evidence upon that point; it would be very interesting and important, if you could give something like an idea as to the cause why communication was wholly impossible at that time?—Communication through the coil on board was rendered difficult from two causes. The first was the volta induction of one wire upon the other in the coil, which always interferes greatly with speaking through coils; but in the Red Sea cable the insulation became very considerably affected by the excessive temperature on board also. So that for those two reasons it was found advisable to divide the coil on board ship in paying it out, by pricking the cable at intermediate points. It would certainly have been possible to speak through the whole length, but the test would not have been so uniform.

181. With regard to the insulation, have you any idea of the amount of deterioration in the insulation from the temperature, which I believe was 92° in the hold of the vessel?—Between the limits of 92° in the hold of the vessel, and 70° or 72° at the bottom of the ocean, the insulation or the resistance of the gutta-percha very nearly doubled; so that when it was submerged, one half of the current passed through the gutta-percha that passed when the cable was on board the ship.

182. (*Mr. Clark.*) Do you suppose that the cable itself was in a temperature of 92° on board, or the air in the hold?—I was not present, but I understand the cable itself.

183. (*Mr. Varley.*) You have stated that there was a fault in the cable laid to Suakin, that this fault disappeared for awhile, and then re-appeared, and was tolerably steady; that fault, I believe, has not been picked up?—No; this fault re-appeared after a few days; for ten days it increased very rapidly, but at the end of one month it had assumed a uniform condition.

184. The fault did not impede the working much?—It never impeded the working of the instruments.

185. What was the nature of the current used for testing that fault or working through it—was it a positive current or a negative current?—We used both.

186. Alternately?—Yes, taking a mean of several observations with both currents.

187. (*Chairman.*) To revert to the question of covering the wire with india-rubber; do you think india-rubber can be used with advantage as an insu-

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lator, until it can be put on through a die, like gutta-percha?—The chief difficulty in the application of india-rubber is, I believe, the manufacture. I do not think that genuine india-rubber can be put on by a die. The best india-rubber for insulation is the native or bottle india-rubber; whereas after it has been worked up it is no longer in the same condition. Whether it is suitable for insulating a submarine conductor without the application of another substance, I am not prepared to say. The india-rubber is softer than the gutta-percha; and I have some doubt whether it might not receive injury by a partial pressure squeezing it away from the conductor, unless it was protected by some more rigid substance. Those are points of detail which experience must show; but I think it deserves great attention as an insulator on account of its closeness of texture, its elasticity, and high insulating properties.

188. Have you had any experience of insulation by vulcanized india-rubber?—No.

189. (*Mr. Varley.*) Had your previous remarks with regard to the test of india-rubber in a kink against pressure, reference to the bottle india-rubber or such india-rubber as is at present used for covering telegraphic wires?—I tried some experiments, with the bottle india-rubber, and I came to the conclusion that india-rubber would be more suitable for withstanding continued effort than gutta-percha. In some of those cables that have partially failed it is evident that the strain upon the cable in paying them out has permanently altered the tension of the copper; and the gutta-percha being of a more elastic nature wants to recover its original position, causing the copper to be too long for the cable and to buckle out gradually, whereas india-rubber would no doubt yield to the copper, and assume with it the new length.

190. Owing to its weaker tension?—Owing to its weaker tension and its greater elasticity.

191. (*Chairman.*) Have you not made some experiments upon the relative power of india-rubber and gutta-percha to retain charges of electricity?—I have made some experiments.

192. What is the result of those experiments?—I found that gutta-percha alone did not hold the charge well nor for any length of time, owing, as appears to me, to its greater facility of decomposition by the current, whereas india-rubber alone holds the charge for a very considerable time, for hours. In the Gibraltar cable a more tentative quality is produced by Chatterton's compound, which I am informed contains a considerable proportion of india-rubber, so that its greater power to hold the charge may be owing to that addition.

193. (*Mr. Clark to Mr. Gisborne.*) Is it the fact that Chatterton's compound contains india-rubber?—There is no india-rubber in Chatterton's compound.

(*Mr. Siemens.*) I merely made the suggestion; the fact is, that the Gibraltar cable retains the charge better.

194. (*Chairman.*) And not owing to the extra thickness of the gutta-percha?—I think the same thickness of pure gutta-percha would not retain the charge so well.

195. (*Mr. Saward.*) Referring to the questions which you were good enough to answer me, is not your brother in possession of a formula for the calculation of the retardation of the current?—Yes, my brother has investigated this subject thoroughly.

196. Is it public?—It has been published in the Scientific Memoirs. The principal formula expresses the capacity of an insulated wire of any known length and section, the specific inductive capacity of the insulating medium being all known. It is as follows:—

Capacity $\frac{2\,l}{\log \frac{R}{r}}$ K, in which

l, is the length;
R, the radius of covered wire;
r, the radius of conductor;
K, the inductive capacity.

For the correct understanding of this formula it is necessary to add that the capacity of a Franklin plate is expressed by $\frac{S}{d}$ K.

S, being the surface;
d, the thickness;
K, the inductive capacity.

197. (*Chairman.*) Assuming india-rubber to be as good a conductor or better than gutta-percha, is not the difficulty of using it wholly confined to putting it on mechanically?—Putting it on mechanically in a satisfactory manner is no doubt the most apparent difficulty. If that difficulty should be satisfactorily solved, there remains another, I think, whether india-rubber is sufficiently rigid to be trusted without adding some other medium to it or surrounding it with some other plastic substance that would add rigidity to it; but that could easily be found if the first question, that of manufacturing it in such a way that it would be impossible to have any flaws or bad joints in the covering, was thoroughly removed. It is manufactured now generally by wrapping it round. I have seen specimens very well cemented by Messrs. Silver & Co., but very irregular in thickness, and that would be a serious objection to manufacturing it into a cable, because the iron covering would not lie to it well.

198. May not every joint in that wrapping be the cause of an electrical defect?—Yes, so long as there is a joint. If it could not be proved that there is perfect continuity established, I should have very great doubts of its answering as an insulator.

199. (*Mr. Saward.*) Is it not a fact also that the slightest contact of oil or grease with india-rubber destroys it?—It does so. There are some specimens which I have seen which are very much affected by some such cause, inasmuch as the india-rubber nearest the wire becomes semi-fluid. The insulating property of the india-rubber is, however, not diminished thereby, and, unless the softening influence continues to extend, no harm would be done. This, however, can be guarded against, either by carefully avoiding the pressure of grease, or by coating the conductor first and last with gutta percha or some other gum.

200. Have you seen the composition invented by Mr. Wray, of india-rubber in combination with some other materials, which I believe he has patented; it presents an appearance somewhat resembling gutta-percha as to its hardness?—I have not examined it.

201. (*Chairman.*) Have you ever seen a piece of copper wire insulated with india-rubber through which a current of electricity has been passed more than once in which some chemical decomposition has not taken place?—Yes, I have seen some specimens that were laid down by Jacobi many years ago; I believe in 1847.

202. Submarine?—Submarine and underground; I think it would hardly make any difference.

203. In those specimens of Jacobi's, was the india-rubber in contact with the copper wire that was the best preserved as well as that which was the worst preserved?—Yes.

204–5. (*Mr. Varley.*) Does not india-rubber sometimes melt on the outside as well as on the inside, if it is exposed to any external agent?—I have observed the effect only in some specimens nearest the wire except where india-rubber had been exposed to oil or to excessive heat. In many specimens, including those from St. Petersburgh, no such effect has been observed. Respecting the Gibraltar core, I would still observe that its insulation is exceedingly perfect at present, owing partly to the comparatively great thickness of insulating medium employed, and partly, no doubt, to its being put on in three successive layers; whereas, hitherto only two layers have been used. Moreover, greater care is used to insure concentricity and uniform quality of coating, owing to the severe tests applied. This will be the first core that will be tested under pressure at the gutta-percha works, and in order to increase the usefulness of these tests, I propose to raise the temperature of the water in the pressure tank

to a uniform degree not less than 75° Fahrenheit, which is the highest degree the water of the canal attains during summer. At this comparatively elevated temperature gutta-percha is more pliable than at lower degrees, and will, therefore, yield more readily to the external pressure which will be applied to force the water into cavities, which cavities will be detected by the tests with static electricity, which I have also proposed to apply. Another material advantage will be gained, inasmuch as the results of the galvanometer or conductivity tests of the gutta-percha covering will admit of direct comparison (being greatly influenced by change of temperature in a ratio which has unfortunately not yet been determined by reliable experiments), and the loss of current by its conductivity will be as great as it is ever likely to be in summer, and greater than it will be at the bottom of any ocean.

Mr. C. W. Siemens.
1 Dec. 1859.

Adjourned to To-morrow at Two o'clock.

Friday, 2nd December 1859.

PRESENT:

Captain DOUGLAS GALTON.
Professor WHEATSTONE.
The Right Hon. J. STUART WORTLEY.
Mr. SAWARD.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

Mr. FREDERICK RICHARD WINDOW examined.

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206. (*Chairman.*) You are a civil engineer?—I am.

207. Have you given especial attention to the subject of submarine telegraphy?—Yes.

208. Will you state briefly your views upon the subject of ocean telegraphy to the Committee?—I have noted down a few points which I thought might be interesting to the Committee. When we treat of the best means of forming an ocean telegraph, I think we must make a distinction between that and the ordinary submarine telegraphs such as have been laid down at present. By that I mean to distinguish the two, ordinary submarine telegraphs I consider those of inconsiderable length laid in moderately deep water, and somtimes also in deep water. Ocean telegraphs are long lines in deep water. Now those should be very differently treated; the scientific portion of course is exactly the same, but still we may, as engineers, look upon them in a very different light. Ocean telegraphs, when they are affected by an injury, cannot be recovered; and ordinary submarine telegraphs (still adhering to the expression "ordinary") when they are injured can be recovered, and can be mended, and resume working. Consequently, it seems to me, that it is an affair of capital in the one case, and of revenue in the other, that is to say, in the case of an ocean telegraph, you should desire so to construct it, even at the expense of a very large outlay, that you may, as far as possible, avoid any chance of accident. With an ordinary submarine telegraph in shallower water you may use a smaller mileage capital; but then you would increase your yearly expenses by repairs.

209. That is to say, you may make a less efficient line and spend less money per mile?—Yes; I mean to say that you need not to go to the limit of perfection for an ordinary submarine line, but that you must do so for an ocean telegraph. I do not know it of my own knowledge, but I have heard that the International Telegraph Company have had to spend some 15,000*l.* a year in the repairs of their lines.

210. Are you referring to Mr. Stephenson's statement?—Yes; now that represents a capital of some 300,000*l.*, and I do not think there is any doubt that if half of that sum had been additionally expended in laying down the same number of wires in a proper cable, in all probability the yearly expenditure would not have been greater than that of the Submarine Company, who state that their lines have not cost them more to repair than an average of 500*l.* a year. The result would have been a saving of some 150,000*l.* to the company.

211. (*Mr. Stuart Wortley.*) Is it within your knowledge that Mr. Stephenson repudiated that statement, as it was reported. Have you ever communicated with him since?—No, I have not. It is not within my knowledge.

212. Very shortly after Mr. Stephenson was reported to have made that statement, I had occasion to communicate with him upon the subject, and he authorised me to state, that he had been reported as speaking far too generally; that his observations were intended to apply to the International lines in which they had, much upon the principle to which you have adverted, used imperfect cables, and that they had, consequently, been subjected to great expense in repairs, but that he did not mean to apply it to ocean telegraphy or marine telegraphy generally. I understand you to say that you are not aware of that correction?—I am not aware of that correction. You will observe, that I instanced the case of the International telegraph lines. I think we may distinguish, for argument, an ocean line as exceeding 300 miles in length, and in water of 2,000 fathoms depth. Now I am not aware of any line at present existing, fulfilling those conditions. The nearest approach to an ocean line, leaving the Atlantic line out of the question, is the late Corfu line; it is known that that line is unfortunately lost. I was present yesterday at the examination of Mr. Siemens, and I heard that he referred the loss of that line to accident; however in any case we know that it is lost. Now I think that it is the duty of engineers, as far as lies in their power, to reduce the chance of accident within the smallest possible limits. The chance of all the accidents that have hitherto been known to have occurred to submarine lines would, in my belief, have been lessened by adding to the working parts of the cables. I do not mean to say that all accidents would necessarily have been averted, but the chance of accident would have been materially diminished. There are three principal causes of the loss of ocean cables; first, in laying them; secondly, from too great resistance of the conductors; and, thirdly, to having a too feeble wall of insulating medium. As ocean lines if lost cannot be recovered, they should be made entirely suited to their work, regardless of cost. With respect to the first-mentioned cause of loss, namely in laying, that may be materially lessened without adding to the expense, simply by a more careful adaptation of the coating for the purpose which it is to fulfil. The resistance of the conductor and the better insulation can only be remedied by adding to the expense. I think that one of the principal causes why telegraphic lines have been imperfectly made up to the present time is, that unfortunately engineers have been too little consulted, and that concessionaires have had too much the conduct of the matter. Concessionaires are

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not necessarily engineers, in fact we know several that are not; their interest is not entirely in the lasting of the line, they are bound by their concession to supply a line by a certain date, and they naturally go to a contractor, with a request that he will supply a proper line for a certain sum of money, which is limited necessarily by their concession. The manufacturer supplies it at the price required, and it succeeds or it fails. I am sure that the Committee will not suppose for an instant, that I am warring against any individuals, but simply against a system; the system has been tried, and from the numerous losses which have occurred, I believe the system may be pronounced to have failed. In consequence of this system, telegraphic matters now have got into a dead-lock; the public will not subscribe any capital for such enterprises, unless under guarantees from the Governments interested in their formation; and from a mistaken idea of economy, originating in a misconception of the real requirements of ocean lines, the Governments refuse to give a guarantee upon a sufficient capital to make an entirely suitable line.

213. (*Chairman.*) The Atlantic telegraph cable was not made upon that system, was it?—I do not say that it was; I believe it was not.

214. Therefore the failure of that cable could not be attributed to the system to which you have referred?—No; but the cable was unsuited to its work, and its failure must be attributed in this, as in other cases, to that cause. The Atlantic was the first ocean telegraph attempted, and we did not then possess so extended a knowledge on the subject as has since been acquired. But even in those days sufficient was known of submarine telegraphy to induce the belief that the original capital of the Atlantic Company—reduced by one-fifth, as it was said to be, by payments to concessionnaires,—was far too small to create a proper and efficient line. Instead of first getting the concession, then finding out what capital is likely to be raised or can be raised, and then ordering a cable to suit that capital, I do not think it can be doubted for an instant but that you ought first of all to design a cable completely suited for its work, and then to base the capital upon that estimate. While I am recommending a what may appear a reckless cost in the manufacture of ocean telegraphs, it must be remembered that in increasing the expense of the line you are increasing also, perhaps not quite in the same ratio, the capacity of the line for earning revenue. Because by adding to its working parts you add materially to its facilities for transmitting signals. Now, I think you will admit that in long ocean telegraphs, and here I may instance the Atlantic telegraph, it is scarcely likely that that line would ever be an instant vacant; that is to say, you will be able to find as many messages to transmit as you are able to send. If you can send ten words or twenty words a minute, you will find ten or twenty words to be sent; and also from the difference of longitude, probably the line must be kept working day and night. Consequently I do not think it would be for an instant idle. Therefore the power of transmission will be the sole limit of the income it can earn, and by increasing the one you increase the other.

215. With regard to the failure of ocean telegraphs, can you state what the mileage is of the telegraphs that have failed, exclusive of the Atlantic telegraph, in proportion to that which has been successful?—I am not prepared to give you that information; there are scarcely any ocean telegraphs.

216. There are none that you call ocean telegraphs?—No.

(*Mr. Gisborne.*) I have a statement of the cables laid, broken, and lost up to May 1858, and I find about 3,000 miles of cable laid, of which 1,000 miles were lost or broken; and that includes the Balaklava and Varna line, which was laid of simple gutta-percha, and the Eupatoria line, which was also laid of simple gutta-percha, so that putting those two out of the question, it would appear that out of 2,500 miles laid there were some 500 miles lost up to May 1858.

(*Mr. Window.*) Since May 1858 many of the lines then laid, and at work, have failed; and also several others have been lost in the attempt to lay them in deep water. Such a statement, to have any value, should have been made up to the present time; it would possibly have shown a different result. In comparing the telegraph lines which have failed, with those which have been properly and successfully made and laid, and have remained in working order, there is no propriety in excluding the case of the Atlantic, which failed from preventable causes, and is the chief example of the disastrous result of starving a cable to proportions inadequate to its work, in order to keep down the capital within a certain sum. In the remarks which I have made to the Committee, I have referred chiefly to ocean telegraphs; I do not know of any ocean telegraph now existing.

217. (*Chairman.*) Your argument generally is that an ocean telegraph should be very substantially made, and much more substantially than those laid in shallow water?—Yes.

218. Have you carefully considered the best means of insulating conductors for submarine telegraphy?—I have.

219. What is your opinion as to the merits of gutta-percha and india-rubber comparatively for the purpose of insulation?—I consider that gutta-percha, though not entirely suited from its nature for very deep sea telegraphy, is certainly the best thing to employ that we know of at present; I say at present, because there are several substances now proposed as insulators which promise well, but we have not yet any practical experience of their fitness. I do not consider gutta-percha perfect for such a use from its cheesey nature, if I may so express it, its softness; and although it is not so good an insulator as I should desire to see, I do not think that our knowledge of the means of preparing india-rubber for telegraphic purposes is at present sufficiently advanced to justify us in employing it.

220. You have considered Mr. Silver's process, have not you, a good deal?—I have; I have seen several specimens which were represented as having been made by Mr. Silver's process that were in a state of decomposition.

221. Do you think the numerous joints which result from the employment of Silver's process would create a liability to failure?—I do not, because, as I understand Mr. Silver's process, the joints cease to be joints; when the cable is finished, it is made homogeneous by a process that I am not aware of.

222. Have you a specimen of vulcanized india-rubber?—I have, and I shall be happy to show it to the Committee.

223. Do you think that vulcanized india-rubber is a material that is likely to be of use in submarine telegraphy?—I think it is a direction in which it would be useful to make some inquiries, because I believe if it were possible to use vulcanized india-rubber as an insulator, it would be the best of all possible insulators for submarine telegraphy.

224. Why?—In the first place it is a very first-rate insulator; and in the next place it is almost impossible to damage it.

225. (*Mr. Stuart Wortley.*) Is vulcanized india-rubber a superior insulator to bottle india-rubber?—I am not able to say of my own knowledge; but I have been informed that it is so.

226. (*Professor Wheatstone.*) Are you aware whether any experiments have been made with vulcanized india-rubber?—Sir William Snow Harris tells me that he has made some experiments, and that he finds it very considerably superior to any other

preparation of india-rubber or gutta-percha. One great advantage of vulcanized india-rubber over any other insulator for ocean telegraphy is, that it is almost impossible to damage it; you might put it on an anvil, and hammer it with a sledge hammer, and you would not damage it in the slightest degree.

227. (*Mr. Stuart Wortley.*) Are you aware whether vulcanized india-rubber is as liable to deterioration in sea water as it is in the air?—Of my own knowledge I do not know; I have been informed that it is quite indestructible in sea water. It is extensively used for valves in marine engines, and I am informed that they are uninjured by the action of the salt water.

228. (*Mr. Saward.*) Is not sulphur used in the preparation of vulcanized india-rubber?—Yes.

229. (*To Mr. Siemens.*) Can you give the Committee any information as to the use of vulcanized india-rubber as an insulator?—My brother commenced with vulcanized gutta-percha as an insulator, which was a better insulator than unvulcanized gutta-percha, but it had to be given up, because there was a chemical action upon the copper taking place on the part of the sulphur. The sulphur acted upon the copper, producing the sulphuret of copper, which sulphuret of copper dispersed itself through the mass of the insulating medium, and rendered it an imperfect insulator. I have not made any experiment with vulcanized india-rubber, but I should be apprehensive that a similar action would take place if vulcanized india-rubber were brought near a copper conductor.

(*Mr. Gisborne.*) I know that that is the case.

230. (*Mr. Saward.*) Would not vulcanized india-rubber also be liable to decomposition from the salts of the sea?—I am not aware.

231. (*Mr. Stuart Wortley.*) It is exceedingly liable to decomposition in the air under common circumstances if we may judge from the india-rubber rings which rot in our drawers?—(*Mr. Siemens.*) I believe if there is too much sulphur in the composition it deteriorates very rapidly.

232. (*Mr. Stuart Wortley to Mr. Window.*) Have you from your experience any reason to know that there is a liability to chemical action from the proximity of vulcanized india-rubber and copper?—I have a specimen to which I wish to draw the attention of the Committee, not of vulcanized india-rubber upon copper, but vulcanized india-rubber upon brass; and it has this peculiarity that, during the process of curing, the brass and the vulcanized india-rubber become one; there appears to be some chemical union between the two, and beyond the mere surface which is affected at the instant of curing, the sulphur does not go any further into the metal. The inventor of that first of all employed it for putting india-rubber tires on to wheels; he drove the dog cart, on the wheels of which those tires were used, for two years. I believe that is six years ago, and he says that the wheels are still in a perfect condition.

233. (*Chairman.*) Who invented this process of coating brass with india-rubber?—Mr. Daft, of Tottenham.

234. Is he the patentee of the india-rubber wheels?—He is.

235. (*Mr. Stuart Wortley.*) I understand you to say that you consider vulcanized india-rubber would not be liable to injury from chafing or from any mechanical injury?—That I believe is found to be the case.

236. At the same time being an excellent insulator and very pliable?—Yes.

237. Is there any danger of vulcanized india-rubber splitting in a kink?—None whatever. The great secret of vulcanized india-rubber is properly curing it. It should be sulphurized with sulphur, and not with any liquid compound, and it should be cured at a certain temperature. If that temperature is exceeded, which I believe is very often the case, the product does not show any fault at first, and that is how such a very large quantity of bad vulcanized india-rubber gets into the market, but in the course of a few months that which is over-cured, that is to say, exposed to too great a heat in the curing, rots.

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238. Is there not a danger of pure india-rubber splitting at too great a point of tension in bending it when it is applied to a more rigid substance like metal?—I am not aware of that.

239. (*Mr. Saward.*) Do you consider that the question of the permanent establishment of an ocean telegraph presents any insurmountable difficulties?—None whatever.

240. When once a proper line of ocean cable was properly laid, do you consider that it would be peculiarly subject to special damage over and above those causes of damage to which other submarine telegraphs are liable?—No; upon the assumption that it is entirely suitable for its work.

241. Should you, as an engineer, attach any importance at all to the question which you are aware was raised some time ago as to the influence of pressure upon a cable at great depths? Should you consider that it would be liable to injury?—I should have some fears with a cable insulated with gutta-percha, but not one insulated with either vulcanized or pure india-rubber.

242. (*Mr. Stuart Wortley.*) What is the nature of the injury that you apprehend to gutta-percha from that pressure?—That the water in time, under that great pressure, might force its way through microscopic faults. Gutta-percha in its commercial form is not homogeneous, but contains dirt, fibre, and other impurities in a state of subdivision, which may form channels for the entrance of water under great and continued pressure. I have seen specimens of white and chemically pure gutta-percha; I should not apprehend the same danger if this were employed, but I have no information respecting its insulating qualities.

243. Have you any reason to know one way or the other whether the presence of sulphur in vulcanized india-rubber would be likely to protect it in any degree from animalculæ or fish?—I have no means of knowing. There is one other advantage in using vulcanized india-rubber, to which I might draw the attention of the Committee. I do not know whether the advantage is real or only imaginary, and that is, that the process of curing vulcanized india-rubber is slightly to contract it. It is prevented in these specimens from contracting by being held by the metal, but when any fracture of the metal, from any undue strain, takes place, the elasticity and contraction of the india-rubber would, when the strain was taken off, bring the points into contact again. There is a specimen where the conductor has been broken, but the points are kept constantly in contact (*exhibiting a piece of wire coated with vulcanized india-rubber*).

244. Is not the supply of india-rubber infinitely greater than that of gutta-percha?—I do not know it of my own knowledge; I believe it is.

245. Is not the supply of gutta-percha in the raw state almost entirely in the hands of one firm? and with regard to india-rubber, you may obtain any quantity?—I do not know; practically, I think, it is so. I believe the supply of india-rubber at Para is unlimited; also upon either bank of the Amazon.

Mr. Richard Attwood Glass examined.

Mr. R. A. Glass.

246. (*Chairman.*) In what way are you connected with submarine cables?—I am a submarine cable manufacturer and contractor for laying.

247. Have you had considerable experience in laying submarine telegraphs?—Yes.

Mr. R. A. Glass. 2 Dec. 1859.

248. What number have you laid?—I have a tabular statement of those we have manufactured and laid and are in working order.

249. Will you be good enough to read it?

The same was read as follows:

SUBMARINE TELEGRAPH CABLES laid down, manufactured by GLASS, ELLIOT, and Co., all of which are in perfect Working Order.

Date when laid.	From	*To	Conductors. No.	Conductors. Size.	Size of G.P., per Birmingham wire guage.	Outside Wires. No.	Outside Wires. Size.	Length of Cable, in Statute Miles.	Length of Insulated Wire, in Statute Miles.	Depths.
1854	Sweden - - -	Denmark - -	3	16	No. 2	10	2	12	36	
1854	Italy - - -	Corsica - -	6	16	1	12	1	110	660	
1854	Corsica - - -	Sardinia - -	6	16	1	12	1	10	60	
1855	Egypt - - -	- - - -	4	16	2	10	1	10	40	
1856	Newfoundland - -	Cape Breton -	1 Strand		1	12	9	85	85	
1856	Prince Edward Island	New Brunswick -	"		1	12	9	12	12	
1857	Norway - - across	Fiords - -	"		1	10	6	49	49	
1857	Across mouths of -	Danube - -	"		1	12	9	3	3	
1858	England - - -	Holland - -	4	13	0	10	00	140	560	
1858	" - - -	Hanover - -	2 Strands		3	12	6½	280	560	
1858	Norway - - across	Fiords - - -	1 do.		1	10	6	16	16	
1859	Alexandria - -	- - - -	4	16	3	10	1	2	8	
1859	England - -	Denmark - -	3 Strands		3	12	5½	350	1,050	
1859	Sweden - - -	Gotland - -	1 do.		1	12	9	64	64	
1859	Folkstone - -	Boulogne - -	6 do.		3	12	0	24	144	
1859	Liverpool - -	Holyhead - -	2	16	3	12	6	25	50	
1859	Across Rivers in -	India - - -	1	13	0	9	2	10	10	
1859	England - - -	Isle of Man - -	1	16	2	10	6½	36	36	
1859	Malta - - -	Sicily - -	1 Strand		1	10	5½	60	60	
								1,298	3,503	

250. Were those lines designed by yourselves, or by the engineers of the companies?—By engineers generally in consultation, I may say, with ourselves. But on looking through them I find that the larger lines were designed by engineers; and as to some of the smaller lines, we communicated direct with the Governments for whom they were laid, and they have adopted our suggestions.

251. (*Mr. Stuart Wortley.*) The list you have read is a list of all the lines you have manufactured that are now in working order?—Yes.

252. Are there any lines of your manufacture, and laid by you, that are not now in working order?—None, with the exception of the Atlantic. There is one that was lost in Newfoundland, in a gale, but that never was laid down.

253. (*Chairman.*) It was lost before it was laid?—Yes. With the exception of the Atlantic cable, we have had no failures, except of a temporary character. In those cases the defects have been remedied in a few days.

254. Several of those lines have been used for a considerable period?—Yes.

255. Were all those lines manufactured on principles of which you approved?—I think I may say that we had no decided objection to any; in some instances we might have suggested, perhaps, a little alteration.

256. What are your views of the conditions which should be observed in the manufacture of submarine lines?—I think first and foremost should be considered the nature of the place where the cables are ultimately designed to be laid. I think that careful surveys should be made as to the nature of the bottom, the shores, and the tideway, and things of that sort generally. Having arrived at that information, I think then should be considered the length of the line, the size of the conductors, and, more particularly, the amount of the insulating material. I think that the mode in which the insulating material is handled, probably, is as important, or more important, than anything, particularly in long lines.

257. What, in your judgment, is the best insulating material for submarine lines, dividing them first into lines for shallow water, and then into lines for deep water?—In shallow water I may say that we have practically never used anything but gutta-percha.

258. And that is evidence of your high opinion of gutta-percha as an insulator?—Yes. We have had samples submitted to us of many other insulators, which certainly show a superiority as regards their insulating properties, but we have no means of judging what the effect would be of dealing with those substances practically in laying the cable.

259. Do you believe gutta-percha to be a very durable material?—I am enabled to speak rather confidently of that. I believe it to be very durable, inasmuch as within the last week I had an opportunity, personally, of picking up and repairing the Dover and Calais cable, which was laid in 1851, and the core of that, composed of gutta-percha, I found to be, in every respect, as good as the day it was manufactured.

260. To pass from the insulating material to the outer covering of the cable, what are your views upon that point?—I think it very material that there should be an ample serving round the gutta-percha of hemp, saturated with some preservative mixture; and we have found that the mixture used in the first Dover cable has answered in every respect.

261. What was that mixture?—It was a mixture of the best Stockholm tar, pitch, a small quantity of linseed oil, and a very small proportion of Russian tallow. We have discontinued the use of the tallow, and we find that the mixture without the tallow answers every purpose.

262. Then after the serving of hemp, what would you propose?—After the serving of hemp I know of nothing, particularly for moderate depths, so good as the covering of iron which has hitherto been used.

263. Do you proportion the iron wires to the depth at which the cable is to be laid?—Yes, and to a great extent according to the nature of the bottom.

264. Do you find the nature of the bottom produce a very peculiar effect upon the iron wires?—Yes, a very peculiar effect in many places. I have seen in the Channel Islands lately, in a depth of between 30 and 40 fathoms, where the iron wire has suffered a great deal from abrasion and motion establishing friction, which has had the effect of destroying the iron wires, and cutting through the gutta-percha to the copper wire.

265. (*Mr. Stuart Wortley.*) What is the nature of the bottom to which you refer?—Rocky and sandy, with a considerable tideway.

266. Is there anything peculiar in the rocks; any iron or copper, for instance?—Not that I am aware of. From the specimens I have seen I should say that the injury has been produced from abrasion, because the wires that have not been in contact with the rock have been perfectly symmetrical.

267. (*Chairman.*) What was the size of the iron wires in that case?—The iron wire, I think, in that case was about No. 5 or 6 gauge.

268. (*Mr. Stuart Wortley.*) Did you manufacture that cable?—No, we did not.

269. (*Chairman.*) Have not you also taken up the cable between Dover and Calais?—Yes.

270. Did not you in that case find some peculiar effects produced by different parts of the bottom?—Yes. The effects seemed to be attributable to the nature of the soil in which the cable was laid. We found on picking up the cable very sudden changes in the appearance of the iron wires. In some places the wires appeared to be as perfect as when they were laid down, and within a few yards there seemed to be a total change, and the wires appeared corroded and eaten away to a considerable extent.

271. (*Mr. Saward.*) So as to affect the external coating only?—Yes.

272. Did not you discover a defect in the cable which was totally unconnected with that cause?—Yes.

273. (*Mr. Stuart Wortley.*) What was the cause of the defect?—One cause was a defect originally in the manufacture of the cable; there was a slit in the gutta-percha, which probably had been made by some workman, and then afterwards that slit was served over, but from lapse of time the electric current passing destroyed the copper wire. The other defect was one very similar. Those were faults that existed when the cable was laid down, though not showing themselves.

274. (*Mr. Saward.*) That cable was laid down in 1851?—Yes.

275. To what extent was the iron corroded in the worst parts?—I may say probably 25 per cent. the very worst. I should think not more than that.

276. (*Chairman.*) Has your attention been turned to the subject of covering the iron wire with any outer covering to preserve it from rust?—Yes, we have adopted many mixtures.

277. Have you faith in any of them?—I have faith to a certain extent. I think the last cable we covered was laid from the Isle of Man to Liverpool, and that covering certainly offers advantages, I think, that are likely to preserve the wire for a considerable time. I may state that we have found that where the iron wire has been in contact with the yarn and tar forming the serving, the wire seemed to be perfect; there appears to have been no oxidation, and the wire seems to have retained its original shape.

278. You therefore assume if it is served with hemp outside saturated with tar, that it would preserve its strength entirely?—I believe if it could be served to that extent, and that serving could be maintained on the cable until it rested on the bottom, it would preserve the iron to a very considerable degree.

279. But still that serving of the iron wires would be of no avail to preserve them from abrasion, as in the case of the Channel Islands telegraph?—Clearly not.

280. Do you find the serving liable to come off in paying out the cable, and during the process of laying?—I can only speak of that from the reports I have received from our own people, who state that they had no difficulty of that kind; there was no abrasion during the running out; the serving lasted well, and they had no difficulty.

281. Do you find any difficulty arise from laying on the serving, saturated with hot tar?—Yes, we have found considerable difficulty in laying-on the tar, as in the case of the Isle of Man cable, for this reason that it will only adhere at a certain temperature, that temperature is nearly 250°, and we are obliged to be very cautious in the operation; and it occupies a great deal of time, inasmuch as the heat is so great upon the external wires that the gutta-percha is liable to be injured. In fact, I may say that the gutta-percha was injured in one case when we were laying it on in a particular way.

282. (*Professor Wheatstone.*) By unsettling the wires?—By unsettling the copper wires in the centre of the gutta-percha; the heat from the exterior caused the wire to leave the centre, and to come in contact with the serving.

283. (*Mr. Saward.*) Are you now referring to the asphalted covering?—Yes.

284. (*Chairman.*) Do you not consider that every plan by which heat is applied to the core after it is manufactured involves risk to the cable?—Decidedly.

285. (*Mr. Stuart Wortley.*) In this list which you have handed in, I observe that the lengths of the cables vary very much. Do you consider, in practice, that it is of great importance to seek to attain a higher degree of perfection in the long than in the short lines of cable?—Clearly.

286. Have you, in practice, in some of the shorter lines laid cables of inferior quality to the longer ones?—Not of inferior quality, though less insulating material; but in the longer lines it becomes more necessary, if I may use the term, to be watchful, and to attain, as nearly as possible, perfection; because, in the operation of laying, we have no means of repairing them as we have in the short lines.

287. In a long line you would not attribute much importance to considerations of cheapness, but rather seek perfection?—I think, being guided by considerations of cheapness is as great a mistake in submarine cables as it is in other things.

288. You have mentioned the Isle of Man cable. Did not that for a time fail?—It failed for a time from mechanical causes.

289. Can you give the Committee an explanation of the circumstances under which that failure occurred?—I shall be happy to give the information which I have received officially.

290. You were not present at the laying of that cable?—I was not. It was laid by us, with the assistance of Mr. Webb and Mr. Latimer Clark. Mr. Latimer Clark can give you direct evidence as to that, if he is examined. I believe the cause of the failure was laying the cable across a race or tideway, and laying it across seaweed, which abounds there to a very great extent and of very great size. The cable, heavy though it is, was carried away, and broken in two places by the strength of the tide and the seaweed collected round it.

291. What length of time did that injury take to repair?—A few days.

292. From that time has it been perfect?—From that time it has been perfect. It has been taken up at that point, and relaid at a point less subject to that strong action of the tideway and seaweed.

293. (*Chairman.*) Are you aware whether any marine insects have ever eaten the outer covering of cables, or would be likely to do so?—That is very difficult to say. If you may judge from the Calais cable, the number of marine insects collected on that was very astonishing and curious; the most perfect parts seemed to be surrounded by the thickest crust of marine insects of all kinds.

294. (*Mr. Stuart Wortley.*) Had they in any instance eaten into the core?—No, not at all; they seemed to have fastened upon the oxide of the iron.

295. And not entered into the substance of the cable?—No, not at all.

296. That did not affect the action of the cable?—No, not in the slightest degree.

297. (*Chairman.*) Has any case come under your notice of animals eating the hemp of the cable?—No.

298. Are you aware, as a matter of fact, that they have eaten the hemp off the cable?—I have never heard of their eating the hemp off a submarine cable. I have heard of their eating the hemp of the cable laid down in the sea. I have had no experience in hemp cables at all.

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299. Have you paid attention to deep-sea telegraphy?—Yes.

300. What is the form of covering that you consider best adapted to deep-sea telegraphs? Were you satisfied with the outer covering of the Atlantic telegraph?—From the experience gained on that expedition, I think it might have been improved.

301. What were your objections to that form of covering?—I think it was probably too hollow in its nature; that it was, if anything, too soft; and I think that it was, probably, not so strong as it ought to have been. I think if it had been used in that form, then the covering should have been steel wire instead of iron. I think that would have been preferable.

302. Were not you concerned in the laying of the Atlantic telegraph?—The services of our firm were engaged.

303. Have you any observations to offer to the Committee upon that undertaking generally?—I think there is a great deal to be said upon that subject.

304. (*Mr. Stuart Wortley.*) What do you consider to have been the great mistakes made in the Atlantic telegraph?—I think those who were connected with that undertaking were to a great extent in the dark as to what they had to encounter.

305. (*Chairman.*) You think that the matter was not sufficiently considered before it was undertaken?—Just so; I think that more time ought to have been given, and experiments ought to have been made; it was far too hurried.

306. (*Mr. Saward.*) Do you consider that the division of the contract into two parts had anything to do with the failure of the undertaking, inasmuch as it prevented the cable from being tested in one piece?—Yes; I think that is objectionable under any circumstances.

307. (*Mr. Stuart Wortley.*) We have heard a great deal about one portion being laid in one direction, and another in another; do you think that that is of much importance?—I do not attach much importance to that.

308. Was there anything in the failure of the Atlantic telegraph which may not be accounted for by the circumstances to which you have alluded, namely, to the little experience which at that time had been obtained in ocean telegraphy, and the hurry with which it was laid down?—I think there was far too much hurry to commence with, and I think that experiments should have been made upon different forms of cable; I am speaking now of the mechanical laying of the cable.

309. (*Mr. Saward.*) Do you think that the experience gained by the Atlantic Telegraph Company and the experience since then, if properly used, will insure the success of deep sea cables?—I think further experiment is necessary; I think the information that has been obtained by laying the Atlantic cable may be considered as a great benefit, and likely to ensure success for a further undertaking, that is if additional experience be obtained.

310. (*Chairman.*) What additional experiments do you consider necessary?—I think before a form of cable is decided on for the Atlantic telegraph, experiments should be made in laying different lengths of cable in deep seas; that the electrical conditions of the cable should be tested when laid, and if that system of experiment be carried out there would be a very much greater chance of success, because we should ascertain the description of cable to be laid with greater certainty.

311. Do you think the difficulties arise in laying the cable, or from the pressure to which the insulating material will be subjected at the bottom of the sea?—I do not think that any pressure upon the insulating materials would have the effect of destroying the cable when once laid. I think the cable should be of such a form that the core can be kept intact until it arrives at the bottom of the sea.

312. (*Mr. Stuart Wortley.*) Do you attribute the failure of the Atlantic telegraph to the laying or to its treatment before it was laid?—I hardly consider that the Atlantic telegraph was a failure as regards its laying. I am not aware, except from what I have heard, but I believe that the electrical apparatus might have been prejudicial to it; I mean the battery power that was used.

313. You believe that the Atlantic cable was injured by the intense currents which were passed through it?—I believe that had a great effect, and was the final cause of the signals ceasing.

314. Am I right in inferring from what you have said, that you do not think it would be expedient to attempt the laying of so long a deep-sea cable as the Atlantic cable without further experiments?—I think so, to insure a fair chance of success.

315. After those experiments, are you sanguine that it is yet to be done?—Very sanguine.

316. (*Chairman.*) Do you think a shorter deep-sea cable, such as the Gibraltar cable, can be laid with safety?—For my own sake I hope I am right in my expectations.

317. (*Mr. Stuart Wortley.*) Do you think mere depth is any practical difficulty in laying a cable?—When you say "mere depth," the depth should be defined.

318. Say 3,000 fathoms?—I think a cable could be laid at 3,000 fathoms.

319. You do not think that the mere depth is any insurmountable difficulty?—I do not think that a depth of 3,000 fathoms is an insurmountable difficulty.

320. (*Mr. Saward.*) If a proper cable were properly laid in 3,000 fathoms, should you consider it as good a property as a cable laid in less depth?—I should consider it better property if once laid, certainly.

321. (*Mr. Stuart Wortley.*) On what ground?—A deep-sea line when laid, as far as we can judge, lies in a quiescent state. If it is laid intact as manufactured and in a perfect condition, it is difficult to imagine how it can be injured by lapse of time.

322. Against that is to be set the difficulty of raising it for examination, if a fault occurs?—Clearly.

323. (*Chairman.*) Do you attribute the difficulty of laying the Atlantic telegraph to the great length of the line rather than to the depth, owing to the storms that you are liable to encounter?—I must always premise that I consider storms to be fatal to such an expedition, particularly such a storm as the Agamemnon was exposed to, but in moderate weather there can be, I think, very little difficulty in laying a cable, particularly one which would be designed after additional experiments.

324. Why have you more confidence in laying a short line of deep sea cable, like the Gibraltar cable, than you would have in laying an Atlantic cable?—Because we can depend upon the weather generally for two days, which would be about the time occupied in laying the deep sea portion of the Gibraltar line, although we cannot for 10 days or a week.

325. (*Mr. Stuart Wortley.*) You have stated yourself to be a contractor for submarine cables; have you any information to give the Committee upon the system which has hitherto been pursued in regard to contracts for submarine cables? do you think the failures that have occurred are in any way attributable to the nature of the contracts?—I think many of the failures that have occurred may probably be traced to that cause.

326. In what way—what is the objection do you think, to the system which has been hitherto pursued?—I think that contracts have probably been made with contractors with the view of their taking the risk of the line, and it has been looked upon (inasmuch as it was at the contractor's risk) that the company with whom that contract was entered into, would be satisfied with whatever cable the contractor chose to lay.

327. Do not you think the contractor is under the temptation to lay the line as cheaply as he can, if it is left entirely to him?—I think that is rather the natural inference.

328. Therefore you would prefer that the form of the cable and the character of the cable should be

fixed by some independent authority, and that the contractor should merely contract to manufacture it according to that specification?—Just so; he then pleases himself whether he contracts or not for it.

329. That system would be perfectly consistent with his taking the risk of laying the cable, would not it?—Clearly; he need not contract if he were dissatisfied with the cable.

330. Has there been any case in your experience in which you have taken the whole risk, both as to the nature of the cable, the manufacture, and the laying?—Yes; the majority of the cases comprised in that list which I have given in.

331. (*Chairman.*) Still the contractor must take some risk if he insures the safe laying?—He takes the risk of laying the cable.

332. Would you be satisfied if one person made the cable and another person laid it?—No; because experience shows that we may have a minute fault, which may not show itself for a few hours, or, in some cases, days; but in the constant working of the wire that fault may become larger until ultimately the cable becomes bad.

333. (*Mr. Stuart Wortley.*) We will assume, without reference to any particular case, that defects have been discovered in cables that have been laid, which defects might have existed in the manufacture; do you consider it would be practicable to keep such a watch upon the manufacture, either by yourselves or by your agents, or by the agents of the companies who employ you, as absolutely to preclude the possibility of faults escaping notice running into the cable, and eventually being paid out?—Keeping an efficient watch has always been a source of anxiety to us, inasmuch as the whole cable is dependent upon every inch of it being perfect. We can do no more than keep the most efficient watch we are able; but with all that we yet find damages which have escaped our observation.

334. For the protection of the manufacturer as well as for the protection of the projectors, or the adventurers, or whatever we may call the owners of the cable, what system should you suggest for that purpose as the most perfect?—I think there should not be divided responsibility to begin with.

335. (*Chairman.*) What do you call "divided responsibility?"—I mean the contractors having their servants and the promoters or the companies having their servants looking after the same object. Each one considers the other as looking after their interests, and it causes a great deal of annoyance and in many cases neglect.

336. (*Mr. Stuart Wortley.*) Do not you think the character of the manufacturing establishment is the best security?—Clearly I think the manufacturer has a very large stake in it.

337. You prefer that to the responsibility being thrown entirely upon the company employing the manufacturer, having their officers to watch?—Yes.

338. (*Chairman.*) Should you be satisfied that the payment to the contractor should only be made at the end of a year after the cable was laid?—No; I do not think that quite bears upon the point.

339. (*Mr. Stuart Wortley.*) What do you think would be a reasonable time to test the efficiency of a cable?—I think that if a cable is worked through day by day constantly for a fortnight, and there is no variation in the test, or the working, you may consider that the cable is efficient.

340. You think if there is no indication of a fault in a fortnight, then the cable may be considered as satisfactory?—I think so, provided you have used a fair amount of battery power.

341. Would a fortnight be a sufficient time to develope a fault in the original manufacture?—I think it would show itself in a fortnight to some extent. I do not mean to say that the cable would fail in a fortnight, but the fault would commence to show itself, particularly in deep water.

342. Why would a fault show itself particularly in deep water?—From the pressure of the water at the greater depth.

343. Then you do attribute considerable importance to the pressure upon the cable by the water at great depth?—I think you would ascertain a fault sooner from defective insulation if you were to submit the core to pressure either in deep water or by hydraulic means.

344. (*Chairman.*) Would you have the core submitted to the same pressure before it is laid that it is to be submitted to after it is laid?—I do not know that that should follow as a matter of course. I think experiments are being made, and it should be done where it is practicable.

345. (*Mr. Saward.*) Is there not a mode by which cables can be tested under pressure as they proceed from the manufacturing process. I refer to something that I understand Mr. Reid has invented?—Yes. I have never used it; but I am informed that Mr. Reid has a patent process for testing insulated wire, but I think he has never carried it beyond 1,000 lbs. to the square inch.

346. (*Mr. Stuart Wortley.*) Would it be possible to have any machinery by which a cable, in its perfect state, could be tested by severe pressure in the course of its manufacture?—I think not; it would be attended with great difficulty.

347. (*Chairman.*) Do you consider that the principle upon which the specification for the Falmouth and Gibraltar cable is drawn is fair to the parties who make, and those who take, the contract?—I think the penalty is too large.

348. Do you consider it fair and right where you take a contract to lay a cable at your own risk, the core being supplied to you guaranteed to do certain work, if through no fault of yours you lose any portion of that cable, that the core should be re-supplied by the parties with whom you contract?—If the loss arises from a defect in the core, unquestionably. I think it should be stated that the core should be submitted to a test at the works of the Gutta Percha Company. I think that very important.

349. Before it leaves their works?—Yes; inasmuch as they are to deliver the core in a perfect state to the Government or the contractor, it should be ascertained at their works whether it is perfect.

350. (*Mr. Stuart Wortley.*) Have you in contemplation, or under examination or experiment, any new materials, either for the conductors or the insulators, as to which you entertain hopes of success?—I have had several submitted to me, but one in particular, that I have paid a good deal of attention to (I am speaking of an insulator); and I think it bids fair to be a good insulator.

351. Upon which you have experimented?—Yes.

352. (*Chairman.*) Whose material is that?—Leonard Wray's. The experiments we have made upon it give us great hopes of getting an excellent insulator.

353. Do you think it will be durable?—That is the point which has to be considered. It is a combination of substances.

354. Which are durable in themselves?—Yes; at present they seem to be durable; they combine very well, and the insulation is very good.

355. (*Mr. Stuart Wortley.*) What cables are you engaged in laying at present, if any?—I am about the Ostend, and a portion of the Dover and Calais. I do not recollect any other at the present time.

356. I believe no difficulty has occurred in laying the Holland cable?—We had a fault.

357. What was the nature of that fault?—It was of a very serious nature. A piece of iron had been driven into the core, forming a contact between the copper wire and the external iron wires.

358. Had that been wilfully or accidentally done in your opinion?—From all appearances it had been wilfully done during the time of paying out.

359. How soon was it discovered?—It was discovered in the middle of the night, midway in the laying.

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360. Immediately on the immersion of the cable?—Yes, the strong contact showing that the fault must have been very dead earth or that it was metallic contact, and it turned out to be metallic contact.

361. Was it a nail?—Yes, a small flat nail driven in and broken off so that there was nothing perceptible through the external wires.

362. With that exception you had no difficulty?—Not the slightest.

363. You found no difficulty attributable to permanent causes or natural causes other than those which must be the condition in every case?—None.

364. I believe the Holland cable is laid in light depths?—Moderate depths.

365. (*Chairman.*) Have you considered the general causes of failure in the different submarine lines that have been laid?—I have not had an opportunity of seeing the state of the cables that have failed.

366. Have you formed any opinion upon the causes of the failures?—I have formed a general opinion that the cables that have failed have been of too slight a character.

367. (*Mr. Stuart Wortley.*) Could you furnish the committee, or must they apply to other quarters, with information as to the number of words those various cables that you have laid are capable of transmitting per minute?—I think it would be better for you to apply to the different parties who have been working them; our connection with them ceases when they are once laid; we have no opportunity of judging.

368. There is only one deep sea cable amongst them; the Malta and Sicily is the deepest, is it not?—There is one deeper than that; the Newfoundland is deeper, between two and three hundred fathoms; and the Sardinia and Corsica; that is one of the early lines laid.

369. What is the maximum depth of that?—About 500 or 600 fathoms.

370. Has there at any time been any difficulty in carrying on or continuing the working of that?—None that I am aware of.

371. That forms part of the Malta line; does it not?—Yes; it originally went by way of Cagliari; that cable connects the island of Corsica with Sardinia; the Malta line is from Sardinia to Malta from Cagliari.

372. Did you lay the line from Corsica?—We manufactured it, and some of our staff laid it; it was laid by the contractors with the Mediterranean Company.

373. Does the Corsica line conclude the line between Genoa and Sardinia?—Yes; it goes from Genoa to Corsica, and thence to Sardinia.

374. And no farther?—No.

375. The Sardinia and Malta is a separate line that has now failed?—Yes.

376. With which you were not connected?—No.

377. What was the cause of the failure of the Bona line?—The cause of the last failure was having an insufficient quantity of cable. They arrived within five miles of the island of Galita, and I believe hung by the cable for five days.

378. Did they pay out more cable than they expected?—Yes; they did not calculate the distance to Bona sufficiently so as to reach Galita.

379. (*Mr. Saward.*) It was for deep water a very heavy cable, was not it?—Yes.

380. (*Mr. Stuart Wortley.*) Did they pay out much more than the actual mileage?—Considerably more.

381. Do you suppose that the cable lay in kinks?—No; it would not lie in kinks; it would lie in folds, and would be drawn into kinks as the cable was lifted afterwards.

382. What was the cause of the first failure?—The first failure arose from a run; they could not control the cable, and had to cut it.

383. (*Chairman.*) Was the cable too heavy for the depth?—The machinery was not sufficient to hold it.

384. (*Mr. Stuart Wortley.*) Both those failures were attributable to difficulties in the laying, and not to any defects in the cable?—Just so; the wires were all perfect in the last cable, and we received a message from Galita requesting us to send out more cable to enable them to reach the island in a few days, but it was not likely that we could do so.

385. (*Chairman.*) That cable was laid in a depth of 500 fathoms? if you were to design a cable to be laid in that depth, should you design a different class of cable? do you consider that the weight of the cable in the water should not exceed half its breaking strain in the greatest depth?—Clearly we should design a cable that could be laid with comparatively small loss of slack, and at the same time would be capable of bearing such a strain as to keep it in hand without injuring its form, and consequently the electrical conditions of it.

386. (*Mr. Stuart Wortley.*) Speaking abstractedly are you of opinion that a cable should be light in proportion to the depth in which it is laid?—I think you may err on the side of lightness; you may have a cable too light; there are other properties than the mere sinking of the cable in a specific time to be considered.

387. (*Chairman.*) How do you mean that you can have a cable too light, what would be the difficulties that you would encounter with very light cables?—I think the difficulties are many; first of all the cable is liable to a certain amount of injury or accident merely from its voyage out, from the rocking and rolling of the ship; I think a cable ought to have a certain consistency so as to prevent accidents of that kind, and there are the injuries which it is subject to in a variety of ways during the process of running out; it should also be of such a form that it will run out without the chance of kinking or overrunning itself or anything of that kind.

388. (*Mr. Stuart Wortley.*) The lighter the cable the less the difficulty in paying it out. Is not that the case?—That does not at all follow; we find less difficulty in paying out a comparatively stiff cable than a very light one.

389. (*Chairman.*) Supposing a cable were so light that you were able to pay it out like a log line with men's hands, what effect would that have?—I think the effect of that would be that you would lose your cable and your hands too.

390. How do you mean that you would lose your cable?—I do not think you could regulate its running into the sea by that mode; you could not control it in any way.

391. (*Mr. Stuart Wortley.*) Do you mean that it would not sink fast enough?—It would not sink fast enough; but I think there is no regularity in that system of running out by hand; you cannot keep a regular uniform strain upon it.

392. (*Mr. Saward.*) Is it not also very important that cables should be perfectly coiled on board ship?—I think the great secret of the successful laying of a cable is its being properly coiled.

393. Would not a light cable be less easy to keep in its proper place in the hold than a moderately heavy cable?—Clearly.

394. (*Mr. Stuart Wortley.*) Within certain limits, probably you would be of opinion that the deeper the water the lighter the cable should be?—Within certain limits, but not to excess.

395. (*Chairman.*) Is there a proportion between the specific gravity of the cable and the depth to which it is to be laid, which proportion has been found the best for paying out?—Yes.

396. (*Mr. Saward.*) Is not the problem to be solved with respect to a deep sea cable to adapt the specific gravity to the depth?—Yes; the mechanical construction for its coiling and running out clear.

397. (*Mr. Stuart Wortley.*) Do you think that No. 9, which we understand is the Gibraltar cable, could be safely laid in deep water?—Yes. I think it is an excellent cable; it is specified with steel wire, and I think it is as good a cable for deep sea purposes as at present I am aware of. I know of no cable that

I would rather take the risk of laying than a cable of that construction.

398. Would you be afraid of one still lighter in very deep water; take the Atlantic for example?—I do not say that I should not.

399. We understand that there is a uniform depth of about 2,000 fathoms for a distance of about 900 miles; are not you of opinion that you might lay a lighter cable at those great and uniform depths?—I am not prepared to say that a lighter cable could not be laid, but I would prefer something stronger, and with more solidity.

400. (*Chairman.*) In using the word "light," you use it as compared to strength. What Mr. Stuart Wortley means, I presume, is, if you had a cable that was lighter than that, but as strong, would you have any objection to lay it?—There is rather more than that in it; I must have consistency; I may say, having regard to the internal insulating material and the copper, as to what effect it would have in either collapsing or stretching. I think you must keep a certain solidity about the whole thing. It is not altogether a question of specific gravity; that is no doubt a very great agent in the matter, but there are other things to be considered in a deep sea cable.

401. (*Mr. Saward.*) Taking this Gibraltar cable, if you were the contractor for an Atlantic cable, would you use it for the purpose?—It is the best I have yet seen; subject to further experience I know, at present, of no better cable than that.

402. (*Mr. Stuart Wortley.*) Is it for the sake of the outward protection of the cable that you require it to be of that weight, or of a considerable weight?—I think it should be a certain weight for the outer protection of the cable. I have seen instances in which cables have been very much injured by the ocean. I think it was one of the faults of the Atlantic cable that it was hardly solid enough; I think it was too soft in its nature; it would not stand a pinch; you want a certain solidity to stand the pinch or pressure that all cables are subject to more or less.

403. Do not you think by strengthening the conductors and the internal core, and making that stronger, that you could compensate for the want of external weight and rigidity?—I do not think that any strain should be put on the internal core or conductors. I think there should be no more strain on them than can possibly be avoided.

404. (*Chairman.*) Have you seen Allan's cables?—I have seen a variety of cables of Allan's.

405. Do you object to his principle of putting the strength of the cable in the centre?—I have seen many of his cables. I have seen one with a rod of iron in the centre; I think in that form it certainly will not do.

406. Do you think the strength of the cable is more necessary on its outside than inside?—If you can confine it to a certain amount of straining.

407. As a general principle do you agree with Mr. Gisborne's opinion, that the rule to be followed in designing a deep sea cable, as to its covering, should be that the depth at which it is to be laid shall be less than half the breaking strain of the cable, and that at half the breaking strain the elongation shall be less than one per cent?—I think that is a very good rule.

408. And a safe rule?—And a safe rule. That elongation of one per cent. I assume has been derived from experiments; if you can attain that I think you have got the proper thing.

409. This Gibraltar cable, which in your opinion is a good one, you are aware elongates only ·75 per cent. at a depth which would represent 2,700 fathoms?—Yes.

410. And that you consider is a safe principle for a designer of submarine cables to follow in designing the outer covering?—I think so.

411. Have you paid much attention to the paying out apparatus?—Yes.

412. Have you any particular views as to what principle the paying out apparatus should be constructed upon?—We use our own apparatus.

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413. (*Mr. Saward.*) I believe a gentleman of the name of Clifford, in your establishment, has given a good deal of attention to that matter lately?—Yes.

414. (*Mr. Stuart Wortley.*) Do you know the specific gravity of the Gibraltar cable compared with the Atlantic cable?—I think about half.

(*Mr. Gisborne.*) The Gibraltar cable is 25½ cwt. in the air, and 13¼ in the water; the specific gravity of the Atlantic cable is 21¾ cwt. in the air, and 16½ in the water.

415. (*Mr. Stuart Wortley.*) Then the specific gravity of the Atlantic cable is lighter in the air, and heavier in the water, than the Gibraltar cable?

(*Mr. Gisborne.*) Yes; it is 16·30 cwt., in the water against 13·11. It is 3¼ cwt. heavier. I can put it in this way; half the breaking strain of the Atlantic cable represents a depth of 2,489 fathoms, and half the breaking strain of the Gibraltar cable represents about 5,000 fathoms. I am giving the mean of a number of experiments, so that the Gibraltar cable can be laid in nearly double the depth of water that the Atlantic cable can with the same strain.

416. (*Mr. Stuart Wortley to Mr. Glass.*) The result is without diminishing the bulk or the rigidity, or those qualities which you think essential to the satisfactory paying out of the cable you may reduce the specific gravity to a very considerable extent?—Yes.

417. And that you think an unqualified advantage?—I do.

(*Mr. Siemens.*) Expressing it in specific weight, the specific weight of this cable No. 9 is 2·1, and the specific weight of the Atlantic cable is 4.

(*Mr. Gisborne.*) The Atlantic cable would run as much risk in being in 1,250 fathoms as the Gibraltar cable would in 2,500, it comes to that on the question of elongation, and the question of strain.

418. (*Mr. Stuart Wortley to Mr. Glass.*) Was not the Atlantic cable lying at your works in the month of June 1857?—I believe it was.

419. You have spoken of the effect of heat upon cables. Do you think that that cable was exposed to any risk from heat on that occasion?—I think that the cable suffered probably, whether from heat I cannot say.

420. Have you any recollection of the circumstances under which it laid there?—I have a recollection that at that particular season we had three or four of the warmest days almost ever known in this country, and we found that the sun had acted upon the core prejudicially.

421. In what way did it act upon it?—In softening the gutta percha.

422. (*Mr. Saward.*) Would not that have a tendency to get the conductors out of the centre?—Yes.

423. (*Professor Wheatstone.*) Did not you cut a good many miles out in consequence of finding that defect?—Yes; there was an awning over it at that time, we were all deceived, we had gutta percha against our factory walls, which was not acted upon at all; it was from the radiation.

424. (*Mr. Stuart Wortley.*) Do you think that may have impaired the efficiency of the cable to some extent?—I think it is possible it may, but everything that showed any defect was cut out at the time.

425. May not there have been a considerable portion very close which would not show the defect at that time?—Just so; I believe there was a very considerable portion very close when it came to our works; that we had proof of by cutting out pieces that never were submitted to the heat of the sun at all.

426. Assuming injury to have arisen from those causes that may be avoided for the future?—Clearly.

Adjourned to Thursday next at Two o'clock.

Thursday, 8th December 1859.

PRESENT :

Captain Douglas Galton.
Professor Wheatstone.
Mr. Saward.

Captain DOUGLAS GALTON in the Chair.

Capt. J. Kell.

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Captain John Kell examined.

427. (*Chairman.*) You are in the merchant service, I believe?—I was.

428. Since when have you been connected with the laying of submarine telegraphic cables?—Since 1854.

429. What cables have you laid?—I was at the laying of the Spezzia cable, and I was at the laying of the other three cables afterwards laid by the Mediterranean Company.

430. Did you lay the cable from Spezzia to Corsica?—I was at the laying of that cable, and of the cable from Cape Spartivento to Bona.

431. Were not they laid in 1855?—Yes; they failed.

432. From what cause?—The first failed in consequence of taking sailing vessels to be towed by steamers. There is not sufficient control over a sailing vessels when in tow of a steamer, but we could not obtain a steamship large enough to carry the cable; it being 8 tons to the mile, and weighing 1,260 tons.

433. Was not that a very heavy cable?—It was the same as the original Mediterranean cable.

434. Was that Mr. Brett's cable?—It was.

435. Did not the cable laid from Cape Spartivento to Galita fail?—Yes; that cable fell short. When we got within about nine miles of land we had no more cable.

436. (*Mr. Saward.*) Was not it intended to have laid that cable to Bona?—It was.

437. (*Chairman.*) Was that cable carried out in a sailing vessel?—No, a screw steamer; the Dutchman.

438. In what way does the laying of a cable, by means of a sailing vessel towed by a steamer, act inconveniently?—You cannot regulate her speed if you have a head wind, which we had.

439. You cannot check the speed when it is required to be checked?—No; nor can you keep a straight course. There is not steerage way enough in a steamer to keep a direct course when meeting the wind. We were steering south and the wind was south, consequently she "shore" out of the direct course.

440. Would you not pay out more slack in that case?—Yes; it is impossible to avoid a zigzag course under such circumstances.

441. What is the greatest depth between Cape Spartivento and Bona?—1,500 fathoms is the estimated depth. It has been sounded by Frenchmen, and I have no reliance on their soundings. I think there are many holes very much deeper. I am quite of opinion that there are holes between Cape Spartivento and Galita nearly 3,000 fathoms, if not more.

442. How do you estimate that?—From the unevenness of the bottom, and the rapidity with which the cable ran out when it came off the known shelves, or came from shallow water into deep. I think the French charts do not represent the inequalities to be so bad as they really are.

443. Comparing the soundings which we have had taken between Gibraltar and Malta, they are found almost identical with the soundings taken by the French where they cross?—That is near the shore.

444. Was not the next cable that you laid between Cape Spartivento and Bona?—Between Spartivento and Galita. It ought to have gone to Bona, but we had not cable enough.

445. Were all those cables of the same construction?—No; the last cable that was short was a three-wire cable.

446. (*Mr. Saward.*) What weight per mile?—About four tons a mile or rather over four tons.

447. (*Chairman.*) The other two cables weighed how much?—Eight tons.

448. How many wires?—Six conducting wires.

449. (*Mr. Saward.*) Was there any hitch in the laying of those cables with the exception of one being too short?—Yes; the first time that we went into deep water it parted, or we broke it rather, and then I underran the cable for twenty miles, and spliced on with what I had in the hold, and went across to Galita. On going across I was obliged to lay it very slack, and consequently we were short of cable and could not reach the island.

450. With the exception of that cable being too short, would not it have been successfully laid, although a heavy cable?—Yes, if we had had ten miles more it would have been successfully laid to Galita.

451. (*Chairman.*) You got over the deep water part successfully, did not you?—Yes, and got into 400 fathoms; we were so near Galita as that.

452. Do you think that there is any practical difficulty in laying, even a heavy cable, in water of that depth?—No, nothing, even in that place; but it must be laid very slack on account of the unevenness of the bottom. Wherever the ground is uneven it requires a very large percentage of cable in slack.

453. Have you any knowledge of how the cable has worked since it has been laid?—Neither of those cables are working, but the cable from Spezzia to the north part of Corsica is, I am told, working better and better every year. It is perfectly faultless as I have heard lately.

454. In what depth of water is that cable laid?—Under 400 fathoms I think.

455. Have not you been connected with the Atlantic Telegraph Company since its formation?—Yes, since 1857.

456. What duty did you undertake in connexion with that company at first?—The coiling of the cables in the holds and superintending their management during the paying out of the coils.

457. Had you the superintendence of the breaks?—No, I attended to the coils exclusively.

458. Were there any difficulties in uncoiling that cable?—It uncoiled itself. I had men ready in case a kink or an accident should happen as it came out of the hold. The Atlantic cable did not require men to touch it scarcely, but I thought it prudent to have them always ready to lower the rings, attend lights, or do what might be required.

459. Do you attribute the fact that the Atlantic cable did not require any handling to the way in which it was coiled or to the nature of the cable?—Certainly to the nature of the cable and not to the way it was coiled; the coating put upon it had much to do with its coming out so well.

460. What coating was used?—Stockholm tar, pitch, beeswax, and a little Russian tallow; it was very thickly coated with this composition and stuck to the tier just enough to prevent its jumping or altering its position before it was required to do so; this made it come nicely out.

461. Is it an advantage that a coiled cable should stick to a certain extent?—It was in that case, because the cable was very pliable.

462. Is a pliable cable more or less safely uncoiled than a stiff one?—There is a limit to that. I do not think that a strand-cable, if it had nothing upon it at

all and was perfectly bright, would uncoil as safely as a cable without strands, that is to say, if the wires were solid instead of being strands.

463. You think it would uncoil better if the strands were solid?—Certainly; there is more stiffness, we do not want it to be too pliable.

464. Which of those samples of cables would you prefer for uncoiling?—That. (*No. 1 deep sea Gibraltar cable.*)

465. What is your opinion of No. 2 deep sea Gibraltar cable, the wire covered with hemp?—I have never handled these cables. I should better like to see them uncoiled before I gave an opinion. I have no doubt this cable with the steel-wire inside would uncoil very well, but that is mere opinion. I have not seen it in practice. Steel-wire cables never coil so well as iron cables, they always have a spring in them.

466. Would the covering of hemp affect that at all?—No, I do not think it would. I found with the steel cable that we had from the Atlantic, when it came to be coiled more than once, and left in the same position for any time, it got "set," which we had the greatest difficulty in getting out so as to make it coil again; it seemed to be all alive, and would not lay even in the coil.

467. Was not the Atlantic cable coiled and uncoiled twice?—A great many times.

468. Do you consider that it was injured by that?—I do.

469. In what way?—The strand cable, in consequence of being so pliable, had always to be unlaid when the strands were separated; it subjected the cable to great injury from anything that came near it; and in some instances, when it was coiled, it threw a turn into one part and threw it out of another.

470. When you speak of a strand cable, are you contemplating any other form of outer covering for the cable?—A solid wire similar to these would not have the same tendency.

471. (*Mr. Saward.*) In a strand cable would not there be a tendency for the thin wires of which each strand is necessarily composed, if a breakage occurred, to stick into gutta-percha?—Yes; I found that to be the case in handling the cable at Newfoundland this year. If the wire was broken or chafed in any way it turned round and stuck into the gutta-percha. I have a specimen, which has not yet arrived where that has been the case. In getting up the cable the strand wire has broken, turned over, and gone right through the gutta-percha as sharp as a needle, and that will be the case especially when it becomes a little rusty.

472. (*Chairman.*) As you have had considerable experience of telegraphic cables, allow me to ask you what you would think of one like that made under Hall's and Wells' patent, assuming that it is made of large wire if necessary?—I do not think it would do at all with large wires, for this reason, if it got a bend you would never get it out. In a cable like this specimen it might do, but in a large cable a bend would never come out, and there would be risks of its getting through the machinery.

473. You think it would be quite impossible to work with a cable of that construction?—I think so from what I see of it. I only speak from the feel of the thing in my hand as I move it.

474. You were on board the "Niagara," were you not, when the first failure happened?—Yes.

475. Will you shortly describe the events that occurred from the starting on the 5th of August until the accident resulting in the loss of 380 or 400 miles of wire?—We commenced with laying the shore-end; owing to the grooves of the wheel being too small, the wire flew off, fouled the machinery and broke; we went back again, fished it up, spliced on to what remained in the ship, and went on again; and we went on then successfully till we got to the end of 380 miles, and then the cable parted in the same way, the small cable flew out of the grooves of the wheel, precisely the same as the large one did; it got round the shaft, and, before there was time to get the ship stopped, it parted.

476. (*Mr. Saward.*) Do you think the loss of the 380 miles of cable was attributable to deficiencies in the machinery?—Yes, I do; if there had been protection to the machinery it could not have got out.

477. (*Chairman.*) The loss did not arise from any fault of the cable itself?—No, not from any fault of the cable certainly.

478. Must there not always be a considerable difficulty in stopping a ship if any accident does take place to a cable?—Yes, always, particularly a screw steamer.

479. Is it impossible in uncoiling a cable to prevent kinks passing into the machinery by watchfulness?—In laying down the Atlantic cable from the Niagara we never had any kink; I do not think there was one in the first instance. I think if the cable is carefully coiled and got out, you have no business with kinks if it is coiled from overhead instead of allowing it to be capsized over the men's heads every turn.

480. (*Mr. Saward.*) Was not the ship in which you were engaged in laying the Atlantic cable, a screw which you have previously objected to?—Yes.

481. (*Chairman.*) After that part of the Atlantic cable was lost, did not the ship return to Keyham?—Yes, and then we uncoiled the cables into the tanks.

482. In what condition was the cable when it was uncoiled?—When it was first uncoiled it did not look so bad, but when it was re-coiled in the tanks and put on board ship it was certainly very much worse than when we first had it.

483. In what way was it worse?—In the way I have described; in many parts the lay of the cable was made uneven by being coiled so many times, and by being put round wheels of such small diameter.

484. Do you think the Atlantic cable was more likely to be injured than other cables by the coiling and uncoiling?—I am of opinion if the Atlantic cable had been made similar to this Gibraltar cable it would not have been so much injured.

485. Were the cables at the laying of which you were present in the Mediterranean coiled and uncoiled more than twice?—A piece of the large Mediterranean cable I think I coiled and re-coiled fourteen times.

486. Was that cable much injured by the coiling and re-coiling?—Yes, it was a heavy cable; an eight ton cable.

487. Will you describe the various events that took place up to the successful laying of the Atlantic cable from the starting?—When we first spliced on and went on, by accident the cable parted after we had got about two or three miles, and we returned and spliced on again.

488. To what do you attribute that accident?—It is scarcely possible to account for it; it slipped off the wheels in that instance, but I think it was a jump.

489. Do you think there was any fault in the machinery?—There was no fault in the machinery. We do not know how these things may happen. A man by pure accident might put his foot upon the cable as it was coming out of the coil, and by that means bring the cable taut; in that case it would spring forward and jump out of the grooves of the paying-out machinery.

490. (*Mr. Saward.*) Was not that on the 27th of June?—I think it was. I have not the dates. That was after the first failure, or rather after we had laid down about 140 miles each. In that case, the cable parted close to the "Agamemnon's" stern. I was not on board the "Agamemnon;" this is the second time that I alluded to.

491. This was the first accident of which you have knowledge?—Yes.

492. (*Chairman.*) Was it then re-spliced?—We returned and spliced the cable again, and went away

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and successfully laid it to the end without the slightest accident of any kind, either a kink, or otherwise.

493. Were the signals always maintained through the cable during the whole time of paying out?—I think three times the electric communication was stopped; one time for nearly 40 minutes (I cannot be exact to a few minutes) altogether, and we were in great doubts about it.

494. And then the communication was restored?—It restored itself.

495. (*Professor Wheatstone.*) To what cause do you attribute the stoppage of the signals on that occasion?—Not being an electrician I cannot answer that sufficiently.

496. (*Chairman.*) You cannot account for these stoppages in any way?—No, I cannot; there was nothing in the paying out from our ship, and I am sure, from the information I have had from the other ship, there was nothing to which you could attribute those stoppages.

497. Did you ever observe in those places where you say the outer covering was very much opened any flaws in the gutta-percha or injury to it?—No, I never could see the gutta-percha.

498. Had the cable been exposed to much heat during the coiling and uncoiling?—I believe it was at Greenwich. I know it was exposed to great heat in consequence of being coiled in a place where the heat could get to it and where it had no covering.

499. Do you think that that injured the gutta-percha?—Yes, in fact I know it did.

500. What was the temperature of the hold of the "Niagara" in which the cable was coiled?—About 80°. I mean when the steam was up and when the holds were filled.

501. Do you think that that temperature was injurious to the gutta-percha?—No, I do not.

502. Are you aware what temperature is injurious to gutta-percha?—I am not aware. I have never tried it. We have been splicing the cables in 90° and upwards, and then the gutta-percha was not at all injured.

503. Was it soft?—No, if submitted to a great pressure it would give; it was softer than in cold weather, but not what I consider soft.

504. You have said that the cable was covered with tar; how was that tar put on?—It passed through iron tanks filled with the composition I have named, and kept hot by gas.

505. Do you think that would have any effect in injuring the gutta-percha?—Without great care it was possible.

506. Do you believe that the cable was injured from that cause?—Part of it.

507. (*Mr. Saward.*) I believe you called the attention of the company once or twice to the possibility of injury by that cause?—I did. One part of the cable we found defective when it was stripped.

508. (*Chairman.*) Do you mean that the cable had become injured very much from some process?—Yes, it must have been heat of some kind because the copper wire was partly visible; it was not quite through for the gutta-percha will allow of being very thin, you could see it through the gutta-percha.

509. Were those defective pieces afterwards cut out?—Yes.

510. (*Mr. Saward.*) Do you attribute that sort of injury, namely, the want of centring, to any fault on the part of the Gutta Percha Company?—Certainly not; it could not have come out of the gutta-percha, because it had been examined so many times and then put in water at the cable works; it would be sure to have shown itself there.

511. Are you aware that in some specimens of the Atlantic cable which have been recently discovered the copper conductor is quite away from the centre, and showing itself on the outer circumference of the gutta-percha?—I have never seen a copper wire quite outside of the gutta-percha, but so close to it that you might say it was outside; you could see every turn right along.

512. (*Chairman.*) Would it have been possible to have examined the cable sufficiently to detect every one of such faults which might occur?—No; it was not possible, because there was a great deal of tar upon the outer part of the cable; it would have taken a year, with a good many eyes, to have examined it minutely.

513. Would it have been necessary to have taken off the whole of the tar?—Yes; we could not have done it without.

514. (*Mr. Saward.*) Would it not have been possible in the first instance to have prevented such a state of things arising?—Certainly, by allowing more time.

515. (*Chairman.*) Do you think, besides the faults which you saw of the nature you have described, it is possible that other faults may have existed in the cable at the time it was laid?—Yes; quite possible.

516. Have you carefully considered the question of laying deep sea cables?—I have.

517. Are you of opinion that there is any difficulty in submerging a cable in water of great depth, provided it is properly constructed, 3,000 fathoms for example?—There is no difficulty whatever if you have a knowledge of the bottom; the only difficulty that arises in my mind is from not knowing what the bottom is composed of. If you could only ascertain its outline or unevenness, so as to have a true representation of the bottom, then it would be quite practicable to lay a cable in, say, a depth of three miles.

518. You would recommend very careful soundings to be taken of every part of the bottom before the cable was laid?—Yes.

519. (*Mr. Saward.*) Do you think the Atlantic from Ireland to Newfoundland has been sufficiently sounded for the purpose of laying a submarine cable?—I do not; when there are spaces of ten or twenty miles between the soundings, the lead might go between those precipices which exist no doubt at the bottom of the sea, as they do on land.

520. (*Chairman.*) Do you think it would be possible to get such a map of the bottom as you describe?—I think soundings might be taken at each five miles; that would be a good guide.

521. Between soundings five miles apart there might be a mountain, I presume?—Yes; but the chances of ascertaining the character of the bottom would be double what they are at present.

522. Have you considered the question of the weight of the cable and the specific gravity?—I have not gone into that question of the weight of the cable and the specific gravity.

523. (*Mr. Saward.*) Is it your opinion that the specific gravity should be carefully adapted to the depth?—Of course it must be.

524. What would you say of that cable, No. 9, or one similarly constructed, as to its suitability for laying in the Atlantic?—I should say that you might lay that cable, from what little I have seen of it, in any depth of water. It would be more suitable than this (No. 12). This (No. 9) would not descend too quickly, there never would be much strain upon it; it would be quite light and go at a much sharper angle.

525. (*Chairman.*) Do you consider that there is much danger from the motion of the ship in laying a cable, or does the angle at which the cable goes out compensate for that to some extent?—The lighter the cable is the less it is likely to be injured; the more the cable lifts up and down over the stern, the more danger there is; the longer the angle at which you can get the cable the better; the ship's pendulous motion is scarcely felt at an angle, say from 10° to 20°.

526. You think that a cable should be paid out at an angle of 10°?—Yes.

527. Must the speed of the ship be kept up to a certain extent to allow of it?—Yes; you can pay the cable out at six miles, and let the ship go through the water at five miles, if you choose to do so.

528. By paying out a fifth of slack?—Yes; but 20 per cent. is too much.

529. (*Mr. Saward.*) In paying out slack, if I understand you, you would like, if you had sufficient soundings, to adapt it to the contour of the bottom as far as you could?—Certainly; in some places where the bottom was uneven, as in many parts of Newfoundland, I would pay out 30 or 40 per cent. of slack till I got over that difficulty. I would allow plenty of slack for the cable to fall into those cavities at the bottom.

530. (*Chairman.*) If you have a great quantity of slack, is not there more difficulty in picking up the cable again?—I do not know. I have never felt it. The only difficulty would arise in that case from the kinking.

531. (*Mr. Saward.*) You have picked up cable for the Atlantic Telegraph Company, both on the Valentia and the Newfoundland sides, have you not?—Yes.

532. On the Valentia side the cable was laid very slack, was not it?—Yes, very slack.

533. Was the cable that you picked up on that side drawn into kinks?—It kinked in coming up, as cables are very apt to do, when they are just lifted from the bottom; we had only one kink.

534. The portion you did get up was not injured, and has since been sold and used, I believe?—All the cables, whether at Newfoundland or Valentia, that I have picked up of the Atlantic telegraph, where it laid over sand, have been perfect, as perfect as when made, with the exception of a little rust; wherever it has laid over rocks the outer casing has been bad.

535. On the Newfoundland side, I believe, you picked the cable up without difficulty in a depth of 170 fathoms?—Yes, part of it.

536. (*Chairman.*) What is the greatest depth in which you have picked up cable?—I think that is about the greatest depth. I do not remember any greater.

537. Do you think it is possible to pick up a cable at a greater depth?—It depends upon the slack entirely. If the cable is laid very slack you might get it up at 300 fathoms, not a heavy cable; but such a cable as this (*deep sea No. 2 Gibraltar cable*) might be got up in 300 fathoms. If it was near the end, you would get this up in 300 fathoms on a soft bottom.

538. If you did not know exactly where the cable was, should you be able to find it in 200 fathoms of water by grappling for it?—If the bottom was soft you would. It requires two grapnels for getting up cables; with a soft bottom one should be made very sharp, with the arms very long, so that they will plough into the soft ground; such grapnels would not do with a hard bottom. When you get hold of the cable and raise it two fathoms from the bottom,—any person of experience can tell whether they have got hold of the cable,—it springs, and then with a heavy grapnel you will have no difficulty in lifting it.

539. At what depth do you think a cable would be safe from being picked up; would it be safe to lay a cable from Falmouth to Gibraltar in a depth of from 200 to 300 fathoms?—If it is a heavy cable such as this (*No. 2 shore-end*) it would be perfectly safe in 300 fathoms, and this would be still safer.

540. Take that No. 1 deep sea, in what depth could you pick it up?—I think from 250 to 300 fathoms, that might be lifted very easily if it will bear its own strain; I should say 300 fathoms.

541. Not more than 300 fathoms?—No.

542. You think if that cable were laid in 300 fathoms it would be quite safe from being picked up?—Quite safe. It might be picked up at 300 fathoms. I should not like to attempt it, but you might lift it very much further. When you pick up a cable and it takes the grapnel, there is quite twice its own weight to lift.

543. In what depth would that cable be safe from being injured by attempts at picking it up?—I should say 400 fathoms.

544. Do you mean that it would not be possible either to get hold of it or break it in that depth?—It would be impossible to tell whether you had got hold of it or not in 400 fathoms.

545. Do you think the shore-ends would be safe in less than that?—I think those shore-ends would be safe in 100 fathoms. I am almost certain you would not be able to get that up at all.

546. Why not? would the weight be too great?—If it were to get a hold of the ground, you could not spring it up, you could not get enough of the slack of it to come up.

547. What would happen?—It would part; the cable would be sure to break.

548. In fact it would not be safe from being injured in 200 fathoms?—No; it would be quite possible to get an anchor or something underneath it.

549. And if anything got underneath the cable it would break?—Yes, of course it would; but it would be apt to break any chain-cable or anything of that sort that got hold of it first.

550. (*Mr. Saward.*) Would not such an attempt as that presume very accurate knowledge of the course in which the cable was laid, and also a good deal of skill on the part of those who were attempting to injure it?—Yes; I am of opinion that in 200 fathoms this could never be got up, even if it was got hold of by a vessel's anchor.

551. (*Chairman.*) It would be just as injurious to break a cable below as to pull it up to the surface and then cut it, would not it?—In order to break the cable underneath you must have something to break it with. You would have to put down 200 fathoms of chain-cable; with this sized cable I broke the steamer's windlass in lifting it even in ten fathoms.

552. You broke the windlass but not the cable?—No, not the cable; we got the cable up right enough. I hooked it first with a grapnel, and then with the anchor.

553. Should you have any difficulty in meeting with the cable if you knew that it was laid at certain places?—At 200 fathoms it would require a very long time; in the first place you must grapple slowly, and if you grapple at 200 fathoms you would want 500 fathoms of line to keep out a proper line. Supposing this pen to be 200 fathoms, you would want twice its own length and a-half, that would be 500 fathoms, as you would want 20 fathoms to lie on the ground. If you grapple properly, a line would have no chance with a heavy cable, you must grapple with chain and you are never so sensitive with a chain as with a rope.

554. Should you consider the cable at all safe in 70 or 80 fathoms?—This heavy cable (*No. 1*) in anything under 100 fathoms could be easily injured but it could not be got up if it was laid taut.

555. Is not it easier to get up a cable laid in soft ground than one laid in hard ground?—It is easier to grapple it, but not easier to get it up; it will come up easier off rocks if you can get hold of it, but on rocks it may be lying in such a way that you may grapple over and over a dozen times before you can get the grapnel to bite. I have often found it so on rocks.

556. Do you attach much importance to the quality of the soil at the bottom or the nature of the bottom?—Mud preserves the cables much better than sand; if it is hard white sand, it has this effect, the cables go half way through, and where it is lifted or where it is disturbed at all, all the pitch that you put on it is left in the sand, so that if from the action of the tide or any other cause it should be moved, or the bottom should alter its formation, the cable would become deteriorated directly; the harder the bottom is, from mud up to rock, the cable becomes more or less deteriorated.

557. Rock is the worst, I suppose?—Yes, but in mud the cable is preserved.

558. Have you lifted a large number of cables?—Yes, I have had a great deal to do with them after they have been laid down for some time.

559. (*Mr. Saward.*) Do you think there is any special reason which you as a nautical man can speak of, why a cable should not be successfully laid and permanently worked in deep water?—No, I do not see any reason whatever.

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560. Is not it a question of good business arrangements, a well considered cable, and care in the laying?—One of the great things is, that a machine should be taken out in deep water, and tested both for deep and shallow water; you want machinery with sufficient power to hold in deep water, and sensitive enough not to cause the cable to be laid too tightly in shallow water. There are many parts in shallow water where you want to lay the cable particularly slack; more so than in deep water.

561. Then if all the experience which has been gained is made proper use of, is there any real obstacle to the successful laying and permanent working, say of an Atlantic cable?—Not any that I can see.

562. Do you consider that the failure of the Atlantic cable has arisen from too great haste?—I do, and I objected to work being done in the night from the first.

563. And too little consideration?—I do attribute the failure to both causes. If there had been 50 miles of cable made at first in a particular way, taken out into deep water, properly laid and tested, say from one point to another where you have deep water, and a cable of another construction laid in the same way, then experience would have been arrived at.

564. (*Chairman.*) Between what places would you lay those experimental cables?—I think something like the straits of Gibraltar; I do not know thedepth.

565. That is only 400 fathoms?—It should be some convenient distance of about 50 miles where there was plenty of depth. There are places to be found on the chart with a little trouble. I think from Madeira some place might be found.

566. I understood you to say that you would have a great many soundings taken previously to laying a cable?—Yes; whenever the soundings were not very even, I should be inclined to think that there was a rocky bottom.

567. A depth of 600 fathoms would be safe, would it not?—That is a very good depth if you can get an even bottom.

568. Have you any knowledge of the south coast of Ireland, near Cape Clear and Valentia?—Yes.

569. What sort of ground is there there?—There is a shoal that lies out from Bantry Bay; you can take the south side of that patch with soft mud and ooze all along at a distance of 30 miles from land. I think you will find the soundings to be from 50 fathoms to 100 fathoms in an easterly direction towards Scilly.

570. (*Mr. Saward.*) I believe you have expressed an objection to the Atlantic cable being taken to Valentia again; will you explain on what grounds?—Because the bottom is so very shelvy; it is the same at the bottom there (I know from having taken up a cable) as it is on land; the slate rocks lie up to the westward, one layer after another, and the cable while it comes over the sands all right, comes over the top of one of those ridges, and the whole of the outer covering is chafed off by the rocks.

571. (*Chairman.*) Have you lifted cable there often?—Yes. These shoals run away out to the westward, it is impossible to know how far. I know that they do. I had that information from a great many deep sea fishermen, who told me that they never liked to fish upon those rocks because when their lines get in they lose them.

572. (*Mr. Saward.*) Are not you of opinion that some where off the coast of Kinsale would be a good place for a cable to be laid?—Pursuing a direct easterly course along the south coast of Ireland and then steering about north, a little way westward of Kinsale, you would have red sand and the clay strata when you get in and no rocks at all, it is all stiff clay. There is a little bay to the westward of Kinsale you will see by the chart; but at the Admiralty the other day they objected to that.

573. (*Chairman.*) Going into Kinsale?—Yes, they thought it could be too easily got at, and they asked me what place I would recommend, and I said the Skeligs, for this reason, that an enemy would have the greatest difficulty in getting at it. Ten miles from the Skeligs lighthouse you have 90 fathoms of water mud, and there you could put this big cable out of the way of an enemy.

574. Your proposal would be to take the cable for Gibraltar from near Valentia to the Skeligs and then lay it in deepish water all the way to Gibraltar?—Yes.

575. Looking at those soundings, what should you say of the coast of Portugal (*handing a chart to the witness*), should you say that we had information enough?—I have no idea. The Burlings that lie off there would induce you to suppose that there might be stones of a similar nature in the sea; where the rocks are jutting off the coast it is natural to suppose that there may be something of a similar nature in the sea.

576. Do you know anything of that coast?—Nothing beyond navigating it several times.

577. Is there a strong tide?—There is a strong current, especially round Cape Finisterre.

578. Is there much rise and fall of tide?—That I do not know. I never have been in any port except Gibraltar on that coast.

579. (*Mr. Saward.*) Is not there a strong current from the west sweeping round towards the south and east?—Yes; it goes right up in the direction towards Madeira. I have felt a strong south-west current set in from that coast.

580. (*Chairman.*) Coming out of the Bay of Biscay?—Yes, right round the coast of Cape Finisterre. I do not know anything of the soundings off that coast.

581. Have you ever taken any soundings anywhere?—Yes.

582. Have you ever taken soundings to ascertain the temperature of the bottom of the sea?—Yes.

583. In what depths?—I scarcely ever did it except for my own information in navigating ships. I have done it off the Cape of Good Hope; never beyond 100 fathoms; a 100-fathom line was what I generally used when practicable.

584. What temperature did you find there?—I have given away and lent for the guidance of others my charts and journals containing my memoranda connected with this subject. When I have been coming from the westward of the Cape of Good Hope, and have been sounding for the temperature at certain seasons of the year, it is always the same within a few miles.

585. Does the temperature vary at different seasons of the year?—Yes, the same as the Gulf stream.

586. Does it vary with the depth at all?—No; I used to run as near one latitude as possible,—about 39° 30′ I think it was,—that I used to run round the Cape of Good Hope.

587. Did you find the temperature of the sea vary with the latitude?—No; at the season when I arrived at the bank, having westerly winds, I could choose my own route in approaching the bank. I always ran down in one parallel as near as possible. I used to sound for the temperature of the water in that parallel and found it, at about the same dates, always the same. With respect to the strength of the currents that I have found in this track, I have set off the currents for different dates, and I found the current running stronger from the southward at one season than another; where the current set the strongest from the south, I had the coldest water.

588. In the same latitude you always found the same temperature at the same season?—Always. You are aware that there is a north-west current running very strong down on the western bank of the Cape of Good Hope, and I have set it down from time to time in one day's work so many miles, to find what drift she made in the 24 hours; how much she had been sent by the current, and I think it was about 45 miles a day.

Kemp House, East India Road,
Nov. 29, 1859.

DEAR SIR,

In compliance with your instructions of Sept. 15th I proceeded to Newfoundland, and on my arrival at the

company's station I lifted the cable as far towards the mouth of the harbour as was prudent, considering the state of the weather and the season. In doing this I found the external part of the cable, for a distance of about two miles from the beach, very much chafed and broken, showing clearly that it had laid over uneven stony ground. From thence to where I left the end there were several places in the same condition, but wherever it had laid on a muddy or soft bottom it was in a good state of preservation.

A little below Stock Cove, a distance of about ten miles from the station, whilst we were lifting the cable it parted, and from its appearance I am of opinion that it had laid over a large piece of rock, as the external wires were quite rotten, and I hoped when I first saw the end that it was a fault, but on a close examination I could not come to a positive conclusion.

I then grappled the cable about half a mile beyond the broken end, and put on the same test as before, but was disappointed to find that the galvanometer did not indicate any improvement.

I continued to lift the cable to the mouth of Bulls' Arm without finding anything further; I then laid the cable to the nearest shore and built a hut, where Mr. Saunders remained for four days to make the necessary tests and carry out Mr. Varley's instructions. He will on his return furnish you with a full report, as he has in his possession all the papers connected with the testing, and I cannot help saying that Mr. Saunders has been of great service to me in many respects whilst on this expedition. *Capt. J. Kell.* 8 Dec. 1859.

From my experience of Bay Bulls' Arm, and from all the information I could gather in reference to it, I do not think that it is a suitable place to land any cable, and I believe that for nine or ten miles out beyond where the end of the cable is at this time anchored, the bottom is uneven and studded with rocks, and I should strongly recommend that any new cable be carried to New Perlican or somewhere in the immediate neighbourhood, as you might I believe keep it on a soft bottom, and save at least 50 miles of cable.

With regard to the station at Newfoundland, I have agreed with a person to live in it and take care of it until next June, but should it be decided not to land any future cable there, I would recommend that the house be then removed, as the *materials* would be much more valuable to us than any sum we could get for the building where it now stands.

From my diary, a copy of which I enclose for your information, you will have the details of my proceedings since I left London.

I am, &c.
(Signed) JOHN KELL.

George Saward, Esq.,
Secretary, Atlantic Telegraph Co.,
London.

Mr. WILLOUGHBY SMITH examined. *Mr. W. Smith.*

589. (*Chairman*). In what way are you connected with the Gutta-Percha Company?—Foreman of the wire department.

590. Have you had considerable experience in the laying of submarine cables?—I have been out with several, but always as connected with the Gutta-Percha Works.

591. Were your duties to attend to the electrical working of the cable?—No; I went on behalf of the Gutta-Percha Company.

592. Have you turned your attention very much to the manufacture of gutta-percha?—Yes.

593. Do you consider it the best material for insulating copper wires?—I do.

594. Have you considered the character of india-rubber as an insulator?—Yes.

595. What in your opinion are the comparative merits or demerits of the two classes of insulators?—I have had one mile of india-rubber covered wire and compared it with a mile of gutta-percha covered wire of the same size; and in trying my experiments with the india-rubber core it broke down after a day's testing. I think I had not more than one day. I divided the core into five lengths, and each length broke down.

596. (*Mr. Saward.*) Was the same battery power used in each case?—Yes.

597. What amount?—504 pairs of plates, the ordinary sand battery.

598. Six-inch?—Four by three.

599. (*Chairman.*) What size was the wire?—16-inch copper wire, covered to No. 4 gauge.

600. Was Silver's wire covered by themselves as a model specimen?—Yes.

601. To what do you attribute its giving way?—I still have the pieces, and I intend to trace out those faults. Messrs. Siemens are trying some experiments at our works. I see that they have half a mile of the rubber wire in the canal, which I tested the other day. It seems pretty good, but that is only one testing.

602. (*Mr. Saward.*) Have not you been in the service of the Gutta-Percha Company from nearly its commencement?—Yes; I crossed the Channel with the first experimental cable.

603. Can you say whether the quality of the gutta-percha used now is as good as when it was first used for telegraphic purposes?—I have every reason to believe that it is.

604. Can you say whether the process of cleansing and preparing the gum so that it can be used for telegraphic purposes has been materially improved since the first arrangements of the company were made?—I believe that it is being improved every day.

605. (*Professor Wheatstone.*) Is there much variability in the composition of gutta-percha?—As in most other things, there are various qualities of it.

606. (*Chairman.*) Is not gutta-percha subject to a great deal of adulteration in the islands themselves, from which it is received?—Not that I am aware of, as regards material suitable for wire.

607. Assuming that there are different qualities of gutta-percha, is it easier to put on a thin coat of good gutta-percha than a thick coat of bad gutta percha?—No.

608. Whatever the quality of the gutta-percha is can it be put on in a thin coat as easily as in a thick one?—A very inferior gutta-percha could not be worked in our machines in thin layers. Any engineer or gentleman who has had experience in telegraphy or of seeing covered wires can tell in a moment whether we have put any inferior article into a cable; that is to say if we used inferior gutta-percha it would look so different after being manufactured a day or two that they could tell it in a moment.

609. Perhaps you have seen that wire before (*handing a specimen to the witness*). It is covered with 20 coats of gutta-percha is it not?—Ten of gutta-percha and ten of compound.

610. Could inferior gutta-percha be worked up in that?—No; we can put an inferior quality of gutta-percha covering if necessary.

611. With the same thickness of coating?—No.

612. What appearance would the gutta-percha of inferior quality have. Would it be more porous?—I can hardly describe what it would be. I know there are many persons come to the Gutta-Percha Works who will take up a piece of wire and say, is this the best gutta-percha?—It does not look like it.

613. (*Mr. Saward.*) Is not the best gutta-percha easily discerned by its smooth and shining appearance?—No, that does not follow. It is a curious circumstance, if you are covering two wires at a time, both of them coming out of the same die, one will be perfectly smooth and the other rough, the same materials covering both. This paper contains the result of some experiments Mr. Clarke is trying at our works (*handing in the same*).

614. (*Professor Wheatstone.*) In the process of manufacture are you always certain of preserving the central position of the wire?—We are not certain; it will vary.

615. How is the centering affected by the process of manufacture?—It will depend upon how the wire was covered, whether with two or three or only one coating.

616. Do you think that any of the failures of submarine telegraphs have arisen from the wire being out of the centre and approaching the external part?—I cannot see how it is possible.

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617. (*Mr. Saward.*) It would hardly be possible if more than one coat of gutta-percha were put on; if there is only one coat of gutta-percha it might be possible, but if there are two or three coverings of gutta-percha it would be hardly possible, would it, for the wire to get out of the centre?—I do not say that it is possible with one coating. The only possibility that I see with one coating is this, the wire passes through the machinery and you put on a large body at once; in going down the tank it may not be sufficiently cold, and when it goes over the pulley the wire would be liable to come out of the centre, but then the gutta-percha would be flattened on the surface.

618. So that the want of centering would be easily detected?—Yes.

619. (*Chairman.*) In this wire you see that there is considerable eccentricity (*handing a gutta-percha covered wire to the witness*)?—That has been what is termed badly centred in the first covering; the wire is double covered and out of the centre, that is the fault of the workman.

620. Have you any test by which you can detect that?—I am happy to say that I think we have got over that difficulty.

621. What test do you employ?—Only by the improvements we have made in our machinery.

622. Are there any means by which the person for whom you are covering the wire can detect eccentricities?—Yes, by cutting the ends of the coil.

623. Would the defect be the same throughout the core?—If the wire was out in one part it would be a hundred to one but you found it so through the coil.

624. You think that it cannot be eccentric in one place and not in another?—It is possible, but the chances are greatly against it.

625. Is there any practical objection to testing each layer of gutta-percha as it is put on under pressure in a pressure tank?—I have a great objection to testing gutta-percha cores under severe pressure.

626. For what reason?—My objection is to a certain extent founded upon the following experiment. I took a piece of wire and tested it with 504 pairs of plates and a very sensitive galvanometer. It was perfect. I then tested it with static electricity, and found it the same. I then placed it under pressure, and after remaining so for some time it still tested perfect with the 504 pairs, but not so well with the static.

627. Do you consider that water pressure does deteriorate gutta-percha?—The above experiment induces me to believe it does with static electricity to a certain extent.

628. Then how can gutta-percha be a safe insulator under the pressure to which it will be subjected at a depth of 2,000 to 3,000 fathoms?—Dry gutta-percha is a perfect insulator; and when the cable is manufactured it is covered with hemp saturated with an insulating fluid.

629. That cannot prevent the water getting into it?—I think so. I have seen some of the old Dover cable that has been down eight years, and the tar oozes out of the hemp when it is taken up.

630. (*Mr. Saward.*) Is there not another reason; in the case you put you test it with 504 pairs of plates; is that a test to which it would ever be subjected in the working of the cable?—That is more battery power than is ever used.

631. Is it not the case that the excessive force of the large battery power would find its way out of the cable under that pressure, whereas the normal working power used in practice would not have anything like a similar effect?—It is more liable to do so if it is kept under pressure for some time.

632. By keeping the core under pressure, and using it at the same time an enormous battery power, would not you be likely to bring about injurious consequences to the core; whereas the same core under the same pressure, worked by the ordinary battery power which would be used in sending messages through the cable, would not have the same injurious effect produced upon it. Do you agree to that?—I do.

633. (*Chairman.*) Then you are of opinion that a great pressure will cause water to enter into the pores of the gutta-percha?—Every experiment with static electricity would lead me to believe so to a minute degree.

634. Does not that tend to show that gutta-percha is not fitted for very deep sea telegraphy, unless you cover it with some impervious material?—In all the instances of testing under pressure, I have never known the core to be defective from bad insulation when tested with a voltaic battery.

635. The pressure that is proposed to be put on the Gibraltar cable at the works, is nothing to be compared with the pressure it will be subjected to when it gets to the bottom of the Bay of Biscay. The pressure at the bottom of the Bay of Biscay is about 7,000 lbs. per square inch, whereas the pressure at the gutta-percha works, in Reid's tank, is 1,000 lbs. per square inch?—Yes.

636. (*Mr. Saward.*) In the case of a cable completed and laid down, would not the pressure be distributed, not acting directly upon the gutta-percha, between the iron, the tube, in fact, in which it is enclosed, the hemp, and the gutta-percha; whereas, in the experiments you are now making, the pressure is applied direct to the skin of the gutta-percha?—Yes, and that is what I object to.

637. (*Chairman.*) Are not you aware that a cable covered with that wire cannot be laid in great depths?—No, I am not aware of the fact.

638. It is not capable of supporting its own weight, therefore you must have resort either to steel wire or to some other covering. If there is any flaw in the hemp covering, or any place which is not perfectly saturated with tar, through which the water could get to the gutta-percha, then the effect which you dread would at once take place, would not it?—That is to say, if the gutta-percha is exposed to this enormous pressure. No, I do not say so. It is said abroad that gutta-percha is porous, and that it absorbs moisture after it has been a long time under pressure. I do not say that. If there are any minute holes, my experiments led me to believe that the water had entered into those under pressure when we tested with static electricity. I found no difference with voltaic. I merely mention that circumstance.

639. (*Mr. Saward.*) In applying enormous pressure on the outside and enormous battery power to the inside, are you not creating two opposing forces which would have a tendency to act injuriously upon the substance which was under the influence of those forces whatever it might be?—Yes; the water is trying to get in and the battery power is trying to get out; they meet each other.

640. Those opposing forces combining to injure the gutta-percha?—Exactly.

641. (*Chairman.*) Will you look at this specimen (*handing a piece of gutta-percha core to the witness*). To what do you attribute those flaws?—These are what we term air holes caused by the air getting into the percha.

642. Is there any means of preventing those air holes?—We have to a certain extent got over them; but the best mode of getting over them is to have a very thin coating at a time, and then there is no room for the air to tell upon; but when you put on a thick coating at once, the percha is liable to these air holes.

643. (*Mr. Saward.*) Would not another preservative from that danger be to work your machinery at a slower pace?—I think not; the speed depends upon the size of the wire; with a large wire we are obliged to go slow; with a small one we cannot.

644. (*Chairman.*) Is there any means of detecting those air holes that you are aware of?—By careful examination or pressure.

645. By exhausting the air, and pressure?—Yes, I have no doubt that these have been found out in that way.

646. Those air holes were discovered by Mr. Newall?—That is under pressure.

647. He only discovered them after he had laid his

cable and he attributes his failure in part at least to those defects?—These air holes only extend to the first covering; that was one of the grounds why we commenced with the double covering.

648. Are those air holes in the outer coat?—Yes, but not in the inner one.

649. Is there any security that there are not similar defects in the inner coat?—It is fair to suppose if you have an air hole in a double covered wire in the first covering it is improbable that there will be another in the same place in the second covering.

650. (*Mr. Saward.*) And still less in a third?—Still less in a third.

651. (*Professor Wheatstone.*) In some specimens of manufactured gutta-percha, I observe a deterioration of a particular kind in the gutta-percha close in contact with the copper. If you cut a piece of gutta-percha and split it up so as to expose the copper you find the copper surrounded by a grey powder. Do you know the cause of that?—I do not think you will find that in submarine lines; that decay is generally noticed in gutta-percha used for subterranean lines.

652. (*Chairman.*) Does not gutta-percha undergo a certain amount of deterioration if it is exposed to air without any moisture?—Yes.

653. Does it undergo a chemical or a mechanical change?—I think it is chemical.

654. Does not it become brittle?—Yes.

655. Can it be worked up again for any useful purpose?—Not as an insulator; its property is destroyed as an insulator; but we find, in the compound that we are now using, that there is none of that decay going on between the copper and the gutta-percha,

656. (*Mr. Saward.*) Are you familiar with the invention of Professor Hughes, of a compound to be applied between the gutta-percha and the coat of the cable?—We have manufactured two miles for the Government experiments.

657. Have you formed any opinion as to its usefulness?—No, I should not like to hazard an opinion at *present; all I can say is that in testing for insulation it does not come up to one of many coats, as Mr.* Clark's paper will show.

658. Is the special piece to which Hughes's compound is applied also covered with many coats?—No, only double covered.

659. That would not be a fair comparison with Hughes's unless his were covered many times with gutta-percha in the same way, would it?—I think it is. The Professor in fact said that we might use an inferior article outside, because he was so positive it would be a better insulator, and I think the thicker you got the fluid the worse would become the insulation.

660. (*Chairman.*) Does laying on the gutta-percha in many coats materially increase the cost of the wire?—There would be the additional labour.

661. What addition would that make to the cost?—That I am not able to tell you.

662. (*Mr. Saward.*) Would it be considerable?—There would be the same amount of gutta-percha; there would be the labour; the additional cost would be in the labour.

663. The additional cost would be very considerable?—Yes.

664. (*Chairman.*) Do you think it would multiply the cost as much as 15 or 20 times?—I think not.

665. (*Mr. Saward.*) That would multiply the labour portion you think fifteen times?—The labour would be as ten to one compared with a single covering.

666. (*Chairman.*) Have you tried any other experiments besides those you have mentioned upon india-rubber?—I have made a compound of india-rubber and gutta-percha, and covered a specimen of the Gibraltar core with the same. I have no doubt you have seen the specimen. It is that with the black covering outside. It is a great protection to the core. I can illustrate the result of experiments made with our compounds by these curves. I have also some curves showing the testings of the core for the Red Sea cable. The copper strand was No. 11 gauge, covered with two coats of gutta-percha and two of compound. I had also a coil of same size double covered with gutta-percha only. The testings were taken from September to September, and this diagram shows the changes of temperature upon each testing (*producing a diagram. Plate No.* 1).

667. To what do you attribute this great difference?—To the increase of temperature: this shows the difference in September, depth of winter, height of summer, and then we fall to September again.

668. In the summer *the gutta*-percha is the worst?—Yes; as the temperature rises it affects the gutta-percha as an insulator.

669. Chatterton's compound also deteriorates, but to a less degree?—Yes.

670. Chatterton's compound is always better than the gutta-percha?—Yes; it is a better insulator.

671. What is it made of?—*It* is made *of* percha, Stockholm tar, and resin.

672. Has not resin an injurious effect upon gutta-percha?—No; not that I am aware of.

673. Has creosote?—Yes.

674. (*Mr. Saward.*) At what temperature does gutta-percha become plastic?—At about 120°.

675. What degree of plasticity does it obtain then; would it fall by its own weight?—No.

676. At what temperature would a piece of gutta-percha covered wire laid on a table or a stone fall down so as to get the wire out of the centre?—I should say about 130°. I placed six yards of the Atlantic cable in water at 110°, and found that after immersion for some time the wires had left the centre, but the same length of core *without* the outer wires, when similarly treated, the conductor had not left the centre.

677. That would be due to the strain on the outside of the coil?—Yes. This will show the experiment I tried in the last three days with 1,000 yards of core covered with pure compound (*producing another diagram. Plate No.* 2.)

678. (*Chairman.*) I think I gather from your evidence that you would prefer that a gutta-percha core should be covered with Chatterton's compound or some other compound instead of being placed in the water without any such covering?—I say of course it would improve the cable as one of the diagrams here will show.

679. I am referring to the capacity of the core for resisting water pressure?—Yes; but I do not wish to be understood as saying that without the compound you could force water through the gutta-percha.

680. Will not it be submitted to that pressure eventually?—I think not.

681. Do not you admit that air holes are injurious to a core?—Yes; but they would not be so injurious as submitting a core to a pressure, which might by an extreme possibility force the water into those air holes.

682. When a cable is put down at a depth of 1,500 to 2,000 fathoms, is it not necessarily exposed to great pressure?—I think the outside covering takes that pressure to a certain extent.

683. Do you think that the water will not penetrate the outer covering?—I do not think it will get through the gutta-percha.

684. It comes to this, that you think it desirable that there should be an impervious outer covering beyond the gutta-percha?—I think you cannot protect your gutta-percha too much.

685. (*Mr. Saward.*) Have not you been present at nearly all the successes or failures in the Mediterranean?—No; I was out at one success and one failure; the last two I was not out with.

686. On what occasions were you present?—When the one from Spezzia to Corsica was laid successfully, and the first failure, the one that was attempted to be laid from Cape Spartivento to Bona.

687. Are those the only two cables you have been out with personally?—I was with two in the Mediterranean, the Dover, the Howth, and the Port Patrick.

688. You were not in the Black Sea?—No.

689. You are not able to tell us any more than

Mr. W. Smith.
8 Dec. 1859.

Captain Kell has told us with regard to the failure of the Spartivento cable?—No; there was one remark of Captain Kell's that I could not understand; he said that he broke the windlass in heaving up the cable, but that the cable did not break on that occasion. I am speaking of when Mr. Brett was out when we had a "terrific run." Captain Kell was heaving in on that cable and wore the windlass out. That cable broke.

(*Captain Kell.*) No; it was on board the "Star."

690. (*Chairman to Captain Kell.*) Is not one of those Mediterranean cables now lying in the East India Docks?—Yes.

691. Which cable is that?—The one that was in the "Result;" it is a fac-simile of the one that is laid from Spezzia to Corsica.

692. What length of cable is there in the docks?—There is 77 miles in the East India Docks.

693. Is it all in one piece?—Yes.

694. (*Mr. Saward.*) Did you lift it?—No, I did not lift it; it was a part that remained. We failed in laying the cable, and brought that back again.

695. (*Professor Wheatstone.*) Was it considered to be injured?—No, not at all; it is as good as ever it was.

696. (*Chairman to Mr. Smith.*) Are you quite certain that gutta-percha cannot become deteriorated by exposure where the moisture cannot dry out of it, that is in continual use for submarine lines?—I am.

697. Are not all the specimens that have been recovered from submarine cables which have been laid for any length of time perfectly sound?—I have seen a kink lately taken from the Dover cable, and the gutta-percha is as good as when first laid.

698. You consider that gutta-percha is safe when so laid?—Yes.

699. (*Mr. Saward.*) What is the longest time you have known gutta-percha to have been submerged and afterwards taken up perfect?—I should say the Dover cable, laid in 1851. We have a piece of the core of that cable that has been immersed ever since that time; it still tests perfect.

700. So far as the evidence goes gutta-percha appears to be indestructible in water?—Yes.

701. (*Professor Wheatstone.*) Do fungi affect gutta-percha much?—Mr. Highton says they do; I have only his authority; we have never tried any experiments.

702. You do not know under what circumstances it is affected by fungi?—No, I do not.

703. (*Chairman.*) Do insects eat gutta-percha?—I have never seen a specimen where such a thing had taken place.

704. That piece of core which is now before you was laid in the Mediterranean somewhere near Corfu, it was covered originally with hemp; when it was taken up the hemp was all eaten away from it and insects appear to have eaten away the gutta percha where you see the small holes. It was also said that shells were sticking to it at the time?—We found barnacles attached to the experimental cable that was laid across the English channel.

705. But the cable was not eaten away?—No, I have not seen anything of this kind before.

706. Have you considered at all a plan which Mr. Macintosh has suggested of vulcanizing gutta-percha?—It is done by 97 per cent. of bi-sulphate of carbon and three per cent. of chloride of sulphur.

707. Does that improve the gutta-percha or deteriorate it?—It does not improve it for insulation; time would be the only test that you could rely upon for its deteriorating the gutta-percha at all.

708. Mr. Macintosh's idea, I believe, is to render gutta-percha more impervious to water. Have you heard that?—No, I thought it was for heat. It is the fact that it will stand more heat as regards the outside casing, but the inside, the gutta-percha, would get quite as soft and the wires would fall.

709. Have any experiments on vulcanized gutta-percha been tried at the gutta-percha works?—We have one mile that has been passed through the solution.

710. Has any other system of vulcanizing gutta-percha been attempted?—Some years ago the gutta-percha company tried to vulcanize gutta-percha, but not for telegraphic purposes.

711. Have you any specimens of it left?—Yes, we have a block there now. I cannot say exactly why it was discontinued.

712. (*Mr. Saward.*) Have you had any experience as to the chemical destructibility of vulcanized india-rubber?—No, I have not.

713. Was not there an attempt, in the early days of gutta-percha, to use sulphur mixed with gutta-percha?—Yes.

714. Did not that fail entirely?—Yes.

715. That was as early as 1848 or 1849?—About 1849.

716. Did not the gutta-percha become chemically destroyed, eaten into holes?—The sulphur affected the gutta-percha. In fact that gave rise to the invention of the fuse that is used for firing at long distances; it is from the sulphur affecting the copper and forming sulphuret of copper that the fuses are made.

717. Can you give any other information which you think will be useful to the Board of Trade in their investigation?—We are not standing still at the Gutta-Percha Works, we are improving every day.

718. (*Chairman.*) Is the supply of gutta-percha unlimited?—I think so.

719. Has not the price increased?—Yes.

720. Is that a sign of its being rarer or of the greater perfection of the manufacture?—*The greater perfection of the manufacture; but still the price does* rise in the market; the material itself increases very much in price.

721. (*Mr. Saward.*) I suppose the native are becoming aware of its uses?—No doubt.

722. Will you describe the system you adopt at the Gutta-Percha Works for testing cables?—When we receive an order we cover the wire, and place it in the canal, and the electrician, or whoever may be appointed, is supplied with 504 pairs of plates (ordinary sand battery), and a very sensitive galvanometer, one similar to the last that was made for the Atlantic Telegraph Company. I might remark that in this diagram the coil that shows 35 on the instrument we now use would not have shown anything on the ordinary galvanometer which was used at the commencement of the works some years since.

723. (*Professor Wheatstone.*) Do you ever employ statical tests?—I have merely for my own experiments.

724. What statical tests do you employ?—I merely do it by watching the discharge in a Leyden jar.

725. What instrument would you employ to measure the statical electricity?—A pith ball in measuring the amount of insulation.

726. I suppose you also measure the amount of charge and discharge when the wire is charged by a voltaic battery?—I always took it in testing for myself for my own information; but we have gentlemen come to the works who do not take the charge and discharge, merely the loss.

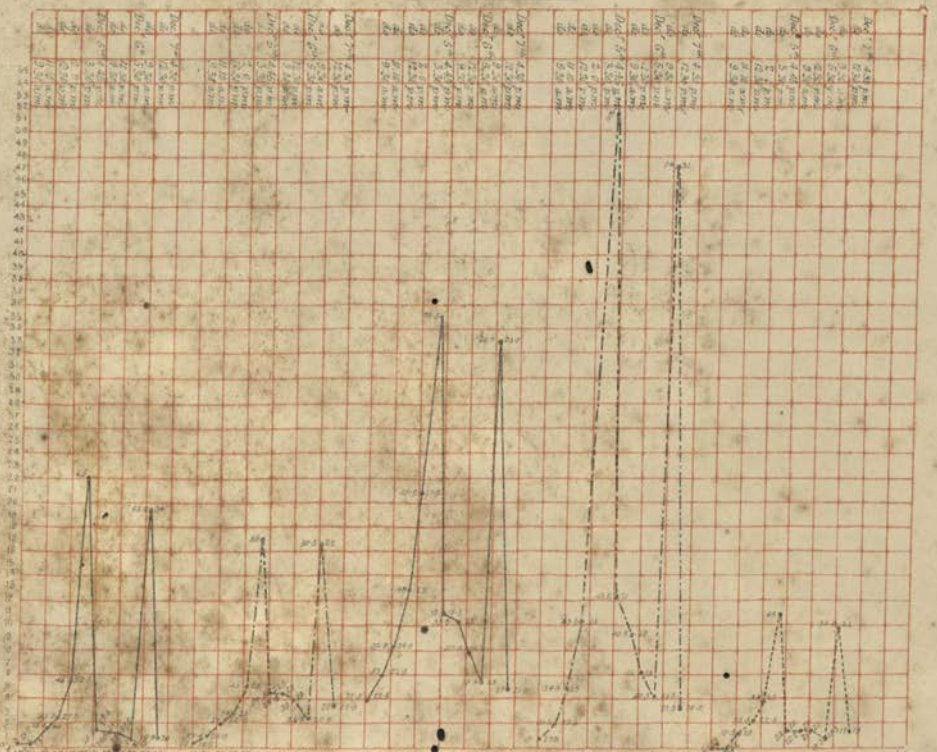
727. Merely taking the leakage?—The loss. There is one thing I should like to mention. Professor Thompson read a paper at the British Association this last year upon experiments that Mr. Jenkins had made at Birkenhead in comparing a coil of plain gutta-percha and a coil covered with compound between the layers, precisely the same as the Red Sea; he had a plain coil and one of two coatings of gutta-percha and two of compound, I found that those experiments were quite at variance with my own, and it rather annoyed me because I had gone somewhat deeply into it. I found that Mr. Jenkins had made a great mistake, he has not got what he thought he had; he has taken two coils, one is pure gutta-percha and the other is a mixture made on purpose for Mr. Newall to try experiments with and report to us. Mr. Jenkins has taken these two coils thinking they are pure gutta-percha and compound; and he gives out that the compound at a low temperature below 56° is a worse insulator than gutta-percha.

Plate No. 2.

THE GUTTA PERCHA COMPANY 18 WHARF ROAD, CITY ROAD, LONDON.

Diagrams shewing the comparative Tests of Five Cells of variously installated Electric Wires at different temperatures. The Battery power used consisted of 504 pairs of plates excited by dilated sulphuric Acid. Instrument one of Reid's very sensitive horizontal Galvanometers.

The figures on the Left-hand side of the Degrees shew the charge and those on the Right the Discharge. Length of each coil 1000 yards.



PREPARED BY WILLOUGHBY SMITH

728. (*Mr. Saward.*) Mr. Jenkins, in fact, was acting upon wrong information given to him?—I do not know where he got his information. We had orders from Mr. Newall to manufacture two coils of 16 copper wire, one covered with gutta-percha, and another of the same length and size was to be covered with what we term "special mixture," that was the arrangement with Mr. Newall and Company, and those two coils were sent for them to test with regard to pressure. This "special mixture" was not equal to gutta-percha, but Mr. Jenkins has fallen into the error of taking the two coils, supposing that one was pure gutta-percha and the other was gutta-percha with compound in between.

729. (*Chairman.*) From what you have heard and know of submarine cables that have been laid, do you attribute their failure to any causes which cannot be overcome?—I think not.

730. Do you think that submarine cables can be laid in any reasonable depth of water?—Yes, I do not see why they should not.

731. By "reasonable depth" would you go as far as 2,000 or 3,000 fathoms?—Yes, I should call that a reasonable depth for laying a light submarine cable.

732. (*Mr. Saward.*) You see nothing inherent in gutta-percha at all events that would render that hazardous?—Not the least.

733. Either as to its being laid or as to its permanency?—As to its permanency, no, that I am positive of.

734. You feel positive that there is nothing in gutta-percha to prevent a submarine cable being quite permanent?—Yes.

735. (*Chairman.*) If the cable is laid successfully?—Then I believe it will last for ever.

736. Provided it is properly manufactured?—Providing the manufacturing is perfect.

737. (*Mr. Saward.*) Do you recollect the circumstances attending the manufacture of the Atlantic cable?—Yes.

738. Was not it almost hopeless to expect success from the hurry in which the cable was first made?—I do not think that many anticipated success.

739. (*Chairman.*) From the necessarily little care that could be devoted to examining and testing it?—Every care was given to the testing of the core at our works; but, generally speaking, there was too much hurry in the completion of the cable.

740. (*Mr. Saward.*) As a man of business, do you think it was possible that the business arrangements in such a case could be perfect?—Our arrangements were perfect.

741. (*Chairman.*) Do you think that the Gibraltar cable ought to be laid successfully?—I think so.

742. Would you suggest any addition to the deep-sea portion from your knowledge of gutta-percha?—I think the outside should have a covering of something of this sort (*a specimen of core covered with a compound of gutta-percha and india-rubber*); it would be of great use, it would act as a cushion, and give power of resistance from any sharp punch.

743. (*Mr. Saward.*) It would act as a sort of buffer?—Exactly.

744. (*Chairman.*) Do you approve of vulcanized india-rubber for the purpose?—No, I am not an advocate for vulcanized india-rubber. *Mr. W. Smith.* 8 Dec. 1859.

745. India rubber compound?—Yes. This could be made as a sort of buffer, so that if you took a knife or any sharp instrument and punctured it, it would offer resistance.

746. Would you cover it with a serving of hemp and tar?—I do not think you can put too much tar and hemp, which, with the compound formed of rubber and gutta-percha, would make a good firm bed for the outer covering.

747. (*Mr. Saward.*) So as to place the core in the centre of a strong tube?—Yes, to protect the gutta-percha.

748. (*Chairman.*) Is not the making of the joints the most difficult part of the manufacture?—It requires great care.

749. Do you employ peculiarly skilled men for that purpose?—We have men who do nothing else.

750. Are they highly paid?—Their wages are higher than those employed in the general work of the factory.

751. (*Mr. Saward.*) Are they paid the wages of skilled workmen?—The pay ranges from 25*s.* a week.

752. (*Chairman.*) Is there any means of testing the joints before they leave the manufactory?—The joints made at our works are tested by those who test the core; but I think an apparatus with statical electricity might be brought into use by the cable manufacturers, for testing the joints made at their works. The present mode adopted by them, I believe, is to place the joint in water, and test immediately.

753. There would not be time for the water to penetrate?—No.

754. Would you expose the joints to pressure?—I am averse to pressure. I do not see why they should not be allowed to soak in water for a long time. I do not care how long gutta-percha soaks in water.

755. Do you think if the joints were soaked for a long time in water, and there were any fault that the water would be absorbed through that fault?—Yes. There is another thing, I would not object to pressure if you gave the gutta-percha time to dry after it came out of the pressure tank, before the tarred yarn is put on.

756. You would recommend before any cable is covered with a serving of tar, or has its final covering, that it should be thoroughly dried?—Yes.

757. What length of time would you give to the drying?—It would take some time to dry a coil of gutta-percha; but if the straps were taken off, and the wires spread out, it would dry more rapidly.

758. Would you hang it up in a warm room?—If hung in a warm room, great care must be taken that the gutta-percha is not injured thereby.

759. Can you give any idea of the length of time that would elapse between its leaving the tank and becoming dry?—I cannot say what time it would take. It would depend entirely on the means used.

760. Would it take a week?—More than that.

761. A month?—If left in the coil it would take months.

Adjourned to To-morrow at Two o'clock.

Friday, 9th December 1859.

PRESENT:

Captain Douglas Galton.
Professor Wheatstone.
Mr. G. P. Bidder.
Mr. Saward.

Captain Douglas Galton in the Chair.

James Atkinson Longridge, Esq., examined.

762. (*Chairman.*) You are a civil engineer, I believe?—Yes.

763. Have you devoted considerable attention to the subject of paying out submarine cables?—Yes.

764. And also to the construction of cables?—Yes; to the question of their proper construction.

765. Do not you divide the cables into two descriptions, one for shallow water and another for deep-sea water?—Yes. *J. A. Longridge, Esq.* 9 Dec. 1859.

766. For shallow water, what class of cable do you prefer?—I think the present shore ends are very suitable for the purpose.

J. A. Longridge, Esq.

9 Dec. 1859.

767. Up to what depths ?—I should say 200 or 300 fathoms ; 200 fathoms.

768. And there you would adopt another form of cable ?—I would then adopt another form of cable.

769. What form would that be ?—Generally a cable very much lighter than the present usual form of cable, and without an outer covering of iron. I should put the whole of the metallic substance inside the cable ; I think the best place to put it is, as proposed by Mr. Allan, close in contact with the lower wire ; but steel wires might also be put outside the insulation, and laid without any spiral, or the least possible spiral ; I should prefer them without any spiral at all.

770. Do you refer to the principle of Hall's and Wells patent, of which there is a specimen before you ? —No ; I should take the present core, and lay the steel wires in the direction of its axis. I should use very small steel wires of the finest steel, and should pass them through rollers, so as to crush them slightly into the gutta-percha, and then over that I should put the outer covering which was to keep them in their place, to give general protection and buoyancy to the cable.

771. Would you anticipate any difficulty in coiling and uncoiling a cable of that sort ?—None whatever.

772. Would the wires on the inner side of the core, do you think, compress sufficiently to compensate for the diminution of length in the coiling ?—The size of the coils is so great that the difference between the inner and outer radius of any one wire would be imperceptible with the thickness of wire that I should use.

773. They would yield sufficiently for that ?—Yes ; the slightest bulging outwards would yield sufficiently for that.

774. You would have no fear that the inner wires might be forced into the insulating covering ?—None whatever.

775. You have mentioned Mr. Allan's cable ; do you anticipate any inconveniences from the union of the steel and the copper ?—I am not an electrician ; but I have always understood that to have electrical action, you must have the presence of three bodies ; I am not aware that there is any electrical action betwixt copper and steel, therefore I do not anticipate any such difficulty as that ; but I fancy it is one which might be submitted, not to opinion, but to experiment.

776. It has been urged that it is not so much the electrical action, but the diminution of the conducting power of the cable ?—We know very well that the conducting power of steel is less than of copper. I have seen a core which Mr. Allan has made, and which he, in fact, proposed to submit here, in which the conducting power is larger than that of the proposed Falmouth and Gibraltar core, and in which he has a very considerable degree of strength ; yet the inductive surface is no greater. The question of induction, of course, is much more one for an electrician than for me ; but I think there are some very simple facts about that which will remove a good deal of the difficulty which hitherto has been supposed to exist. It has always appeared to me that the cause of the retardation in these long cables is, that they have made their conductors a great deal too small. The theory, at the time the Atlantic cable was made was that the smaller the conductor the less the retardation. I combated that theory myself at the Institution of Civil Engineers. I am bound to say that Mr. Alfred Varley agreed with me in that opinion, and I believe now that that theory of small conductors is entirely abandoned. It is quite contrary to all analogy and reason, whether we are conducting electricity, water, or anything else, that we should get it quicker and easier through a small pipe than a large one. I look at the passage of electricity as very much analogous to the passage of water through a pipe, and I believe, when the conductor was made very small, they were obliged to use electricity of such enormous intensity, that it actually charged the dialectric itself. I do not believe that there is any such thing as an absolute dialectric, that is to say, a substance which will not receive electricity ; it is very difficult to charge it, but with the enormous intensity which they tried to force through these cables, I believe the dialectric itself became slightly charged, and that it was this which produced the retardation. Of course, these are simply opinions of my own, which are not worth much, because I am not an electrician.

777. In fact, you divide the retardation into two parts, as it were; one due to induction, and another to the residual charge within the lining of the gutta-percha itself ?—Yes.

778. (*Mr. Saward.*) Will you be good enough to explain your reason for preferring the placing of the strength of the cable inside to an external envelope ? —In the first place, the chief reason is, that by doing so you relieve the strain from the insulation altogether ; you throw the strain entirely upon the conductor and its accompanying wires ; the conductor itself, of course, being copper, is very extensible, and will not bear that strain; therefore the strain practically comes entirely upon the wires which are placed round the conductor. In any of those iron-covered cables, as soon as the strain comes on them it first comes on the outer shell, which, from the peculiar construction, with the spiral lay, tends to extend independently of the extension due to the tension. Whenever you deal with a spiral body like that, you are quite certain that you will more or less elongate the spirals, and lengthen the outer covering. Take, for instance, this hemp-covered cable ; the difference of length between one of these spirals and the length of the cable itself is $2\frac{1}{2}$ per cent. If these spirals were untwisted and laid straight, they would be $2\frac{1}{2}$ per cent. longer than the cable itself. I do not mean to say that putting them in a testing machine they would untwist to that extent, but I believe, when that cable comes to be laid down, the untwisting would go on for another reason, to which I will presently advert. In testing, it will not untwist to that extent, but will untwist to some extent ; and whenever it untwists it elongates these spiral strands, and consequently throws the strain upon the gutta-percha and the copper inside.

779. (*Chairman.*) Why do you anticipate the cable will untwist ?—In the laying, simply for this reason. We have here a spiral body which is descending through the water ; as it descends through the water, the water acts, in fact, as a nut fitting a screw ; it is a nut which offers only a minute resistance, it is true, but it does offer a resistance. When you take into account the enormous length of cable which is going through the water, to the extent of perhaps 40,000 or 50,000 feet, and that you are running that out at a velocity, frequently, of a foot and a half or two feet per second, you will find, if you calculate the effect of untwisting that cable, it is something very considerable. Mr. Brooks calculated it for the old Atlantic cable, and it amounts to not less than about 102 grains per lineal foot, applied at the circumference of the cable.

780. Was that calculation in your paper ?—No ; that is a matter which has only been calculated lately; 102 grains per lineal foot, corresponding to the velocity of one foot per second. Now the effect increases as the square of velocity. Consequently, with two feet per second, it is equal to 408 grains per lineal foot of cable, which, in 2,000 fathoms of water, the cable paying out at an angle of 20 degrees, would amount to not less than 2,000 lbs. applied at the circumference of the cable, not to any one point, but all throughout.

781. From the friction ?—The passing through the water as a screw passes through a nut.

782. (*Mr. Saward.*) Would not that be equal on all sides ?—Of course ; it acts as a power to untwist the outer coating.

783. (*Chairman.*) But the cable is held at one end, and descends vertically. Besides would not any tendency to untwist to which you allude, to some extent be counteracted by the tension and the weight of the cable itself ?—No ; the cable is running out

freely at the top, and it is fixed at the bottom; at least, it soon becomes fixed at the bottom.

784. Still there is the tension upon it?—It is simply longitudinal tension; but the longitudinal tension cannot counteract it; whatever effect it has, increases the tendency to untwist.

785. (*Mr. Saward.*) There would not be the tension, but an opposite quality at the bottom?—No, it is running down, finding its way to the bottom.

786. Would you say there is a portion of it lying upon the bottom?—At a certain distance, where it reaches the bottom (I do not pretend to say how far; I believe, very soon after it reaches the bottom); the cable by the friction against the bottom, is prevented from moving. Consequently it is the case of a cable fastened at the bottom, and untwisting all the way up to the top. Of course, as it is untwisting, —the tension of which Captain Galton spoke,—is tending also to lengthen it, and, instead of acting against it, it acts in the same direction. Now, when you consider the great length of cable which is passing through the water, betwixt the bottom and the ship, as I have said, not less, perhaps, than 40,000 or 50,000 feet, and that there is a force of 102 to 500 or more grains per foot upon that, tending to twist it round, the effect must be very considerable. It is very evident to me that a cable does untwist; and, moreover, I think it has been observed, in laying the Atlantic cable, that it was constantly untwisting in a direction contrary to its strands. I have seen it stated in print by a person who was on board the expedition; and I may also mention, in confirmation of that, that I frequently, in a part of the country where I used to live, had an opportunity of seeing wire ropes and other ropes on inclined planes, which were fastened at one end to a rope roll, and at the other, to the waggons. Any person going at the same speed as the waggons are travelling might see that that rope, whether wire or hemp, was constantly turn round.

787. (*Mr. Saward.*) Have you ever been down a deep coal pit suspended by an iron rope?—A great many times.

788. Did you observe that you went round?—You cannot because the cages go in slides. I should mention that that would not be at all an analogous case, it is merely passing over a drum at the top of a long shaft. I was speaking of its passing over a series of pulleys at intervals of 20 feet. The action of those pulleys and the spiral lay causes that rotary motion.

789. Is not it the fact that in coiling a cable properly a turn is put into the cable?—One turn is created in each coil by the process of coiling.

790. Is not that taken out of it as it comes off, or should it not if it is properly coiled?—I believe you could not take it out again unless you put a turn in it.

791. You put a turn in it first when it goes in, as it is coiled in you put a turn into the cable?—The coiling necessarily produces a turn as the cable goes out.

792. Is not that turn taken out of it as it comes round the cone out at the stern of the ship?—No, certainly not; the turn will be in it always, and that is one of the great causes of kinks.

793. Would not the effect that you speak of be greater or less according to the angle at which the cable fell from the stern of the ship?—Of course because the length of the cable between the ship and the bottom is greater, it would be greater as the angle is smaller.

794. In the case of a cable having a superior power of floatation to the Atlantic cable, would not your objection be very much modified?—It would be greater provided it were made with the spiral lay, but I object to a spiral lay on the outside of the cable altogether; cables with a smooth surface without spiral lay have no tendency whatever to that action.

795. Because it is, to a certain extent, homogeneous, so far as the outside is concerned, at all events?—Yes.

796. Is not the theory of laying cables so to lay them as to avoid any strain?—It ought to be as far as possible.

797. You think that cables should be laid without strain?—As much as possible.

798. In that case the objection to a cable of that description would not hold, would it?—The objection would hold just the same.

799. But the evil consequences which you anticipate would not come into action?—I beg your pardon; the untwisting due to the spiral lay would come into action just the same; the extra lengthening due to tension, if there was no tension, would not exist, but the case of a cable being laid absolutely without tension is practically impossible.

800. (*Chairman.*) To meet your objection it would simply be necessary to cover the cable outside with hemp or something of that sort, which would prevent the water from acting upon the spiral lay?—It would, provided the cable were so covered.

801. If the cable were wrapped round with hemp for its whole length, that action could not take place?—It would not take place.

802. (*Mr. Saward.*) Would not the process which you advocate involve considerable extra expense in the construction of the cable?—I think not. I think it would be a great deal less expensive. I have no doubt of it.

803. (*Chairman.*) If means were taken to get rid of the effect of the spiral lay in passing through the water which you anticipate, should you then have any objection to it?—I should have the same objection on account of the tension. The tendency of the tension is to untwist the spiral, and the more direct you get the strain upon the material which is to bear it, the better; the only object of making ropes with a spiral lay is to keep the strands together. The wire ropes which were originally made in Germany, and which lasted perhaps better than any other ropes which were made, were not made with a twist at all; they were simply bundles of wire fastened together longitudinally.

804. (*Mr. Saward.*) What was the objection to their continuance if they are discontinued?—They had other objections. There was the difficulty of keeping the ropes together, and the perfection to which they have got this rope machinery now renders it much more easy to make them; they could not make them in those days, but no one would think of adopting the spiral lay in the chain of a suspension bridge or in anything of that sort. Where there is a direct pull, you should always get your strain right direct through the axis of that which is to bear it. I can see no reason whatever for the adoption of the spiral lay excepting the convenience of putting it on.

805. (*Chairman.*) You do not think that a sufficient reason, upon the assumption that part of its defects can be guarded against?—Certainly not, because that would not apply if you put the iron where I would have it put.

806. In the centre?—Yes; either within the insulation or just outside the insulation, within the outer covering.

807. In this specimen it is put just outside the insulation?—Yes; but there is an intermediate substance of hemp.

808. Merely to prevent the wire pressing against the insulation of gutta-percha?—Yes, you cannot lay them longitudinally without you put something outside to keep them in their position.

809. (*Mr. Saward.*) Would not a cable made longitudinally imbedded in a plastic surface, such as you have described, be liable to get bent about and injured in the machinery, and if it once took a form would it not be difficult to get it out of that form?—I think not; I have had pieces of cable through my hands made in that way, which I have turned into circles of about 12 inches diameter without injuring them in the slightest degree.

810. Were those completely imbedded in some plastic material?—In gutta-percha.

811. (*Chairman.*) I believe you have turned your attention to the question of machinery for paying-out cables?—I have.

812. Are your views embodied in the letter which

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you forwarded to me?—That letter was intended more especially to bring to your attention some methods of paying-out which had been brought to my notice by my friend, Mr. Brooks; my views on paying out apparatus are not embodied there, but they amount to this, that it is absolutely essential, especially with cables where there is a considerable amount of tension, in paying out that you should have machinery which is not rotative. The cause, I am quite satisfied, of the failure of the Atlantic cable was the paying-out machinery. The construction of the cable itself was vicious, but the cause of the failure was the paying-out machinery. The second machinery was more perfect than the first, because it was much lighter, but with a cable of that construction or any iron covered cable in deep water, you must have heavy paying-out machinery or some means of balancing the tension. The tension, we all know now, and every one admits, is due to the length of the cable equal to the depth of the water which you are passing through; that has been proved mathematically, and there is no doubt whatever about it, and that is diminished in the case of a cable paying out by the friction of the water. If the cable is paying-out at the same rate that the ship is moving, as it ought to do, the diminution of friction I find is very small indeed. It only amounts to a few pounds in 3,000 on the Atlantic cable. But if you let the cable run out faster than the ship is moving, then there is an increase of friction, which varies as the square of the increased velocity of paying out, and also according to a certain co-efficient of friction, depending upon the outside surface of the cable. It has been attempted to relieve the great tension in deep water by letting the cable run out in that way, but the amount of relief so obtained is so small even with a cable like the Atlantic that I think it is very unadvisable indeed to do it. Not only is there a great expense of extra cable, amounting to 30 or 40 per cent. frequently, but you have a greater length of cable to work through afterwards. To prevent that cable running away, you must have some paying-out apparatus which will balance the tension at the top. Now taking that tension at 30 cwt., as it was in the Atlantic cable, you will require very heavy rotative machinery, and if you use heavy rotative machinery (and to be strong it must be heavy) to take that up, then arises this other difficulty, that whenever by a sudden lurch of the ship, whether she heaves her stern up or yaws about, or "scends" forward, which she sometimes does, you have this heavy machinery to put into more rapid motion than it was in before; consequently you have the inertia of the drums to overcome. I have gone carefully into that question, with regard to the last Atlantic machinery, and I am quite satisfied that at times strains came upon that cable from the inertia equal to 80 per cent. of the weight of the rotative machinery, that would be at least three tons. Now it has been said to me that it was not so, that if so the indicator would have shown it. That is a complete mistake. The indicator has exactly the same vice in its construction as the paying-out machinery; there is the inertia of the indicator; which comes into play just at the moment it should not. The inertia of the indicator tends to show a minimum tension when, in point of fact, the tension is the greatest; the tension is at the greatest just at the time when the stern of the ship has got to the bottom and is beginning to rise again. There are diagrams here which will show that very distinctly, but that paper does not show the amount to which the action will take place; but I am quite sure from observations I have made upon the velocity of vessels pitching, that the inertia of that machinery would amount in some cases to three tons.

813. What angle do you assume that the cable is going out at?—I assume an angle of about 15 or 16 degrees. The strain varies as the sine of the angle.

814. (*Mr. Saward.*) Would not the relief from friction by flotation be enormously greater with a cable like that than it would be in the Atlantic?—It would be greater considerably. I should be very desirous indeed (perhaps it has been done) that experiments should be actually made with respect to the amount of friction upon cables. It is a most important element, particularly with heavy cables; it is not of so much consequence with light cables, because the question of their submergence is very simple, but with heavy cables it is a most important element. I suggested to Sir Charles Bright and Mr. Cyrus Field when they went on their experimental trip, some experiments which would easily have determined it for the Atlantic cable, but they did not make those experiments. I suppose they had not the time, or did not think them of sufficient consequence.

815. (*Chairman.*) What were those experiments?—Having ascertained the depth of the water, letting the cable run out vertically, and keeping the ship stationary, to ascertain the final velocity it would take, and from that velocity there is a formula, by which you can calculate the co-efficient of resistance. It would have been very easily done. There are other ways of doing it, by dragging a length of cable over the stern of a ship, for example, but that would not be so satisfactory as the other method. I have no doubt in my own mind that the friction increases somewhat as the cable descends.

816. As you increase the pressure?—As you increase in depth; I think so, but that is not a point I am sure about; but then that same increase of friction, is at the same time producing that effect on the spiral lay which I think is so very objectionable. I may, perhaps, mention, with reference to the paying-out apparatus proposed by Mr. Brooks, the way in which the principle of it differs from what has been hitherto used. This is a model that I have had made. In the first place it is very well known that the normal pressure upon any curve is equal to the tension divided by the radius. The small drums that you must necessarily use on board ship produce a very severe normal strain indeed upon the cable, just when it leaves the drum, which rapidly decreases as it goes round. The first object of this cone is to have a uniform normal pressure throughout the whole paying-out apparatus, and consequently the friction being uniform, there is a uniform diminution of tension for each foot of the apparatus. That curve which I have here shows the curve of equal pressure; the cable is seen passing over one part, under the next, and so forth. That, I dare say, would answer very well; but this other plan, which is a different application of the same principle, is, I think, very much better. Curiously enough, a cone with straight sides has the same mathematical properties precisely as the curve. This spiral crosses always at the same angle—in this model it is not made correctly, the man made a mistake—but the spiral should cross always at the same angle as it comes up; in that case the normal pressure is exactly the same.

817. Is that cone intended to revolve?—No; it is *substituting* a sliding motion for a *revolving motion*; in fact, having the friction absorbed by the sliding of the cable itself, instead of by the using of drums and brakes. We will suppose that we are in a depth of water which requires the tension represented by what you feel by that string; we increase the depth of water; we want to put on a heavier tension. Now we can, by having this cone properly proportioned, make it suitable for any depth of water we like. Knowing what the depth of water is, we know at what point of the cone the cable should leave it. I should have the cone held in its place, not fixed; it may be fixed, but it is a better plan to hold it by means of apparatus, so arranged that if the tension exceeds the amount at which it is held the cone is free to revolve, and the moment that takes place the tension decreases. This apparatus will then draw back the cone, and bring it up again exactly to the normal state, so that, in fact, it is perfectly self-acting, and the tension never can exceed the amount at which you fix it.

818. (*Mr. Saward.*) In fact, it can be managed by hand?—Yes, it can be managed by hand, but it would

be much better managed by a simple mechanism. You will see by this model, in case of a kink coming up from the hold, all you have to do is to let the drum run free, and the cable frees itself immediately.

819. (*Chairman.*) You must have a cone constructed to suit the dimensions of each cable, I suppose?—The angle of this cone depends upon the co-efficient of the friction of the cable.

820. That would require a cone for each cable?—Yes; the same effect takes place if the spiral is not suited, though not so perfectly. To make the thing act perfectly, the spiral ought to be suited to the co-efficient of friction. These cones might be carried on board ship, and put in their places in five minutes. There is another plan which is on that paper, which consists of two endless belts. So long as the tension does not exceed a certain amount, those belts revolve and the cable goes with it. If the tension of the cable exceeds that, it immediately slips through between those belts. Those belts are faced with steel, with a groove on each. By means of this arrangement, like what are called lazy tongs, you may put on any amount of pressure you choose. If the tension exceeds that amount, supposing the ship gives a sudden lurch or pitch, the cable immediately slips through this groove. It is like holding it in a man's hands; when it pulls too hard it slips through, and I think that apparatus is peculiarly valuable for picking up, because in picking up there is infinitely more danger than in laying down.

821. (*Mr. Saward.*) Supposing a proper cable were made, either in the way you have indicated or in another way, so that it could be deposited at the bottom of the sea in a perfect state, do you see or know anything to prevent that cable from being permanent, or that would militate against the certainty of its continuous action?—My own opinion is that it would remain for ever in deep water; in shallow water it is subject to various causes of injury which might destroy it.

822. In shallow water it could be easily repaired?—Yes, in shallow water it can be easily repaired, but once in deep water, and the deeper the water the better, the more safe the cable is, in my opinion; and I may say also that I feel the utmost confidence in being able to lay a light cable with the greatest possible ease. Almost every nautical man that I have talked to on the subject, has said, "If we had only just a very "light cable that could be paid out like paying "out a log line, there would be no difficulty at all "about it."

823. (*Chairman.*) You would not pay out too much slack?—It is with the heavy cables that there is the tendency to draw out slack. With a light cable of a specific gravity of, say, 1·5, and a rough outer surface, you could not pay out so much slack, the friction so rapidly takes up the increase of velocity.

824. (*Mr. Saward.*) You would at all events try to pay out sufficient slack to cover the contour of the bottom as nearly as it could be ascertained?—I would pay it out with very little slack at the bottom.

825. Would you not make a careful examination with a view to ascertain the contour of the bottom? I think it is very important to have proper soundings taken; and another important point is to take proper means to ascertain the actual velocity of the ship. Without you do that you must be guided entirely by the tension of the cable. If you know your section and the velocity of your ship, you may go to work with the greatest possible confidence.

826. (*Chairman.*) With respect to the effect of paying out light cables as compared with heavy cables, are you aware that when the first Dover and Calais telegraph, which consisted of a single gutta-percha line, was tried, a certain length was paid out, and that when an equal amount of heavy cable was paid out they ran short?—I am aware of that, and also that they put weights on the first Dover and Calais line to sink it. The most successful case of cable laying that ever was known, I believe, was the Varna and Balaklava cable; that was quite a light cable without any outer covering, and it was paid out a distance of 300 miles with something like 2½ per cent. of slack; that cable remained perfectly good for twelve months, and only gave way at last, I believe, from not having been properly protected at the shore ends. With reference to testing the cable for insulation, I mean as far as the mechanical question is concerned, to see that there are no holes that the water can get through; I observe in the specification for the Gibraltar cable, a copy of which I have here, it is said that the core shall be tested when it leaves the works, under pressure. In the first place I think not only should the core be tested, but the cable after it is complete should be tested; I think that is absolutely essential.

827. In the same way?—Yes. In the next place I think it is quite useless, if I may say so, to test it by a pressure of simply 1,000 lbs. per square inch when that cable is to be subjected to 7,000 lbs. pressure. I think it is essential that cables should be tested both before covering and after covering at a pressure at least equal to the depth of water they will have to go under.

828. (*Chairman.*) Are you not aware that there are great difficulties in testing large coils of cable?—I think it would be very easy.

829. In what way would you test a cable?—I should not test it in coils, but as the cable was made I should pass it through a two-inch gas pipe of a certain length, say 200 yards. I should fit it at each end with leather collars, and I would bring a pressure of four tons per square inch to bear. I would let that remain for so many hours, carefully examining the state of insulation, and when that had been done I would relieve that portion, and draw through another length. I would test every inch of cable in that way both before and after the covering was put on.

830. Would not testing the whole length of a cable take a long time?—It would take a good while, but that is of no consequence whatever compared with the security that you would get.

831. You would test each portion according to the depth of water at which it was to be laid?—At least to the depth at which it was to be laid.

832. (*Mr. Saward.*) You think that the testing could be done with considerable facility?—It would be done with the greatest possible facility with a smooth cable; it would be more difficult to do it with a cable with spiral strands, because it would be difficult to make it tight at the ends.

Captain Galton, R.E.

DEAR SIR,

I RETURN the corrected proof of my evidence. I have, since I gave it, looked for and found the "Times" report of the proceedings on board the Agamemnon, and as there are two or three passages which show very strongly the evils of a heavy paying-out apparatus, I have copied them out in the hope that you will append this letter to and consider it as part of my evidence.

"During the afternoon of Saturday (31st July 1858) the "wind again freshened up and a tremendous sea ran, which "made the Agamemnon pitch to such an extent that it "was thought impossible the cable could hold on through "the night." "Men were kept at the wheels of the "machine to prevent their stopping as the stem of the "ship rose and fell with the sea, for had they done so "the cable must undoubtedly have parted."

(2.) On Sunday 1st August it is stated:—"The two engi- "neers who had charge of the relieving wheels had to keep "watch and watch alternately every four hours, for on their "releasing the breaks every time the stem of the ship fell "into the trough of the sea, entirely depended the safety of "the cable."

(3.) "During Sunday night and Monday morning the "weather continued as boisterous as ever, and it was only "by the most indefatigable exertions of the engineers upon "duty, that the wheels could be prevented from stopping "altogether, as the vessel rose and fell with the sea, and "once or twice they did come completely to a stand still, "in spite of all that could be done to keep them moving, "but fortunately they were again set in motion before the "stem of the ship was thrown up by the succeeding wave. "No strain could be placed upon the cable of course, and "though the dynamometer occasionally registered 1,700 lbs. "as the ship lifted, it was oftener below 1,000, and was "frequently nothing." Again Professor Thomson of Glasgow, who was on board the Agamemnon, says in a

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letter to the editor of the Civil Engineer and Architects' Journal, dated 23rd November 1859:—"that, for a time, "the cable with every pitch of the ship, was to be seen "alternately hanging loose nearly vertical over the stem, "presenting the appearance of having been broken short, "and then immediately drawn up so as to look like a rigid "bar, or sometimes even flying up above the straight line, "so as to present a convexity upwards."

These extracts will, I think, satisfy the members of the committee that the opinions I expressed respecting the effect of inertia and the erroneous indications of the dynamometer were, although arrived at from theoretical considerations, quite borne out by practical experience.

With reference to Mr. Woolhouse's opinion based upon a mathematical investigation of the construction of the dynamometer, I give you an extract from his letter.

"The greatest value of T (the tension at the ship) will, "in general, therefore occur near the bottom of a wave and "the least value near the top. It thus appears that the "*maximum tension* really takes place near to where the "*indicator tension is a minimum*, and vice versâ."

If the committee will look at the table given in by Mr. Woohouse on Friday last, they will at once see how incorrect must be the action of the dynamometer, for whilst the depth of water varied from 1,550 to 2,424 fathoms, and the rate of paying out was at one time 48 per cent. in excess of the ship's rate, the indicated tension varied only 75 lbs. or about 3½ per cent.

On looking over my evidence I find I have not expressed so distinctly as I ought to have done my opinion with reference to the specimen described in the specification of the Falmouth and Gibraltar cable as the deep sea cable No. 2. What I said respecting the "spiral lay" bears especially on this cable, even more so than on the Atlantic. I think the disposition of the steel wires is radically bad, and that as they are placed they do more harm than good. The cable will stretch and untwist during the process of submergence, the gutta-percha and the copper will be strained, and although the cable may not be broken, the insulation will very probably be so injured that the signals will become weaker and weaker, and eventually die away altogether, as they did in the Atlantic cable.

I remain, dear Sir,
Yours faithfully,
JAS. A. LONGRIDGE.

16th December 1859.

Mr. JOHN MACINTOSH examined.

Mr. J. Macintosh.

833. (*Chairman.*) You have paid considerable attention to the manufacture of india-rubber, I believe?—Yes.

834. Do you prefer it as an insulator to gutta-percha?—I do in some cases.

835. What are those cases?—The advantage of india-rubber where it is cold vulcanized is, that if an incision be made by a piece of wire, or it is fractured at all, the aperture immediately closes up under any pressure, providing there is no stretch upon it. For instance, if you were simply twisting the india-rubber as it was formerly done round the wire under great tension, and an incision was made into it, that hole elongated; but in putting it on as I put it on cables, nothing of that sort can take place. I put on, perhaps, six or seven coats simultaneously, under a pressure of, perhaps, four tons to an inch, by rollers, the same as iron is rolled with, so that every particle of it is subjected to pressure. It is put on in successive coats, perhaps there are ten rollers or six rollers; the first roller puts on one coat, and the second another, and so on, till you get the required thickness; but every inch is subjected to a pressure of two or three tons, so that there is no possibility of any air getting into it. The gutta-percha or india-rubber being in a warm plastic state while the several coatings are applied to the wire (without the use of water), the heat and great pressure render the insulated cable perfectly homogeneous.

836. Your principle is to put on a number of successive coats?—Yes, a number of successive coats under great pressure. Then, after it is finished, I pass it through bi-sulphate of carbon and chloride of sulphur, so that it is vulcanized, and that prevents abrasion or heat injuring it; in that case it is thoroughly impervious to moisture.

837. Have you made any tests upon the subject?—Yes, very practical tests.

838. To what pressure?—Three and four tons; but both gutta-percha and india-rubber ought to be put on in a series of thin coats; and in the case of wires, I think putting them on under great pressure consolidates them, and renders the specific gravity of the mass heavier. The next point is, to protect the insulating core from abrasion, and at the same time to prevent its stretching. The strands are all put on parallel, and imbedded in a cheap insulating material, so that the insulation commences from the outside. You may turn the cable or twist it any way you like, neither heat nor abrasion will have any effect.

839. That is for the outer covering?—Yes.

840. Are those strands hemp or steel?—They are hemp; there is no steel; each of those strands will lift 150 pounds; I consider this cable (a specimen of hemp-covered cable) one of the worst I have ever seen; first, if the cable is broken in any way you have two sharp points of steel, that in case of a kink, would penetrate into the gutta-percha and render the cable unserviceable; and in the next place, from exposing the hemp in such an immense mileage to the salt water, the contraction of it would be very serious indeed.

841. (*Mr. Saward.*) In what direction would the contraction be exerted?—It would contract the whole affair.

842. Would it not contract and assist to protect the core?—No; it would contract so that the inclination would be to kink as much as anything.

843. Are you aware that this hemp is laid round a steel wire?—I am perfectly aware of that.

844. Would not the contraction be round the circumference of the wire?—Certainly, in all directions; but the effect of the contraction would be to kink it. If they were parallel there would be no inclination to kink.

845. (*Mr. Bidder.*) Have you made any experiments to show the extent to which that contraction would go on?—No, not to any extent.

846. You think that there would be a considerable contraction of the hemp?—I have no doubt that there would be a great deal of contraction; besides there is no body in it. In this cable the hemp untwists itself. In the cable I am speaking of, the strands are laid parallel, they are absolutely imbedded in the water-proof material.

847. Have you exposed this cable of yours under pressure in water to any considerable extent, in order to see whether the hemp contracted at all?—The water does not get through, because the whole material is rendered an insulator, being vulcanized; it is not vulcanized in the usual way by heat, it is done by cold. Now the strain upon the cable is entirely upon the longitudinal strands; in that case the whole of the strain is upon the gutta-percha core, or the contractor. If one of these steel wires breaks, what is the result? There are two sharp points that might spoil the core, whereas, if the outer covering of my cable is injured, at all events there is nothing else injured.

848. (*Chairman.*) Has that cable been under pressure long enough to enable you to say that the water would not penetrate to the gutta-percha through this outer covering?—I do not think it would, because I prefer putting a skin of that collodion and asphalte, and it is as impervious as glass itself. Now, both india-rubber and gutta-percha are absorbents, but there is no absorption in that whatever. I have boiled it for weeks.

849. Does not the collodion get injured by being bent?—Not the slightest, when it is properly prepared collodion. The collodion that is used for photography will crack.

850. On bending this a series of cracks appear. Will those disappear when it is unbent?—You will find that it is not cracked.

851. At any rate there are a series of wrinkles?—It may be that.

852. (*Mr. Bidder.*) Have you subjected any of

these specimens practically to any very great pressure, and at the same time to extensile force, tending to draw them out longitudinally?—I have not tested them except by pressure.

853. Do not you think it would be desirable, before a definite opinion is expressed, that the wires should be exposed to that test. You must be well aware that in laying cables at any great depth, they ought to be calculated to meet extreme pressure?—Yes, as a question of laying.

854. Apart from that, whatever may be the system of laying, they may be exposed occasionally to extensile action, as well as compressile?—That would depend a great deal upon the specific gravity of the cable. I am talking of deep sea cables, where the specific gravity would be just that that it would make it sink, and in passing the abrupt portions of deep water, even if it did not find the bottom, it would matter very little; but if you incorporate the wire with your outside covering, then it becomes a very serious thing.

855. Is it not a very grave question, in adjusting a cable, whether you should have a covering so strong as to resist any pressure, or at once resort to that which is solely dependent upon its internal structure?—I take it that deep sea cables, when they once reach the bottom, remain there quiescent, and then it is a question of the absorption of the core. In paying out cables, if you make up your mind to subject your cables to a strain of three or four tons, or even one ton, I think it is very injurious, because the gutta-percha is put on by heat, and it is all set. If there are any slight imperfections in the sharp wires, by elongating the cable you disturb the whole affair.

856. Whether the cable adopted be heavy or light, you must pay it out under circumstances in which it is exposed to a very small tension, at the same time it may be exposed to a considerable tension?—The apparatus that I proposed was a very simple thing, nothing more than having a strong canvas cylinder filled with hard wood balls, passing the cable through it, and having a case where you could compress air as you liked; the cable, as it were, passed through so many human hands, those small balls revolved, and the cable was always kept at a proper temperature; then it passed from that into another longitudinal cylinder of canvas, those cylinders were kept in proper position by bridles, so as to accommodate themselves to any heaving or pitching the ship might have; kinks and all might pass through. But the experiments that I have tried clearly convince me that if a vessel is going along at the rate of six knots an hour, and a sea comes up at the rate of 10 or 15 knots, and strikes her, she will rise suddenly, and the inertia of the mass over which the cable passes cannot come up to the same speed; there is an extra strain put immediately upon that part of the cable passing over the pulley at the point of departure, and consequently the tendency is to flatten the core of gutta-percha, and bring the conductor nearer the surface, instead of having an eighth of an inch of insulation, it may not have a quarter of a sixteenth. I have found several instances of that, particularly where the hold is at 90° in the cable passing over the pulley.

857. (*Chairman.*) Have you found instances of that in cables that have been laid?—In experimental cables, and even those that have been laid.

858. How were your experiments made?—By taking the cable over a common crane, and lifting a certain weight, and doing that with the temperature at about 80°, taking the temperature of the hold to be about 80°.

859. Are you aware when a cable leaves a ship going at the rate of 6 knots an hour, that the angle of that cable would be under 15°, and, of course, any transverse strain upon the cable very much depends upon the angle which is formed between the cable and the ship?—I am speaking now of a wave striking her under water, which alters that angle entirely, and the ship would raise up 20 feet in a few seconds.

860. Have you ever calculated or considered, in a case where the cable leaves the vessel at an angle of 12° or 15° going at the rate of 6 knots an hour, what would be the effect of raising the stern of that vessel suddenly 20 feet. Would it have a great effect upon the angle at which the cable was leaving the vessel?—It would have a great effect on the point of departure, the point of pressure; it is pressure I am speaking of now particularly.

861. Should you be surprised to hear that experience has shown the contrary to be the fact, that a sudden rise of the stern of a ship has very little effect upon the cable, if any?—How is it that you absolutely increase with the sea striking the ship upon the quarter, two tons in three or four seconds? Is that no effect?

862. Increase two tons in what?—In three or four seconds on the sudden rising of the ship in point of tension. Is it not the case that cables are snapped by the rising of the ship?

(*Mr. Gisborne.*) I never knew an instance.

863. (*Chairman to Mr. Macintosh.*) Will you name any that you know?—I will tell you one that I feel interested in, that was in picking up a portion of the Atlantic cable; and I have no doubt Mr. Longridge could name many others. I arranged to pick up 360 miles of the Atlantic cable, in order to satisfy myself that the mode in which it was laid was bad, from the very fact of the conductor being so near the surface, and I engaged a vessel for the purpose. The captain told me that all was going on very well indeed, and as long as the water was smooth they were paying in beautifully; but the moment the ship got into a little jumble of sea, the stern rose up suddenly, and the result was that the cable snapped. On leaving Valentia to cross to Newfoundland, there is no doubt if the sea had been calm, and free from any abrupt undulation, that the machinery would have gone as perfectly as a cotton-mill. What was the result? After they got about 350 miles it began to blow, the ship rose, and snap went the cable. That was with the old machinery.

864. (*Mr. Saward.*) You did not go out in that ship, I believe?—No; but I ascertained that fact from Captain Rivers; it must be the natural consequence.

865. (*Chairman.*) That took place in picking up the cable?—Yes.

866. Would not the cable then be vertical?—Yes, in both cases it is the same; here is the fact,—why did we break our cables? Mr. Gisborne says it has no effect whether the ship rises or not. I know there are men at the Institution of Civil Engineers who are of opinion, that if the vessel did not rise more than three or four feet the effect would be serious. I have known the log-line of a ship carried away by a sudden rise of the stern. It is the friction quite apart from the weight of the cable; we have absolutely the friction of the whole mass. Here is the result, if we are at this angle, and we cannot overcome the inertia of the machinery, instead of being at that angle we straightened the cable, and there is the friction of the water before we can alter the position of the cable; that is the cause of its snapping, no doubt. I believe the principal injury was done to the Atlantic cable by the paying-out apparatus.

867. (*Mr. Bidder.*) You have not noticed that, besides the mere elevation in vessels, there is an absolute translation of the mass of a vessel to a very great extent?—Yes.

868. Not only vertically, but horizontally?—The vessel yawing.

(*Mr. Longridge.*) There are certain mechanical questions which are so extremely simple to any person who understands them, that I think it is not fair to call upon Mr. Macintosh to substantiate his evidence upon that point by what he has actually experienced. This is just one of those questions. I have never laid a cable myself, consequently I have never seen a cable broken; but I know that those cables which are stated to have borne three or four tons' strain have broken under what they were pleased to call a tension of 30 cwt., and the indicator has not shown more. But 30 cwt. cannot break a cable which will

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bear four tons. The indicator, as I have explained already, is no guide whatever in these sudden jerks, because the indicator is subject to the same vice as the machinery, namely, the inertia; and when the last indicator which they used for the Atlantic cable was applied just at the very moment when the greatest strain came upon the cable, when it had acquired the maximum downward velocity, and consequently tended to show a diminution of the tension instead of an increase, a thing subject to mathematical reasoning and perfectly evident. (I am sorry I did not bring with me a paper which was drawn up by Mr. Woolhouse, an eminent mathematician). When the indicator shows the minimum tension, the tension is actually at the maximum, simply owing to the inertia of the indicator itself. It is quite easy to prove this. There is the formula in this book for it. When the cable is at 15°, which is just what Mr. Gisborne mentioned, and when a sudden rise takes place, such as I have frequently seen, without any such forward motion as Mr. Bidder has referred to, but when a sudden vertical rise takes place to the extent of 20 feet, in that case it may bring on a strain of 80 per cent. of the whole weight of the rotative machinery, and it cannot do otherwise. The strain varies as the sine of the angle; it varies inversely as the square of the time of the oscillation. But even at 15° it will amount, under some circumstances which I have witnessed, to 80 per cent. of the weight of the rotative machinery. Now in the case of the Atlantic cable, that rotative machinery was somewhere about 80 cwt.; 80 per cent. would be 64 cwt., or three tons and a quarter. I remember distinctly seeing myself, in some of the published accounts in the "Times," upon which I cannot lay my hand just now, that the indicator, under certain circumstances, oscillated from 1,500 to 6,000 lbs. It was mentioned by a person who professed to have seen it. I have no reason to doubt the truth of the statement, but to make the oscillation vary to the extent of 6,000 lbs. it must have had a very much greater pressure, close to the breaking strain of the cable. These are facts that I do not think it is fair to set aside, by asserting that a cable never broke from such a cause. Cables have boken, and we ought to call upon those who assert that this is not the cause to show what the cause is.

(*Mr. Gisborne.*) I alluded to cables with which I had been connected, the paying-out machinery was of a very different description, and in no instance that I am aware of was there the slightest danger to the cable from the heaving of the ship. I will add that we had no very rough weather.

(*Mr. Longridge.*) And no deep water perhaps.

(*Mr. Gisborne.*) 1,000 fathoms.

(*Mr. Longridge.*) That is not much. I have made these remarks because I feel that the chief cause of the failure of the Atlantic cable, and if not the failure, yet the injury to all deep sea cables, has been that very thing, the heavy paying-out apparatus required for heavy cables.

869. (*Chairman to Mr. Macintosh.*) Do you wish to make any further statement to the committee?—The only further remark that I have to make is with reference to an experiment that I have tried on some very pure pieces of india-rubber and gutta-percha as regards their absorption of moisture. I have found under great pressure that both gutta-percha and india-rubber absorb water, though to the eye they appear to be perfectly dry. The means by which I tested them was, taking out the copper wire and putting a wire of potassium into the gutta-percha, and hermetically sealing the ends carefully, then subjecting them to pressure. The way I subjected them to pressure simply consisted in a large piece of india-rubber thoroughly vulcanized placed between two hemispheres full of water, and pieces put in of various thicknesses, and with a lever of some six or seven feet long I got the required pressure; but I found in the Atlantic cable it took 23 or 24 weeks before the potassium became oxidized.

870. In every case, after a certain length of time, you produced oxidation of the potassium?—Yes; it is not from any decomposing influence in the gutta-percha.

871. (*Mr. Saward.*) What pressure did you use?—Three tons to the square inch.

872. (*Mr. Bidder.*) How did you propose to remedy that defect?—By putting between the layers of gutta-percha or india-rubber a substance that does not absorb water, for instance, that black stuff, sealing wax does not absorb water, and collodion will not absorb water.

873. (*Chairman.*) Did you weigh the specimens to ascertain what quantity of water was absorbed?—No, they were too small for that.

874. (*Mr. Bidder.*) Did you try experiments to ascertain how the cable would be affected by extreme pressure, if a cable were sunk 500 fathoms it would be exposed to a certain pressure per square inch, the pressure increasing with the depth?—The whole of the samples that I have tried were subjected to three tons, they varied from one coat up to three or four coats.

875. (*Mr. Saward.*) When you speak of gutta-percha being affected after 23 weeks' pressure, was that a single or a double covered specimen?—It was a bit of the Atlantic core with the wire taken out of it.

876. Did you take pains to ascertain that it was in fact a core which had had the copper in it?—Yes, I untwisted the copper out of it, apparently it was as perfect a specimen of gutta-percha as I have ever laid eyes on.

877. (*Chairman.*) Were any of the specimens made with Chatterton's compound?—No, there was no compound; it was pure gutta-percha.

878. And pure india-rubber?—Yes, they were not compounds.

879. (*Mr. Saward.*) You say that you drew the copper out of the Atlantic core; is it possible that your experiment might have been disturbed by any traces of the copper remaining in the gutta-percha, and affecting the potassium?—No, I do not think so. I had enveloped the potassium, which I had had for six or seven months, as well as phosphorus, in films of either india-rubber or gutta-percha, carefully getting all the atmospheric air out and hermetically sealing the whole, I found it would remain perfect. I have tried gutta-percha in which there was no wire, and I have found that it depended upon the purity and thickness of the gutta-percha as regards the time of decomposing the potassium.

880. Have you tried the experiment with gutta-percha so pure and so thick that the potassium was not affected?—No, I never went further than that. When I found that that was the result (indeed I knew that india-rubber and gutta-percha were both absorbents of water), the next object I had in view was to find that out. It struck me that collodion with asphalte, so as to get a skin to intervene between the various coats, would thoroughly prevent the water getting through. If it got through at the first trial it would not get through at the second. Lac will not absorb water, at least collodion will not absorb it, and gutta-percha or india-rubber vulcanized with bi-chloride of sulphur and bi-sulphate of carbon are not so readily acted upon by water as they are when pure. Another experiment that I have tried is this: I have two or three machines for making these cables, and by subjecting the india-rubber to a very great pressure at the time the series of coats are put on, there is no possibility of any air-hole or cavity remaining, indeed it constitutes one entire mass.

881. (*Mr. Bidder.*) Have you any specimens?—I have furnished specimens. You can go through the experiments very easily, because in putting the coats on the wire, in the course of a week you will find that the water will not penetrate.

882. Have you any specimens of wire covered with collodion?—I have specimens that I have tried with collodion; there is a lot of stuff sent up to the gutta-percha works now for the purpose of covering, I think there are two miles ordered.

883. (*Mr. Saward.*) And now in course of being

covered for experiment?—Yes, but the object I think was, to put two or three thin coats of collodion on the wire first, and to put the gutta-percha on afterwards.

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W. H. Woodhouse, Esq.

WILLIAM HENRY WOODHOUSE, Esq., examined.

884. (*Chairman.*) You are a civil engineer?—Yes.

885. Have you had considerable experience in the superintendence of the coiling and the submersion of telegraphic cables?—Yes.

886. What cables have you been principally concerned in laying?—Two or three Irish and English, and Irish and Scotch, in 1852 and 1853, and the Black Sea cable and the Atlantic cable.

887. Were you concerned in the construction of the Black Sea cable?—Not in the construction, but in coiling on board and laying of it.

888. Will you describe the process of laying the Black Sea cable?—It was a very light cable, merely a copper wire covered with gutta-percha, except the shore ends, about 10 miles each.

889. What was the length of the Black Sea cable?—Three hundred miles.

890. What length were the shore ends?—About 10 miles at each shore, and about 25 miles besides that from Varna to Cape Kaliakra, that length is through a bay in which ships frequently anchor.

891. Were there any particulars about the laying of that cable which would be of importance to the subject of our inquiry?—None, except that the Black Sea cable was the first cable laid with a cone and rings. Mr. Newall had invented that immediately previous to its being laid, and that was the first time of its being attempted, and was most successful; we had not the slightest difficulty, although it was such a light cable, and only bore a strain of two hundredweight. One day we had a beam wind rather heavy, yet we had not the least difficulty in laying it.

892. (*Mr. Saward.*) Was not that cable laid with a very small amount of slack?—Yes, a very small percentage.

893. (*Chairman.*) What was the per-centage of slack?—I believe it was ten miles; I am speaking from memory.

894. What was the nature of the bottom?—That we do not know, it was only 50 fathoms deep.

895. Have you been concerned in laying heavy cables as well as this light one?—Only one or two short lengths in shallow water.

896. In similar depths or in greater depths to that in which the Black Sea cable was laid?—150 fathoms between Port Patrick and Carrickfergus; I was with Mr. Newall upon that occasion.

897. Did you find great additional facility in laying a light cable?—Yes, we could lay it much faster.

898. Although it had no outer covering of any importance, was it injured in laying?—No, it had no outer covering, nothing but the gutta-percha; it was laid without any wheels or paying-out machinery, or anything of the kind; there were merely boiler tubes over the centre of the coil and a boiler tube at the taffrail.

899. There was no revolving machinery?—No machinery of any kind.

900. It merely slid over, in fact?—Exactly.

901. The Black Sea cable did not last very long in working order, I think?—It lasted till the war was over, I believe.

902. A year or two?—Not so much as that; about six months.

903. To what do you attribute its failure?—Breakage.

904. Broken by what?—Either by anchorage, or probably purposely.

905. Did you see the ends that were broken?—No, it has never been got up, I believe.

906. (*Mr. Saward.*) You do not know whether it was the shore piece that was broken or the gutta-percha portion that was broken?—I do not; Mr. Liddell told me the last time he went out that he should try to recover it; but I do not think the cause is known; I do not know it, at any rate.

907. (*Mr. Bidder.*) The actual cause of the stoppage was never ascertained, was it?—I believe not.

908. (*Chairman.*) Can you describe the Black Sea cable? Was it No. 16 copper wire?—Yes.

909. (*Professor Wheatstone.*) Was the failure of the cable sudden or gradual?—I believe it was sudden; I was in England when it failed; I had returned home.

910. (*Chairman.*) You were concerned with the Atlantic Company, I believe, were you not?—Yes.

911. For how long?—From the first, during the coiling on board and the laying in 1857 and 1858.

912. (*Mr. Bidder.*) Were you at the first laying?—Yes.

913. (*Chairman.*) Will you inform the committee what was the cause that led to the first accident to that cable?—Without a doubt, the accident in 1857 was occasioned by screwing the brakes too tight. They were different brakes in 1857 to those used in 1858. They were screwed up, the strain was indicated by a lever and a Salter's balance, and when they were unscrewed, the screw did not pull the brake back from the wheel, it left it jammed on the wheel till the wheel released itself. I was ill at the time; I had hurt my leg, nearly broken it, about six hours before, and I was in bed; there is not the slightest doubt that it was screwed up too tight; the machinery was stopped, but the ship went some 50 yards before the cable broke.

914. What distance from land was that?—We had paid out 335 nautical miles of cable, and the ship had run 275 miles.

915. That was starting from Valentia?—Yes.

916. Did you consider it prudent to start from Valentia?—No; I considered it was suicidal.

917. Will you explain why?—It would take twice as long to lay a cable across the Atlantic, if the laying was commenced from one side instead of from the middle; consequently there would be twice the chance of bad weather, and twice the chance of a gale. Supposing the cable to be started from one shore, where half the cable was laid, you would then have to make the splice; this would be very difficult in any weather, and in bad weather it would be utterly impossible. I believe that in above half the days in the summer months the splice could not be made, consequently half the cable, or say 1,000 miles, would be lost. I speak from having tried many times, both in experiments and in actual splicing.

918. (*Mr. Saward.*) By "difficulties," do you mean increasing depth?—No; I mean, suppose you are in water of 1,800 fathoms with a strain on the cable, and the wind blowing, that the cable never could hold the ship till the splice was made, which would take, perhaps, an hour and a half, or say, if made very quickly, an hour. I do not think the boats could be lowered and the end got on board the other ship, and the cable got off the machinery; I think it would be utterly impossible in bad weather, and in good weather it would be very doubtful indeed. If you start from the middle there is no strain on the cable, because you choose your day for splicing; you can wait till it is fine weather, as we waited two or three days in one instance; and if you have an accident, as we had twice, once after having paid out only two miles, you save the cable, for in that case you have only lost two miles but if you start from one side and the cable breaks, you will lose half your cable—1,000 miles of cable. Being half the time in laying in starting from the middle, you have only the chance of a gale.

919. (*Chairman.*) Do you consider that the specific gravity of the cable had anything to do with the loss?—Not in any way. Since I received your letter I have looked up the logs, and all the accounts and memoranda that I have, and I have prepared a statement showing the dynamometer strain and the brake strain, the dynamometer showing a strain in deep water of 1,800 lbs. and the brake about 200; the difference I consider due to the friction of the machinery.

The following Table was delivered in:

TABLE showing the Rate at which the ATLANTIC TELEGRAPH CABLE was paid out of the U.S. Steamer "Niagara," in 1858; also the Strain, Depth of Water, Weather, &c.

Date. 1858.	Time.	Latitude.	Longitude.	Weather.	Electrician's report.	Depth of water in fathoms.	Dynamometer strain in lbs.	Brake strain in lbs.	Speed of cable per hour.	Speed of Ship per hour by observation.	Speed of Ship per hour by log.	Miles of cable paid out.	Ship's run by observation.	Ship's run by log.	Percentage of cable lost.	REMARKS.
		° ′	° ′						mls. fms.	mls. fms.	miles.	mls. fms.	miles.	miles.		
July 29	1.0 PM	50 09	32 27	Light, wind S.E.	Signals perfect.	1,550 -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	The splice was made at 1 o'clock p.m.; distance to the end of Trinity Bay, 822 knots, to Telegraph House, 832 knots. Cable on board, 1,100 knots, or 24½ per cent. over the distance to be run.
"	7.45 PM	- -	- -	- -	Continuity gone.	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	At 7.45 p.m. signals from the "Agamemnon" ceased. Electricians reported insulation perfect, but continuity gone; paid out from the "Niagara" very slowly till 9 p.m., when electricians reported signals perfect. N.B.—This was caused by their cutting out a bad piece of cable on board the "Agamemnon."
July 30	Noon.	51 50	34 49	thick and rainy, wind from S.E. some sea.	-	-	1,550 to 1,975	2,000 to 2,050	1,800 to 1,900	5 735	3 870	4·565	131 900	89	105·	48	The principal cause of this great loss was, that Captain Hudson steered by his own compasses, which were wrong from local attraction. After the first day we were steered by the "Gorgon."
"	3.50 AM	- -	- -	- -	-	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	-	At 3.50 a.m., finished spar-deck coil, and commenced main-deck coil.
July 31	Noon.	51 05	38 28	cloudy, with rain, winds S.E.	-	-	1,675 to 2,250	2,000 to 2,050	1,800 - -	6 660	5 7 10	5·733	159 843	137	137·6	17	
"	2.45 PM	- -	- -	- -	-	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	-	At 2.45 p.m. received signal from "Agamemnon," that they had paid out 300 miles of cable.
"	5.37 PM	- -	- -	- -	-	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	-	At 5.37 p.m., finished paying out coil on main deck, and commenced lower-deck coil.
Aug. 1	Noon.	53 32	4155 W.	cloudy and misty, with squalls and heavy wind.	-	-	1,950 to 2,424	2,000 to 2,075	1,800 - -	6 862	6 046	5·904	164 683	145	141·7	14	Total amount of cable paid out, 456 miles, 400 fathoms.
"	3.05 PM	- -	- -	- -	-	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	-	At 3.05. p.m., finished paying out lower-deck coil, and commenced coil in the hold.
Aug. 2	Noon.	49 52 N.	45 37 W.	cloudy, wind, N.	-	-	1,600 to 2,385	2,050 to 2,300	1,800 to 2,200	7 381	6 417	5·900	177 150	154 ·	141·3	15	Total amount of cable paid out, 633 miles, 500 fathoms. The "Niagara," getting light, rolled very heavily. It was not considered safe to carry sail to steady ship, as, in case of accident, it might be necessary to stop the ship as speedily as possible.
"	12.38 AM	- -	- -	heavy swell.	Insulation imperfect	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	-	At 12.38 a.m., imperfect insulation of cable detected in sending and receiving signals from the "Agamemnon," which continued to 5.48 p.m., when the fault was found in the ward-room coil on board this ship, about 60 miles from the lower end, and taken out of the circuit.
Aug. 3	Noon.	49 17	49 23	calm	-	-	742 to 1,827	1,200 to 2,000	1,200 to 2,000	6 740	6 125	5·604	161 763	147	134·5	10	At 8.26 a.m., finished paying out from hold, and commenced from ward-room coil.
"	9.10 PM	- -	- -	- -	-	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	-	At 9.10 p.m., received signals from "Agamemnon," that she was in water of 200 fathoms.
"	10.20 PM	- -	- -	- -	-	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	-	At 10.20 p.m., sent signals to the "Agamemnon," that we were in water of 200 fathoms.
Aug. 4	Noon.	48 17	53 43	calm	-	-	less than 200	850 to 1,200	800 to 1,200	6 431	6 083	5·917	154 360	146 ·	142·	6	At 8.0 a.m., made land of entrance to Trinity Bay. At 6.30 p.m., entered into Trinity Bay.
"	7.30 PM	- -	- -	- -	-	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	-	At 7.30 p.m., Captain Otter, of H.M. Steamer "Porcupine," came on board to pilot us up to the anchorage near Telegraph House.
Aug. 5	1.45 AM	- -	- -	- -	-	- - -	- - -	400 to 1,000	400 to 1,000	4 828	4 654	- -	66 383	64	- -	4	At 1.45. a.m., "Niagara" anchored off Telegraph House, Bay of Bull's Arm.
"	5.15 AM	- -	- -	- -	-	- - -	- - -	- - -	- -	- -	- -	- -	- -	- -	- -	-	At 5.15 a.m., end of cable was landed on shore.

	Miles.	Fathoms.
Total length of cable paid out - - - - -	1,016	- 600
Total distance run by observation - - - - -	882	- 000
Amount of cable paid out over distance - - - - -	134	- 600 being a surplus of about 15 per cent.

920. Do you consider that there was "vice" in the original machinery altogether?—Except in screwing on the brakes I do not consider that there was; it was a good machine.

921. The brakes were perfect?—Yes, I consider the brake used in 1858 was perfect; having to trust to any one to use his own judgment must be bad. The principle of that brake was invented by Mr. Appold, and it would be impossible to put an extra strain without there came a sudden jerk.

922. Do you think that the dynamometer used on either or both occasions gave true indications of the strain from time to time?—I believe it was obliged to do it; it was impossible it should be otherwise. After passing the wheel which was on the top of the cable, there was only the friction of two wheels; therefore it must be true.

923. In the case of a cable hanging to the ship, over the stern of the ship, with a lift of the stern, say 20 feet clear, would you then consider that these dynamometers, or either of them, gave a true indication of that sudden lurch?—I should consider that the dynamometer would give a true indication, but I think the brake would not. I think the inertia of such a vast body of metal would affect the brake, but the dynamometer would show it.

924. What was the inertia of the dynamometer itself?—I have not a drawing of the machinery; I think about 40 or 50 lbs.

925. (*Mr. Bidder.*) Were you present at the coiling of the cable?—I had the superintendence of the coiling on board the "Niagara" from first to last, not on board the "Agamemnon."

926. Was the cable tested as a whole under water?—It was never tested in any way under water after it came to Keyham; I had nothing to do with the rope before in any way.

927. Were you present at the experimental trip which preceded the last trip?—Yes.

928. Did not the cable, on that occasion, break under a variety of circumstances?—It did.

929. Was it the fact that the cable generally broke when it was nearly up and down?—It broke generally when we were hauling it on board, consequently it would be when it was up and down, when there was no way upon the ship. There were two other instances of its breaking.

930. I am only speaking of information which came through the "Times;" I wish to know how far that is correct information. It was said that upon that experimental trip the fracture took place when the vessel was at rest generally?—It did.

931. Was not there an experimental trip, or rather an attempt made which failed in the first instance, of laying a cable before starting from Valentia?—There was an experiment made in going round from Cork to Valentia.

932. Where the cable again broke?—Yes.

933. Was that when the vessel was going at speed?—It was broken purposely. We tried how much it would bear to see what liberty we could take with it; we were getting near Valentia, and we must have given over the experiment, and as the cable was worthless, the brakes were put on, to see what liberties we might take.

934–5. (*Chairman.*) Was not the first experiment to which Mr. Bidder alluded in the Bay of Biscay?—Yes, in 1858.

936. (*Mr. Saward.*) You went into 1,600 fathoms, I believe?—Yes; and more than the depth of the Atlantic.

937. The ships were there together?—Yes; the experiments were made together as to splicing and hauling on board.

938. Is it not the fact that on that occasion the cable was hauled in from a great depth?—Yes, 2,500 fathoms.

939. Was there a weight at the end of the cable?—It was hauled up with the splice at the end of it.

940. What was the weight of that splice?—Three-quarters of a hundredweight.

941. Was not the cable hauled in without breakage?—Yes.

942. (*Chairman.*) Was it hauled up from the bottom?—The splice came up to the level of the water without breakage, but we did not get the ball attached to the splice on board that weighed ¾ cwt.

943. (*Mr. Bidder.*) What was the state of the weather?—You would call it a fresh sea; not by any means calm, nor was it rough.

944. (*Chairman.*) Was the electrical condition of the cable injured by raising it?—I had nothing to do with the electrical department from the beginning to the end; I believe it was injured by raising it.

945. (*Mr. Saward.*) In paying out the cable on the first occasion and subsequently, have you often watched it descend from the stern of the ship often?—Of course; it was my duty.

946. Have you observed any tendency in the spiral to unlay?—Yes.

947. Do you agree that it would unlay to a dangerous extent in deep water so as to expose it?—I do not think it would; when both ends of a cable are fast, it cannot unlay for any distance; what may be unlaid in one place must be laid up in another.

948. If the spiral unlaid in one direction, would not it be in the course of laying tighter in another direction further down; I mean, did it unlay, or did it lay the twist tighter?—It was unlaying.

949. Are you familiar with Mr. Allen's cable?—With the principles of it.

950. Is not the principle of that cable that the strength should be in the centre? In any modification of it the strength would still be underneath the external covering and bedded in some plastic substance. Would you consider a cable so constructed as suitable for laying in deep water, or as suitably constructed as a spiral cable?—No, I think not.

951. Will you be good enough to explain why?—I think if it lay over a sharp ridge of rock it must be inevitably cut through; it is a mere matter of time.

952. Supposing there were longitudinal layers of tolerably thick iron or steel wire inside laid all round, and then covered with the plastic substance, would you consider that objectionable?—It would be better than the other; but I do not see the advantage of it. I do not see why there is any advantage over the old cable, insulating the copper wire and protecting the insulator.

953. Would it be more or less expensive, in your judgment?—I should say more expensive.

954. Without corresponding advantages?—I do not see any advantages to be derived from it.

955. (*Chairman.*) Is the decrease of specific gravity any advantage or not?—As you will see from the table I have handed in, we never had a ton of strain on the iron wire; I do not think it is material.

956. (*Mr. Bidder.*) *Cæteris paribus*, would not you prefer a light cable to a heavy one?—If it were as strong, I should certainly.

957. Being as strong and as good an insulator, should not you prefer a light cable?—Yes, with one exception; if you had a very strong current indeed at right angles, I think a very light cable which would only just sink, and sink very slowly indeed, would be a disadvantage.

958. Why?—I think it would wash out by the lateral current; the ship going one way and the current going the other.

959. Do not the ship and the current go together?—No, you keep your ship up by steam power.

960. If the ship and the current of the ocean are all going together, you may throw out more cable. I should like you to explain, if you can, why that should throw more strain upon the cable?—We will take the case of the Thames at Waterloo bridge. Supposing you started from one side, and rowed a boat to the other side of the bridge, and parallel to it, towing a light cable, the stream would wash the cable down the river; it would not remain parallel to the bridge.

961. (*Chairman.*) You have stated that you had

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great facility in the laying of the Varna and Balaclava cable, which was a light cable?—Yes, a very light cable indeed.

962. Should you have found the same difficulties in laying the cable across the Atlantic if it had been as light as that which you experienced with a heavier cable?—I do not think there would have been any great difficulty in laying a very light cable across the Atlantic, but I should not advise anybody to lay as light a cable as the Black Sea cable.

963. For what reason?—Because so small a strain will break it. If it is once safe at the bottom perhaps it may rest, but if it is lying on a rock in 200 fathoms water, where there is a current, I think it will be most liable to be injured.

964. Then it is a question of strength and protection?—Yes, and liability to injury.

965. (*Mr. Bidder.*) As a mere question of laying you would rather lay a light cable than a heavy one?—I would.

966. (*Mr. Saward.*) Reverting to Mr. Allan's cable, if specific gravity be an object, could it not be obtained in the one case as readily as in the other by enveloping it in a lighter covering?—I should say it could.

967. (*Chairman.*) Were not you in the "Niagara" when the cable was successfully laid?—Yes.

968. There were one or two failures attending it?—Yes.

969. Will you describe the cause of those failures shortly?—The first time we sailed in 1858 on the 10th of June. We met with a gale in going out, of which probably you have all heard. We spliced on the 26th of June, and when 2 miles 40 fathoms had been paid out from the Niagara, the cable ran off the wheels from being slack. We had a little brake to make it run tight on the large wheel. It was not put on in the hurry of starting, in fact it was an oversight, the cable jumped off the wheel and broke, and we lost 2 miles 40 fathoms.

970. Did you attribute that to the apparatus?—To an oversight. We commenced again at 5.30, and at 10 minutes past one on the 27th, the next day, the signals ceased, and the electricians reported that the cable had parted at a considerable distance. We supposed it to have been on board the "Agamemnon," and those on board the "Agamemnon" supposed the "Niagara" had broken it. We lost on that occasion 42 miles. At 7 p.m: the splice was again made. When 142 miles of cable had been paid out, the electricians reported that the cable had parted at or near the "Agamemnon," the wind was fresh. The cable anchored the ship 1 hour 45 minutes in 1,650 fathoms of water.

971. On both these occasions the cable parted when the ship was proceeding at some speed?—On the first occasion the electricians reported that the cable was parted, but we never ascertained where.

972. It was not parted?—Not near either of the ships. I think it parted at the splice; but at any rate the electricians thought that it was broken on board the "Agamemnon," and the others thought that it was broken on board the "Niagara."

973. (*Mr. Bidder.*) You never ascertained the cause?—No; it was the most unsatisfactory failure that we had.

974. (*Mr. Saward.*) That was on the second attempt?—Yes.

975. Then came the loss of the 140 miles, when it broke at the stern of the "Agamemnon." It was after that occasion that you stoppered it to the stern of the "Niagara," and anchored for the time you have mentioned?—Yes.

976. (*Mr. Bidder.*) What happened then?—It parted close to the stern of the ship.

977. (*Chairman.*) Was it then vertical?—No, it was a fresh wind; it was merely anchoring the ship, and the ship hung on to the cable till it parted.

978. (*Mr. Bidder.*) The ship had no kind of way on her?—No, her sails were down, and all was perfectly still.

979. What prevented the cable being up and down?—The wind.

980. On the hull?—Merely on the hull.

981. Did you observe the angle of the cable?—No, it was a very dark night.

982. Whether the cable was going up and down you cannot tell?—I know it was not up and down, we could see it going out, but we did not take the angle.

983. (*Chairman.*) On that occasion it parted near the stern of the Agamemnon?—It had parted close to the stern of the Agamemnon.

984. What were the circumstances attending that parting?—I do not know; Sir Charles Bright and Mr. Canning told us that they had not much strain, but I think there must be some mistake somewhere; some wheel must have been jammed, I think.

985. What was the next proceeding?—Then we went back to Cork, and went back, and the effort was successful.

986. No failure occurred?—No, not in any way.

987. (*Mr. Bidder.*) On the next laying of the cable, the last time, you kept the vessel going, I think, at a pretty good speed all the way, generally speaking?—Generally speaking, we did. The average speed of the first day was only 3 miles 870 fathoms, but that occurred in this way: Captain Hudson determined to steer himself, and his compasses were wrong from local attraction. The patent log showed 106 miles run, and the miles run by observation were only 89, showing that we had steered a very bad course.

988. You may not have made in the direct course above three miles, but the vessel must have been going four and a half miles per hour?—Precisely; 3 miles 870 fathoms is the speed on the 89 miles, but the actual speed of the vessel was much greater. The next day the average speed was 5 miles 710 fathoms.

989. By log?—By observation.

990. What does the log give?—5 miles 733 fathoms.

991. (*Mr. Saward.*) Do you attribute the misfortunes of the Atlantic Telegraph Company to any extent to the pledge given by the original promoters that an attempt should be made to lay the cable in 1857, and the consequent hurry with which everything was done?—In the first instance I have not the slightest doubt of it; I do not mean to say that we should have joined in the middle if we had laid the cable so far, but we had no chance from the way we were hurried.

992. Everything was necessarily deficient in experiment and previous observation?—Yes; so unfit were we to start that the machines were literally being put together as we were going round from Cork Harbour to Valentia Bay.

993. (*Chairman.*) You mean the paying-out apparatus?—Yes, they never were worked until we got round to Valentia Bay. In 1857, on the first attempt there was so much anxiety on the part of the directors to get us away that we were not at all in a fit condition to start.

994. (*Mr. Bidder.*) When the cable broke, was it from imperfection or rather want of application of the machinery, or had the cable got jammed and jumped off the wheels; how long after was it that it broke instantaneously, or did any time elapse?—Very soon afterwards; I was in bed at the time, but from what I heard away from it, at a guess I should say that it broke in half a minute.

995. The vessel keeping going?—We could not stop her way before the cable broke.

996. (*Chairman.*) At what angle was the cable paid out on the occasion of the successful laying?—From 12° to 15° with the horizon.

997. (*Mr. Bidder.*) You kept very little pressure on the brake, of course?—You will see the brake strain and the dynamometer in the table which I have given in.

998. (*Mr. Saward.*) What is the breaking strain of the Atlantic cable?—About 3 tons 15 cwt. is said to be the breaking strain, but on the occasion of its anchoring the ship for one hour and 45 minutes it bore

four tons, according to the dynamometer, before it broke.

999. (*Chairman.*) You have mentioned in the course of your evidence the lay being one part in an opposite direction to the other; do you attribute any inconvenience to that?—Not if you get 50 miles or 100 miles away from one another; at first undoubtedly it is a great disadvantage.

1000. Do you attribute one fracture to that cause?—I cannot say that it did occur on that account, I cannot understand how otherwise.

1001. (*Mr. Saward.*) Do you consider that the experience gained by the repeated attempts to lay the Atlantic cable will, if a cable is properly made and once laid, result in permanent success?—I have very little doubt of it.

1002. You are not aware of any inherent difficulties that are not easily surmountable?—None.

1003. Should you anticipate that a proper cable properly deposited at the bottom of the Atlantic Ocean in deep water would be a permanent institution?—Certainly, as far as the mechanical part of it goes, I should say so; I know nothing of the electrical part.

1004. (*Chairman.*) Did you remark whether the pitching of the vessel had a very great effect upon the cable during the paying out?—Almost imperceptible.

1005. To what cause do you attribute that?—From the angle at which it was paid out.

1006. You think if the cable is kept at an angle of from 10° to 15° over the stern of the vessel, that the pitch of that vessel will not endanger the breaking of the cable?—I think not.

1007. Is not the danger to the cable very much measured by the angle at which it leaves the vessel?—Certainly; if it is nearly up and down the danger would be very much greater, and I think then the pitching of the vessel might break it.

1008. Have you found in picking up the cable, where the cable is up and down, that any sudden change in the position of the vessel was apt to break it?—Certainly.

1009. But not when the cable is out at an angle?—No.

1010. (*Mr. Bidder.*) The lighter the cable the less the angle, and the less the danger?—Certainly, in paying out; but in picking up, a light cable would be up and down.

1011. Had you any very heavy weather?—We had a very heavy swell one day.

1012. Was that a ground swell?—There was some wind with it.

1013. (*Chairman.*) Did the vessel go up 20 feet?—No, not so much, I think.

1014. (*Mr. Saward.*) You were out in the "Niagara" at the time that the heavy gale occurred to the "Agamemnon"?—Yes.

1015. Did you observe what the greatest roll of the "Niagara" was during that time?—Yes, she rolled at an angle of 45°. I do not know what the pitching was.

1016. About how much was it?—Nothing, in comparison.

1017. (*Chairman.*) The cable had been coiled before the successful laying at Keyham?—Yes.

1018. Were you concerned in the coiling?—No; I had hurt my leg, it had been very nearly broken, and I was laid up for six weeks.

1019. Was the cable injured in coiling and uncoiling?—I am afraid it was injured.

1020. (*Mr. Bidder.*) Was it injured in strength?—By kinking in a great many places.

1021. (*Chairman.*) How many times was the cable coiled and uncoiled that went on board the "Niagara"?—It was coiled, of course, in the warehouse after being made; it was then coiled into the tender at Liverpool, to be taken to the "Niagara," which was lying in the river; it was then coiled on board the "Niagara," and from that it was uncoiled in Keyham, and coiled again on board the "Niagara."

1022. Would that frequent coiling and uncoiling be likely to be injurious to the cable?—Yes. It was carefully examined; I had two men the whole time, night and day, while it was being coiled on board, examining it for bad places; and wherever the bad places occurred they were cut out; a great many places were cut out.

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1023. Did not that increase the number of joints very much?—Yes.

1024. Of course you could only detect those injuries which were very apparent?—Of course.

1025. (*Mr. Saward.*) Do not you think that a minute injury unobserved, and impossible to have been observed, may have gone over and caused a difficulty; for example, an interior injury to the gutta-percha?—Certainly, that is very possible indeed; for instance, a nail; it was covered with a wood roof, and a single nail would have injured the insulation or destroyed it.

1026. (*Chairman.*) With regard to the simple question of laying, as you have laid a cable without machinery and with machinery, would you prefer the strength of the cable and its specific gravity to be such as to require the use of machinery, or that you were able to do without it?—I would rather have to use machinery decidedly.

1027. Do you think, as a question of laying, if you can keep a regular strain upon the cable by machinery, it is safer than allowing it to run out free?—In running out the Black Sea cable we had the greatest difficulty with these tubes; the gutta-percha cut through the boiler tubes, almost as deep as the cable, till it got through continually, and we had to keep shifting it all the way. It was a very rude way of running it over, and I would rather have run it over by machinery, having some control over it, than run it absolutely loose in that way. We had no indication of what cable we were paying out.

1028. Supposing you have a cable, the breaking strain of which is a ton, would you sooner pay out that cable loose by itself or run it over with machinery, upon which you could bring a strain of five hundredweight?—I should not like to pay out a cable, the breaking strain of which is a ton, without machinery; it would simply be another experiment, and I think a very dangerous one.

1029. As a question of safety simply, irrespective of the per-centage of slack?—I think that the machinery might be made sufficiently light.

1030. Do you think the inertia of the machinery, even with so light a cable, would be an important element in its destruction?—Certainly not; a machine could be made to suit the cable.

1031. In all cases you think it better to adapt the machinery to the strength of the cable than to let the cable run out loose?—Yes.

1032. Do not you admit that, as you increase the specific gravity, and consequently the strain of your cable, you must increase the weight of your paying-out machinery?—No doubt.

1033. You require a heavier rotative apparatus with a heavy cable like the Atlantic, than you would with one of half its specific gravity?—No doubt.

1034. Will not the inertia of that apparatus increase as you increase the weight?—It will.

1035. Therefore it is a mere question of degree to what extent you would carry the opinion which you have expressed?—It is.

1036. If it were necessary to have paying-out apparatus so heavy that the inertia brought a serious strain on the cable, you would rather not use that machine, I suppose?—But I think that is impossible.

1037. Have you ever made any calculations?—The machinery that we used for paying this cable out, which only bore a strain of from three tons 15 cwt. to four tons, was quite strong and heavy enough to pay out a cable five tons to a mile, in fact to lay the shore ends.

1038. (*Mr. Bidder.*) Therefore you would lighten it?—Yes, if I had only that cable to lay out. The machines were strong enough to lay out the shore ends of the Atlantic cable also.

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1039. (*Chairman.*) You think it an objection to have inertia in paying-out apparatus if you can avoid it?—Yes.

1040. You have mentioned that the indicator never exhibited above a certain strain?—Yes.

1041. Supposing that the indicator were descending in a given direction, and a sudden change in motion took place, is not the tendency of the weight to keep on its downward motion, whereas the tendency of the indicator is to indicate a decreased strain, and not an increased strain?—Yes.

1042. Consequently, at the very time when the greatest tension is on the cable the indicator is indicating a less strain?—Possibly.

1043. Therefore the error in the indicator is exactly in the opposite direction to the strain of the cable?—You are perfectly correct in theory, but not in practice. I think if it went down with a jerk at any speed, I will not say that it would be perfectly correct; but a body of 5,000 tons, such as the "Niagara," cannot go with jerks like a fishing smack; it must go easily, and return very easily. I do not think that you are right in practice.

1044. (*Mr. Bidder.*) I gather from you that your view seems to be that you would only use the paying-out apparatus for the purpose of registering what you were doing, making it but little applicable for the purpose of controlling the cable?—It is necessary to know what strain is on the cable, and without a paying-out apparatus, let the cable be ever so light, you do not know the strain; it is also necessary to know what relative pace the ship and the cable are going in the water; it is of no use to know the speed of the ship without you have the speed of the cable also. To attempt to lay a cable across the Atlantic without any paying-out apparatus would, in my opinion, be a very dangerous experiment.

1045. I gather from what you have said, that your mind seems to incline to the idea that the value of paying-out apparatus is rather more for registering how much cable has gone out than for any other purpose?—That is one of the advantages; but I say, another is that it is necessary to see what strain you have on; there might be some particular circumstances that made that very important.

1046. If the cable runs loose you would have no strain at all?—It never can run absolutely loose, when you have the rings and cone to deal with, at any rate; the friction of the rings and cone, and the pulleys over the coil, is more in practice than would be supposed in theory.

1047. They are constant, are they not?—No, they are not constant, they vary very materially; the friction of the rings, and I think Mr. Gisborne will bear me out in that, speaking merely from practice, varies very much. I have seen cases after a gale of wind when the rings, but for the guys, would have been pulled out of place by the cable; that is a strain which would be indicated by machinery, but if the cable is running loose you would break it without knowing the reason.

1048. (*Chairman.*) Do you consider properly designed paying-out apparatus a source of danger or of safety to a cable?—That depends entirely on the description of cable.

Mr. JOHN CHATTERTON examined.

Mr. J. Chatterton.

1049. (*Chairman.*) You are the manager of the gutta-percha works, I believe?—I am the manager of the engineering department.

1050. Will you be so good as to describe to the committee the process which the gutta-percha undergoes before it is placed in a core?—As far as I can consistently with the interests of the Gutta-percha Company. The imported blocks as they come into the establishment are selected for their different purposes and stored away.

1051. Is it the fact that no adulteration of gutta-percha takes place either before it comes to England or after its arrival?—I do not know that. Some iron shot and stones are found in the blocks. Any matters mixed with gutta-percha by the natives would be separated by the cleansing process we adopt.

1052. It comes to your establishment in a rough state?—Yes; after the gutta-percha is selected and stored away in vaults properly apportioned for the different sorts, the blocks are cut up into small particles, and undergo a thorough cleansing process, and from that cleansing process the gutta-percha goes into the boiling tanks; it then assumes a sort of plastic state, and in that state it is subjected to what we may describe as the combing process, which also has a tendency to cleanse it still further, and before it is masticated, as we term it, or kneaded, it is passed through very fine gauze. After it has undergone a certain time of kneading, it is ready then in a plastic state to be moulded into anything, or put into the wire machines to be put on the wires.

1053. Is it more advantageous to put gutta-percha on the wires in a thin coat than in a thick coat?—In a thin coat certainly.

1054. For what reason?—In the system that we adopt in our machines a certain quantity of air does unquestionably pass into the machines, and in putting the gutta-percha on in thin coatings, the gutta-percha being in a plastic state, has not strength to contain the air; any air hole must exhibit itself on the surface after it has passed through the die, and as every coating is looked over very carefully, these air holes or imperfections are repaired.

1055. Is gutta-percha more dense when it is put on in a thin coat than it is in a thick coat?—It is subjected to rather more pressure. I cannot tell exactly what it is.

1056. Do you know at all what pressure the gutta-percha is subjected to in passing through the dies?—No, I cannot describe it. The power that we apply is very great; the hole is very small through which it is forced.

1057. Do not the number of coats which are put upon the wire add very materially to the expense?—They do.

1058. Which is the greater cost, the cost of labour or the cost of the material?—The cost of the material is very great; and then with regard to every additional process to which the wire is subjected, that is to say, putting six coats on to a certain sized wire, the expense would be very considerably more than putting on the same weight of material in three.

1059. Can you state what the expense would be of a wire of this description (*handing to the witness a specimen of* 20 *coated wire*)?—It would be very expensive, but I am not prepared to say at what price such a wire could be supplied at in quantity. Of course, an estimate can be formed if required.

1060. Would it multiply the cost by some 15 times?—Not so much as that, because if we do not put so much gutta-percha on at one operation, the wire goes rather faster through the machine; so that it is not quite so expensive as you may imagine.

1061. Have you prepared or patented a compound?—I am very glad to take this opportunity of explaining exactly the part I have taken with respect to the compound. By some means it is known as Chatterton's compound. In fact I invented the plan of putting on any suitable compound, but the merit of the mixture we use really and truly belongs to Mr. Willoughby Smith, who was examined here yesterday. We wanted a compound which we could put on at the same time with the different coats of gutta-percha; he tried several mixtures, and produced the compound we now use as the most perfect.

1062. What are the advantages which you assume that compound possesses, or some of them?—I think I can only explain the advantages by telling you that the perfect or rather the improved insulation we get by the application of this compound is very remark-

able; although I cannot answer you electrical questions, I do see that in testing wire having the compound introduced between the coats of gutta-percha, the insulation is very superior to anything we have been able to produce without the compound.

1063. As much as 50 per cent.?—Yes, I think fully that; I think Mr. Smith produced before you some tables which show the daily tests.

1064. It is a better insulator, and does it suffer less from temperature than gutta-percha?—I think not; it becomes plastic at a less temperature than gutta-percha.

1065. In the diagrams that were exhibited to the committee by Mr. Willoughby Smith, the compound did not seem to be so much affected by change of temperature as the gutta-percha?—No.

1066. Have you made any experiments upon the permeability of gutta-percha by water?—Not under pressure; merely by long continued immersion.

1067. What results have you found?—We find that there is not the slightest variation in either sound gutta-percha wire or sound compound; they do not vary at all by immersion as to time, but they do as to temperature.

1068. Have you made experiments upon gutta-percha in water subject to pressure?—I have not; I have never seen any tested under pressure.

1069. Is not it the fact that the price of gutta-percha has risen very much?—Very much indeed.

1070. Is that owing to the increase of price upon importation, or to the extra processes which it is subject to?—To the price of the raw material.

1071. Do you attribute it to any diminution in the supply?—I think the difficulty of getting gutta-percha increases every year.

1072. Do you think that precautions have not been taken to increase the supply by planting trees?—I think there was a large sum of money sent out some years ago to try to induce the natives to renew the plantation, but it failed for some reason or other. There was some prejudice existing, and the object was not obtained.

1073. Does not gutta-percha principally come from Borneo?—Singapore is the principal place from which it comes.

1074. (*Mr. Saward.*) Do you consider, since the increased price of gutta-percha, that any deterioration in the quality has taken place, comparing the gutta-percha used in 1851 with the gutta-percha imported at the present day and used?—I think now that we get equally good gutta-percha as ever, but there is a great quantity of inferior material comes over, in consequence of the great price (because it is used for many other purposes besides covering wires); and a good deal has appeared in the London market, which otherwise would hardly have been worth the cost of transit.

1075. (*Chairman.*) Do you believe that gutta-percha in submarine cables can deteriorate at all?—I think not; and I have every reason to believe so, inasmuch as I have never seen any symptoms of decay exhibited in the gutta-percha cores that have been submerged.

1076. Do you consider that the cable which has been taken up from the Dover and Calais line, which was laid down in 1851, is as good as when it was laid?—I think it is; I do not know how long this specimen has been out of the water, but it is in a beautiful state now.

1077. It is as good as it was when it was laid?—Fully as good.

1078. You have had experience in the manufacture and the laying of cables?—I have had nothing to do with the laying of cables; I have not been out with any one of them.

1079. You have had considerable experience in the manufacture?—Yes; I have been interested in insulated wires for some years. In 1851 I took out a patent for covering gutta-percha wires with lead; so that I have had a good deal of experience in gutta-percha wires.

1080. Have you considered the question of covering the wires with india-rubber?—Not exclusively with india-rubber. I have tried several experiments in making a compound or mixture of india-rubber with gutta-percha.

1081. Do you consider that that forms a good insulating covering?—It is very useful in its way, but I should not adopt it for insulating wires; it is not so good as gutta-percha, but it forms an excellent protection to insulated wires as an outer covering.

1082. You consider that pure gutta-percha is the best insulator?—Yes, with the assistance or the accession of coats, as it were, of the compound previously mentioned.

1083. Do you know the results which were afforded of a small test which has been made upon that wire with the numerous coats?—Yes; I have been given to understand that it is perfect.

1084. More perfect than any other?—It is the most perfect that has yet been produced.

1085. Have you tried Professor Hughes's compound?—We have made two miles of it; I believe one mile is sent away to-day for the Government experiments.

1086. Do you think that that process has any advantages?—I have not been able to discover them at present. Certainly the insulation is very good; and the process of mechanical repairing is very curious, but I do not understand in what case it would be likely to be available practically.

1087. Do you think it would be better to take great care in covering a wire with numerous coats of gutta-percha?—Yes, with these layers alternately of compound and gutta-percha I think we get very perfect insulation, and moreover we get the wire equally coated all round.

1088. So as to prevent eccentricity?—Yes.

1089. Will you look at that specimen (*handing the same to the witness*). These are air-holes, are they not?—Yes.

1090. To what do you attribute them?—In this case air must have passed through the machine into the gutta-percha, and not been noticed; very carelessly examined.

1091. Ought these air-holes to have been observed by the person who examined the wire?—I think that these air-holes ought to have been detected, though the wire may have been subjected to pressure, which has developed them more than they were when examined.

1092. Is the best mode of guarding against air-holes to put on numerous thin layers?—It is the best mode, there can be no question. From the experiments we have tried lately, we have got very beautiful results from the thin coverings, carried out to their fullest extent. At Mr. Gisborne's request, we are putting them on to a core the size of the Gibraltar, twelve coats instead of six, to see if they will improve the insulation, though with six coats it is wonderfully good.

1093. Have you considered at all Mr. Macintosh's process of vulcanizing gutta-percha?—I have. With respect to air-holes, we have never found from the reports we have had, and I admit we have had complaints of air-holes existing; they have been accompanied with this declaration, that in no instance have they found the insulation impaired by them.

1094. Have you not had a complaint from Messrs. Newall of those air-holes, and was not some wire returned to you in consequence of them, because it was badly insulated?—No, not through the air-holes.

1095. (*Mr. Saward.*) Would not the chances be very much indeed against these air-holes being opposite to each other down to the conductor?—Yes, the air-holes in the first coating or the second coating, if they were allowed to pass examination, in all probability would be burst by the process of putting on the subsequent coating.

1096. (*Chairman.*) Or driven up the wire?—No, they could not be driven; the air would not run at

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all. If you were to cut a wire, you would not find air holes exist in any but the outer covering.

1097. Taking the case of the Gibraltar core: it is tested under pressure in a vacuum after the outer covering is put on; suppose there were air-holes in the inner covering, would they be detected by that pressure?—I think not.

1098. Have you any objection to testing cables in water?—Not the least in the world.

1099. Do you consider that testing cables under pressure in water forces the water into the gutta-percha?—If gutta-percha really and truly be porous, no doubt, under pressure, the water would be more likely to go into it.

1100. If gutta-percha is porous, is it not merely applying artificially the pressure to which it would be subjected when it gets to the bottom of the sea?—Yes, it would be so.

1101. If gutta-percha is porous, and is subjected to great pressure at the bottom of the sea, will it become a non-insulating material?—Yes, to the extent of the water that is forced into it by the pressure of the depth.

1102. If it is permeable, will it answer permanently for deep sea cables?—No; if the water permeated gutta-percha sufficiently to interfere with the insulation, of course it would be very much deteriorated in working.

1103. (*Mr. Saward.*) Have you ever known a case in which these air-holes existed all through the coatings in the same place?—Never. My belief is, that in the process of putting on the thin coatings of gutta-percha, the last coating would burst or break any air holes that had escaped inspection in the previous covering.

1104. (*Chairman.*) And fill up the space?—Yes. I say that with confidence, inasmuch as when I examine the wire I never find air-holes except in the outer covering. I think we have got pretty nearly to perfection in our present method.

1105. You have not yet succeeded in preventing any air passing in with the gutta-percha?—We have not; but as in rolling a very thin sheet you cannot find any symptoms of air, so in passing the gutta-percha through a small orifice you are less likely to have it.

1106. Have you considered the question of vulcanizing gutta-percha?—I have seen some specimens exhibited by Mr. Macintosh.

1107. Do you think favourably of them?—I have no reason to speak either one way or the other. If the vulcanizing have no decaying action upon the gutta-percha, it is possible it may be advantageous.

1108. It is a matter of experiment which you have not had an opportunity of trying?—Just so.

1109. Is there any great difficulty in applying Mr. Macintosh's plan to the outer covering of gutta-percha?—In Mr. Macintosh's cold process of vulcanization, one material that he uses is a solvent of gutta-percha.

1110. What is that material?—Bi-sulphate of carbon. Bi-sulphate of carbon being a solvent, the wire will have to pass quickly through it, and it will require a certain length of time for the evaporation to take place, to harden it before it can be coiled up again.

1111. (*Mr. Saward.*) From your evidence I gather that you do not altogether coincide with the idea that gutta-percha is permeable by water under pressure?—I do not; that is to say to an extent that would be detrimental to the efficient working of the cable otherwise perfect. I have heard that experiments were tried upon the Atlantic core, I think Mr. Whitehouse said something like three tons to an inch, without the slightest effect. That was only for a short time.

1112. (*Chairman.*) The Gutta-percha Company as a rule do not take part of the risk of laying the cables, do they?—They do not.

1113. And as a rule they deal for ready money?—They deal for ready money.

1114. Therefore there is no advantage in the Government or a private company dealing with the Gutta-percha Company through a contractor?—There is not.

1115. Are not the Gutta-percha Company ready to manufacture core for a company or a government, and give them the testing of that core, and deliver it to them in perfect order?—Yes.

1116. (*Mr. Saward.*) I believe that was the way in which the Atlantic core was purchased?—The Atlantic core was purchased at a price on a special contract.

1117. By the company, not through a contractor?—Yes.

1118. (*Chairman.*) It was handed by the Gutta-percha Company to the contractor for the Atlantic Company?—Exactly, after Mr. Whitehouse had tested the wire he gave us a certificate for payment, and our instructions were to send it away from our own works, partly to Messrs. Newall and Company at Birkenhead, and partly to Messrs. Glass, Elliot, and Company, at Greenwich.

1119. (*Mr. Saward.*) The company's risk, with regard to it, commenced, I believe, immediately after the date of Mr. Whitehouse's signature to the test?—It was considered the property of the Atlantic Telegraph Company from the date of Mr. Whitehouse's test.

1120. (*Chairman.*) What are the tests to which gutta-percha covered wire is subjected?—It is put into water in a finished state from the works, and allowed to remain a certain time before the electrical test is applied to it. A very delicate instrument is now used, and a very strong battery.

1121. Do these instruments test eccentricities?—Merely the loss of insulation.

1122. Do you consider the tests at present applied perfect, or might they be improved?—From what I know of submarine cables I do not know of any testing better than we adopt.

1123. Has not there been a great improvement in testing within the last few years?—Yes, the detecting instrument is more delicate, and the battery power much greater. If we were now to test by the old instruments our cores would all be precisely alike.

1124. You can now detect defects which were before passed over in consequence of the inefficiency of the instruments?—Yes.

1125. There are certain defects which even now you cannot detect, such as eccentricities or air-holes?—Yes.

1126. Would not these air-holes be injurious if a cable was subjected to great pressure?—I believe not, and from reports that we have received there are no instances of loss of insulation from these air-holes.

1127. You have stated that the pressure which is put on the wire by putting on a third coat, as it were, would detect an air-hole in the second coat, or would burst it?—Yes, and also I should tell you that the wire is carefully examined; we are bestowing more inspection probably upon the wires we are now making than we have been accustomed to do; we have been obliged to do so from the improved methods of testing; we have endeavoured to keep pace with the instruments by improving our manufacture in every possible shape.

1128. The more delicate the tests are which are used in testing the wire, the more careful of course must be your manufacture?—Yes, our wire is now very carefully examined in the first and second coatings.

1129. Therefore it would be very desirable that extra tests for eccentricities or air-holes should be devised if possible, for the security of the person who orders the cable?—Yes.

1130. (*Mr. Saward.*) Have you seen some specimens of the Atlantic cable in which the conductor is nearly outside of the gutta-percha?—I have.

1131. Have you formed any opinion as to the cause?—I do not think it is possible for the defects to exist to the extent you describe, unless the wire

had been subjected to heat sufficiently to render the gutta-percha plastic after it had left our works.

1132. (*Chairman.*) What heat would that be?—About 100° or 110° would render it sufficiently plastic under a strain for the copper wire to become eccentric.

1133. Did you manufacture the Dardanelles cable?—Yes.

1134. Was that as carefully manufactured and as carefully tested as the core that you manufactured for the Red Sea Company, or that you are now manufacturing for the Gibraltar cable?—Yes, I suppose you mean the last one, some 300 miles odd.

1135. I mean the Dardanelles core about which there have been a great many complaints made to your firm by the contractors who laid it; was that core subjected to the same examination and the same care as you now bestow upon the cores you are manufacturing?—I do not think it was subjected to quite so much inspection as now.

1136. (*Mr. Saward.*) That is the wire, is it not? (*handing the same to the witness.*)—Yes. There is not very much the matter with this; that is 14 strand doubly covered.

1137. I understood you to say that it was not as carefully supervised as cables usually are?—It had quite as much supervision as at that time was bestowed upon any wire, but we are trying to get nearer perfection. It was subjected to precisely the same electrical test, 504 pairs of plates, with the same delicate galvanometer.

1138. (*Chairman.*) Do you know of any causes of the deterioration of gutta-percha when it does deteriorate?—I do not, but there are cases of decay which I have seen that have evidently arisen from local influence, that is to say, in underground wires; I have seen wires taken up that have been buried for some years, in which the main portion has been perfectly good, as good as the day it was put in the ground, and there have been perhaps a few yards upon which something has acted, and seven or eight wires, as the case may be, totally decayed.

1139. Wherever it is exposed to air and dried rapidly it deteriorates, does not it?—Yes.

1140. Does it undergo a chemical change or merely a mechanical change, do you think?—It is no mechanical change; it is by some chemical action.

1141. Can it be restored again by being worked up to its original state?—It is not so good; there is a sort of resin or a resinous feel at any rate upon the surface of wire that has been long exposed to the air and that would render it unavailable for insulating wire again.

1142. (*Mr. Saward.*) You have not seen any cases of deterioration in wire that has been submerged?—Not one single case.

1143. (*Professor Wheatstone.*) Have you observed an alteration in gutta-percha which has been in contact with copper wire when all the external parts have been perfectly intact?—An oxide of copper is found upon the copper, and that is very evident in most of the wires that have been covered for any length of time, and whether that has been checked or the action altogether stopped by the introduction of the compound between the copper wire and the gutta-percha, I do not know. Up to the present time I have not seen anything like it.

1144. (*Mr. Saward.*) Might not that arise from the dampness in the copper at the time it was used, and the subsequent use of the battery power exciting oxidation?—A similar oxide is formed, if you have impaired insulation, and keep the current on.

1145. (*Professor Wheatstone.*) To what degree is gutta-percha affected by fungi?—That I do not know; whether the evidence of decay that I have explained to you has been occasioned by them or not, I do not know.

1146. (*Chairman.*) Do you know whether any marine insects eat gutta-percha?—I have heard they do, and I have seen samples of wire that have been marked, but not to any great depth, as if an insect had tasted it, and did not like it. There is a sort of uniform depth in all the specimens I have seen, only skin-deep in the gutta-percha.

1147. As far as you know, gutta-percha is an indestructible material when it is in contact with moisture?—Yes; perfectly protected from the action of the air, I believe it to be indestructible. I have some specimens of wire that have been covered with lead and hermetically sealed, which do not exhibit the slightest symptom of alteration either in the colour or any other respect.

1148. Do you think that the salts in sea water are injurious to gutta-percha?—I think not.

1149. Is tar injurious or perservative to gutta-percha?—I believe that wood tar is a very excellent preservative. So satisfied are we that tar has that property that we are coating our underground wires with it, that is to say, the wires are first insulated perfectly, and then they are laid together in a sort of rope or strand, and put into a tube of gutta-percha surrounded by Stockholm or wood tar.

1150. Are you referring to the wires of the London District Telegraph Company?—Yes. In specimens of the old Dover and Calais cable we find the wires surrounded with the tarred hemp fully as mellow and as good as the day they were put there; and from that circumstance we believe that wood tar is certainly not injurious to gutta-percha.

1151. Creosote is injurious to gutta-percha, is not it?—Creosote I believe to be a solvent.

1152. Is coal tar injurious?—Coal tar, I believe, is injurious unless it be subjected to some purification. What is called gas tar has an injurious effect upon gutta-percha.

1153. Are you not aware that Professor Hughes's compound has some description of tar obtained from lignite or coal or shale?—It is from coal. I have it from Professor Hughes himself that it is from coal.

1154. Do you think that would be injurious to gutta-percha eventually?—We have not had it analyzed, and therefore I am unable to give an opinion.

1155. Does Professor Hughes's compound contain naphtha?—I really do not know. From what I have heard from Professor Hughes it does not.

1156. Is resin injurious to gutta-percha?—Resin is not injurious in the way it is used by us; it is one of the elements of the compound.

1157. Are any of the gums—gum-dammar, or any of them, injurious?—I think not; I do not know that they are.

1158. Is not your compound composed of resin, india-rubber, and gutta-percha?—Gutta-percha is the principal ingredient in the compound, with sufficient wood-tar and resin to render it the proper consistency, to be applied at the same time as the coat of gutta-percha is put on.

1159. How is the compound put on?—By passing the wire through a vessel containing it fitted with proper gauges immediately before the die that puts on the gutta-percha.

1160. Does it dry rapidly?—No, it hardens as it becomes cold.

1161. It is not hard when the gutta-percha is put over it?—No, it is in a semi-fluid state.

1162. It does not get rubbed off in the process?—No, it cannot by the arrangement of the machinery be disturbed.

Adjourned to To-morrow at One o'clock.

Saturday, 10th December 1859.

PRESENT:

Captain DOUGLAS GALTON.	Mr. SAWARD.
Professor WHEATSTONE.	Mr. LATIMER CLARK.
Mr. G. P. BIDDER.	

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

Sir CHARLES TILSTON BRIGHT examined.

Sir C. T. Bright.

10 Dec. 1859.

1162. (*Chairman.*) I believe you are a civil engineer, whose attention has been very largely devoted to telegraphic science?—Yes, since 1847.

1163. Are you engineer to some of the telegraphic companies?—I am engineer of the British and Irish Magnetic Telegraph Company, and have been since 1852. I am consulting engineer to other companies.

1164. You have been also connected for some time with the working of submarine cables?—I have.

1165. You have assisted in laying several submarine cables?—Yes.

1166. What was the first that you laid?—The first cable I was connected with was that of the Magnetic Telegraph Company from Scotland to Ireland, between Portpatrick to Donaghadee; it was laid by contract.

1167. What depth is that laid in?—The greatest depth is about 150 fathoms.

1168. Is it now in working order?—Yes.

1169. In what year was it laid?—It was laid in May 1853. Another cable of similar character was laid in June 1854, between Portpatrick and Whitehead.

1170. Has any difficulty ever been experienced with that cable?—None whatever.

1171. Has it ever cost anything for repairs?—Nothing whatever.

1172. I believe you were one of the early projectors of the Atlantic telegraph undertaking?—I was.

1173. And you were subsequently engineer-in-chief to the company?—I was.

1174. When you first became engineer, had the contracts for the manufacture of the cable been entered into?—Yes; they were entered into by the provisional committee of the company.

1175. Had you any control over the specifications?—Not over the specifications.

1176. (*Mr. Saward.*) Was not that done before the regular board was formed?—Yes.

1177. (*Chairman.*) To what contractors was the manufacture of the Atlantic cable let?—There were three contracts; one with the Gutta Percha Company for the supply of the core; another with Messrs. R. S. Newall and Company, of Birkenhead, for the manufacture of half of the external covering; and the third contract with Messrs. Glass, Elliott, and Company, of Greenwich, for the other half of the external covering.

1178. Was the separation of those contracts your act, or that of the board?—It was done before I had any connexion with them as engineer. I was not a member of the provisional committee myself.

1179. Do you consider that arrangement a good one?—I consider it exceedingly objectionable.

1180. On what grounds?—Because of the difficulty of testing the working of the two cables together. The one cable being made at Birkenhead, and the other at Greenwich, of course it was impossible to test them with any advantage unless there had been a wire of a similar character laid between Birkenhead and Greenwich.

1181. Do you object to the separation of the contract for making the core from the contract for making the outer covering?—No, I do not object to that.

1182. You think that the perfect condition of the core could be ascertained before it is given to the contractor for the outer covering?—Yes, if the company employ proper officers.

1183. Was that core very carefully examined during its manufacture?—I believe it was; the company had an electrician who was specially appointed for that purpose.

1184. Your business lay more with the manufacture of the outer covering?—With the mechanical construction of the cable and its laying; the alterations of the ships for carrying the cable, and the machinery for paying it out.

1185. Had you the superintendence of the manufacture of the outer covering?—Yes.

1186. Had the form of the outer covering been determined upon before you joined the company?—It was determined by a sub-committee formed from the provisional committee of the Atlantic Telegraph Company, consisting of Mr. Brett, Mr. Cyrus Field, and Mr. Tupper.

1187. A good deal has been said of the lay of the separate portions of the cable being in different directions; do you attach any importance to that circumstance?—No; I attach little or no importance to that circumstance, but of course if it had been under my control, I should have made them uniform.

1188. (*Mr. Saward.*) Will you explain how it came to pass that there were different lays?—I believe the cause of that was from the specimens made up for tenders by one contractor being made up by hand in the opposite direction to the run of his machines, and being sent out with the specification to the other contractors, when Messrs. Newall received the specimen which had been made up by hand, they supposed that the lay of the cable was to be in that direction.

1189. (*Chairman.*) By whom were those specimens made?—I believe by Messrs. Glass and Elliott.

1190. (*Mr. Saward.*) At that time were not Messrs. Glass and Elliott acting on the belief that they would have the entire contract?—Yes, it was so understood. Messrs. Glass and Elliott had assisted the promoters of the company very much in making up different specimens for experiments during 1856, and a portion of 1855.

1191. (*Chairman.*) Do you consider that the Atlantic cable was in a perfect state as to the form of manufacture and insulation, in the first expedition which failed in 1857?—As to its mechanical construction, it was in a perfect condition, and I presume, from the reports of the electrician of the company, that it would be considered in an electrical state of perfection.

1192. It has been stated by previous witnesses that the summer of 1857 was one of extraordinary heat, and that the cables suffered from that cause; do you think that that was probable?—Yes. Some portions of the cable, the upper flakes of two coils, were injured from that, but the cable was very carefully tested by the electricians, and the defective portion of the core, namely, the upper portion of the coils, was removed.

1193. Was it tested in water?—It was not tested in water; the size of the outer strands of which the cable was composed prevented that, without not only a probability, but the almost certainty of injury to the cable from rust.

1194. (*Mr. Saward.*) I believe at that time the necessity of testing cables in water had not been recognized so much as it is now?—No.

1195. Is not the belief of the necessity for testing under water the result of a good deal of experience?—Yes.

1196. (*Chairman.*) You sailed, I believe, with the whole of the expeditions which were concerned either in the laying of the cable or in experiments previously to the laying ?—Yes, I had charge of the whole.

1197. Will you give the committee a history of the experimental expeditions ?—The first expedition consisted of the "Agamemnon" and "Niagara" as cable ships. The "Agamemnon" sailed from Sheerness to Queenstown, and tried some few experiments with the running of the machinery on the voyage. The "Niagara" sailed from Birkenhead, and the machinery was also run to some extent on the voyage to Queenstown, and after that the ships went round to Valentia and the laying of the cable commenced.

1198. What class of experiments was performed on the preliminary voyage ?—The experiments were not of a satisfactory character, in consequence of the shallowness of the water, and consisted in laying some portions of defective cable,—cable which had been injured while in the tanks,—and hauling portions of them back again. I should say that it had been the intention of the company, upon my recommendation, to try these experiments in deep water ; but the lateness of the season at which the ships were obtained from the Government made it necessary to try those experiments in shoal water, when the directors determined upon starting from the Irish coast, instead of from mid-ocean.

1199. Was the break machinery in perfect order before the "Agamemnon" left Sheerness?—There were some slight fittings to be put to it, for guards of lamps, and so forth ; but the machinery was finished, as far as all its working parts were concerned.

1200. It was in perfect working order ?—It was in perfect working order, but we had not been able, from the shortness of the time, to get much experience of its working. It had been intended to start, originally, from the centre of the ocean, and it had been intended, also, that the experiments which we tried should have been all made in deep water, instead of shoal water.

1201. What was the occasion of the change of intention from beginning in deep water, to going from Valentia ?—I believe it arose from suggestions made by nautical men in the first place, and by the electricians of the company in the second place, as to there being objections in regard to starting from the middle. I myself was opposed, as well as the engineers who were under me, to the alteration.

1202. Will you give an account of the first voyage? —In the first voyage we started, I think, on the 5th of August, 1857, from Valentia, and we proceeded in paying out the cable, everything working very satisfactorily, till we had paid out about 380 miles, and about 3 o'clock in the morning the machine, which had been running exceeding well up to that time, was badly manipulated by the men in charge, and the cable broke off short.

1203. (*Mr. Clarke.*) It has been stated that the break machinery sometimes actually stopped and started again, and sometimes required assistance to make it turn round during the expedition, is that correct ?—Not on that expedition. I will explain what you are referring to afterwards. The cable got off the sheaves during the first paying-out expedition, and we stopped the ship as quickly as possible, and put the cable again on the machine and commenced paying out again.

1204. (*Mr. Saward.*) I believe that form of machinery is not what you would now recommend, after the experience you have had ?—No ; there is no doubt it can be improved. It was considerably improved.

1205. (*Chairman.*) Did you return immediately after that failure ?—We returned immediately to Plymouth.

1206. Did you make any further experiments ?—We tried a few experiments in respect to making the joint in the middle, with the view of determining, to some extent, whether it was prudent to start from the middle, or otherwise ; and then we returned to Plymouth.

1207. In what depth of water were those experiments made ?—In about 1,800 fathoms.

1208. Did you finally decide upon the class of joint that you would use at that time ?—Yes.

1209. Will you describe that joint ?—It consisted of two wooden boards, covered over with boiler plate, in which the cable was laid, and a weight attached below to keep the joint from turning over, through the difference of the lay of the cable.

1210. Was that done because you anticipated any difficulty from the difference of the lay of the cable ? —No serious difficulty. The objection that I made to starting from the shore instead of starting from the centre, was the possibility of not being able to make the joint, owing to the state of the weather or other circumstances when we got out to the centre. Of course starting from the centre, both the ships could go out and wait there for a few days, and, if necessary, until perfectly calm, or sufficiently calm weather for making a joint; while in the other case, if the weather was bad, or if the ships were separated by rough weather, or by a fog, the whole of the cable then laid might be lost.

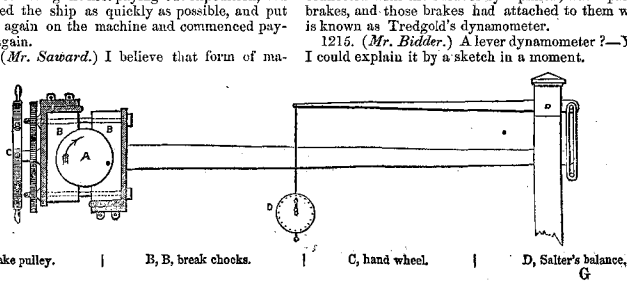
1211. After that expedition you returned to Plymouth. What took place with the cable then ?—The cable was then uncoiled from each ship into tanks prepared for the cable at Keyham.

1212. Were those tanks filled with water ?—They were made so as to be filled with water in case the electricians of the company desired to test the cable under water, but that was not afterwards done.

1213. Will you give an account of the experiments which were made before the next expedition?—On the return of the first expedition we began to consider what improvements could be made in the paying-out machinery ; and I determined upon the alterations which appeared to me to be necessary, consisting of some alterations in the form of the sheaves, of having a travelling pulley to act as a dynamometer, and some other alterations. A committee was then formed by the company, consisting of some practical engineers, experienced in the construction of marine engines ; the question was referred to that committee, and the machinery was altered pretty much in the way that was suggested; and a machine was put up made upon the altered principle, which machine was constructed at the works of Messrs. Easton and Amos.

1214. What were the defects of the first machinery? —I had better explain briefly what was the form of the first machine. The first machine consisted of four single sheaves geared together ; upon a central shaft, connected with the sheaves by a pinion, was a pair of brakes, and those brakes had attached to them what is known as Tredgold's dynamometer.

1215. (*Mr. Bidder.*) A lever dynamometer ?—Yes, I could explain it by a sketch in a moment.



A, brake pulley. | B, B, break chocks. | C, hand wheel. | D, Salter's balance.

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1216. (*Chairman.*) Were you satisfied with the action of that dynamometer?—Not altogether.

1217. Was the alteration that you suggested solely for the purpose of obtaining a better dynamometer, or for the purpose of diminishing the inertia?—For both purposes; for diminishing the inertia and obtaining a more correct indication of the strain upon the cable.

1218. (*Mr. Saward.*) In the second machine the wheels were not pinioned together, were they?—No; they were made so that they could be geared together for raising the cable when necessary. The second machinery consisted of a pair of sheaves with four grooves in them, very similar to the arrangement used on inclined planes, and the dynamometer consisted of a vertical travelling pulley weighted so as to indicate the strain upon the cable. The new machine was constructed at the works of Messrs. Easton and Amos, and such experiments as could be tried on land were tried there by running the machine with the cable upon it, driving it by another machine.

1219. (*Chairman.*) So as to get a tension upon the cable?—So as to get tension upon the cable, and to test the breaks with the dynamometer.

1220. Do you consider that machinery as perfect as any that could be devised?—I think that machine if made lighter would be a very excellent machine for paying out submarine cables.

1221. (*Mr. Saward.*) You would probably adapt the machine and its weight to the kind of cable you were paying out?—Yes.

1222. (*Mr. Bidder.*) What was the weight of the running parts of that machine?—I cannot give you the exact weights, the strain required to set it in motion was not more than 2½ cwt.

1223. (*Chairman.*) Will you describe the experiments that were made before the laying of the cable in 1858?—We started in May 1858, from Plymouth, and proceeded to about 350 miles to the west of Ushant, and there soundings were taken by Captain Dayman in the "Gorgon," and a bottom of 2,400 fathoms was got; we then commenced making a joint between the two ships with some of the old cable that had been injured, and we paid out a few miles of it from each ship.

1224. Do you remember how many miles?—Altogether we paid out about 12 miles from one ship and 15 miles from the other, in several different sections, trying experiments in buoying, in raising, and in paying out as far as we could upon such a very small scale.

1225. The cable was not in a sound condition when it was first immersed, therefore I presume you could not make any electrical tests?—We could not make any electrical tests, but we did work through the whole of the cable in both ships for some time, when the cable was lying at the bottom.

1226. Did you raise much of that cable from the bottom?—Not very much; we raised upwards of three miles in one case for experiments.

1227. Did you observe any peculiarity about the cable which had been at the bottom?—It was a good deal twisted.

1228. Was the lay of the cable injured at all?—The lay of the cable was injured in some places.

1229. Was it so much injured as to increase, very materially, the extension of the cable?—Not very materially.

1230. Was it sufficient, do you think, to have caused an injury to the core?—No; we examined the core, and the core was not injured. We had no means of testing electrically, because, as I said before, this cable had been rejected on account of its imperfect insulation.

1231. Was the core affected in any particular manner?—I cut off a considerable part, and I did not notice that it had been injured in any way mechanically.

1232. There was no evidence of its having been penetrated with water?—No; none whatever; the hemp serving was a good deal squeezed, but not to the extent of injuring the gutta-percha below it.

1233. Was the tar squeezed out of the hemp serving at all?—A good deal would come out under any circumstances in the strain of paying it out.

1234. Was it so much squeezed out as materially to diminish the diameter of the core?—Not materially.

1235. And not sufficiently to interfere with the extension of the cable?—No.

1236. What was the final result of those experiments?—We returned to Plymouth, and made some slight alterations in the machinery.

1237. To get rid of the inertia still left by the previous alterations?—No; not with that view. We found no difficulty with respect to the inertia of the machinery then.

1238. Was the weather rough?—The weather was favourable; the alterations principally consisted in changing a small feeding wheel which we had on board one ship of cast iron, for a lighter wheel of wrought iron, and putting scrapers to remove the tar which hardened in the sheaves to a greater extent than had been expected, and putting some additional pulleys for guiding the cable from the hold to the machine.

1239. When you say that the weather was favourable, do you mean that it was perfectly calm?—There was a swell on, but it was such weather that you could work in boats.

1240. At what angle was the cable paid out on that occasion?—From about 11° to 15°.

1241. At what speed were the ships proceeding?—We went up to six knots an hour in one experiment.

1242. In paying out the cable did you perceive any twisting of the cable?—Yes.

1243. To what do you attribute that?—Partly to the turn coming out from the hold from the coil, and partly, to a certain extent, from the strain.

1244. (*Mr. Clark.*) In what direction? Was it twisting or untwisting?—Untwisting.

1245. (*Chairman.*) Do you attribute any of that untwisting to the friction, upon the strands of the cable, of the water as it passed through the water?—Slightly, perhaps, but very little, on account of the great tension of the cable during the process of paying out. I do not attribute the effect of untwisting to the friction of the water.

1246. (*Mr. Clark.*) Are you certain that it was untwisting that took place?—Yes.

1247. (*Mr. Saward.*) When the cable is coiled on board the ships with the intention of subsequently uncoiling it again in paying out, is not a turn put into the cable?—Yes.

1248. When the cable makes its egress into the water, is not that turn taken out?—Yes.

1249. Would that, in your judgment, have a tendancy to decrease or do away with any injury that might be supposed to arise from the apparent untwisting motion that you have observed?—I do not think it would act to obviate it, but I think that the evil of which you speak is very much exaggerated.

1250. You do not think it would ever untwist so as to lay the core bare?—Certainly not.

1251. (*Mr. Bidder.*) Was the cable that you laid covered with steel?—With iron.

1252. Then had it hemp outside?—No, the hemp was inside; it was a piece of the ordinary Atlantic cable, of which you have, no doubt, seen a good many specimens.

1253. Do you think that the alteration was from the weight of the water, or from the pressure of going out?—I think the very slight alteration in the form of the core, which was so slight as to be scarcely gauged, would be the effect of pressure from the strain of paying out, and not from the pressure of the water. I think it would have recovered its form again by the elasticity of the gutta-percha.

1254. (*Chairman.*) You think it might have taken a permanent compression from strain in paying-out, but not a permanent compression from the pressure of the ocean?—No.

1255. Was the outer covering quite tight round the

core when it was brought up, or loose ?—Quite tight; in some cases it was twisted, but we were lifting up the loose end in that case.

1256. What caused the fracture of the cable in picking it up ?—In picking it up an experiment was tried from both ships at once, with the object of each ship lifting up a certain portion to see if we could get up the joint again. The cable parted from the "Niagara" after raising about three-quarters of a mile, probably from some defect in the cable.

1257. The cable would be vertical when it was picked up, I presume ?—Almost vertical; and we recovered from the "Agamemnon" the joint and the splice, and a portion of the "Ningara's" cable with it.

1258. Was the joint perfectly good ?—The weight was broken off the joint.

1259. The joint itself was not therefore perfect when you brought it up ?—The joint, as far as regards the cable, was perfect, but the splice had the weight broken off from it.

1260. Was there any tendency to kinks either in paying out or afterwards ?—Not in paying out at all; the portion of the cable at the end nearest to the joint was a good deal kinked; no doubt it had been lying slack upon the bottom, and successive coils had been put upon it. We could not, of course, pay out exactly the amount to reach the bottom, nor could we tell with so great a length of cable exactly when we reached the bottom.

1261. (*Mr. Saward.*) Did not you make an allowance for that in making the joint, by paying out ten miles when you were permanently laying the cable ?—Yes, by paying out an additional amount of slack.

1262. So that it would lie in coils ?—I do not think it would lie in coils at that spot in the expedition.

1263. (*Mr. Clark.*) Does the act of paying out as a general rule give the cable a tendency to kink ?—I should say not necessarily.

1264. You have not picked up enough to tell ?—If paid out very slack it might lay in coils, which would kink in hauling in.

1265. (*Mr. Saward.*) Were not 53 miles picked up of the first cable ?—Yes.

1266. And some of it in water of 200 fathoms, was not it ?—150 fathoms was the deepest.

1267. Was that kinked when it came up ?—I believe not; the whole that was recovered had only a very few kinks in it.

1268. I believe the cable that was brought home was electrically perfect, and was subsequently sold as such, was it not ?—It was tested and found good. I know nothing about the sale of it.

1269. (*Chairman.*) Will you describe the various events which took place between the last start in 1858 with the Atlantic cable till the final submersion of the cable ?—On the last voyage we had very bad weather indeed, and a great deal of the cable was very much disturbed, owing to the excessive motion of the ship. We started paying out, and after two starts returned to Queenstown, and recoiled a portion of the cable there.

1270. Then you made a fresh start ?—Yes.

1271. Before making the fresh start, did you make any more experiments ?—No.

1272. Were you satisfied with the experiments which you had previously made ?—We were satisfied with the experiments; we lost some cable upon that voyage. I have no doubt it was occasioned by the cable being injured; it was the last turn in a coil, and the flooring had been a good deal unsettled.

1273. At what date did you proceed to the successful laying of the cable ?—At the end of July in last year; we started from the centre on the 29th of July.

1274. Had you to wait for calm weather?—No, we had very calm weather directly we got there, and we proceeded with paying out the cable until we landed the end of it on the 5th of August at Valentia, and the end of the "Niagara's" portion of the cable was landed in Trinity Bay on the same morning. The weather became very bad the night after starting, and continued so until near the Irish coast.

1275. Were any defects of insulation observed during the paying out ?—We had several cases of entire cessation of the signals, but I attribute that to the electrical apparatus being out of order, or not being properly manipulated. When the cable was laid the electrician reported that the working of the cable was as satisfactory as at Keyham, and therefore I conclude that the reports of cessation during the passage on several occasions were from some defects of the instruments either on board the "Agamemnon," or on board the "Niagara."

1276. Did you observe any mechanical defects in the paying out?—I think the machine should be made a little lighter another time; in ordinary weather the machine would answer the purpose as perfectly as any machine could possibly be made to do. On board the "Niagara" they had no difficulty whatever with the machine; on board the "Agamemnon" we had exceedingly bad weather, the ship rolled and pitched very heavily during the paying out, and we had on some occasions, rather as a matter of precaution than necessity, to keep the wheels moving, to keep men by the wheels in order to move them quickly round whenever they had a tendency to go slower.

1277. (*Mr. Clark.*) Did the paying-out machine ever stop entirely ?—It stopped for a moment twice.

1278. (*Chairman.*) What was the depth of water then ?—In deep water on one occasion, for a moment, when the ship was pitching excessively, and once in shoal water when changing from one coil to another, when the ship was nearly stopped, and a good deal of slack out.

1279. When the vessel pitched, were the men obliged to watch the motion, and to cause the machine to pay out more rapidly as the vessel rose?—The men watched the machine, and an engineer was stationed also at the dynamometer, attached to which was a hand-wheel by which the whole of the weights could be released, if necessary, from the brake; the two things were done simultaneously; the ship pitched so much that the cable varied in its angle from about 11° at sometimes to so nearly a vertical position at others, as to appear to be in danger from the screw.

1280. When the vessel pitched was a very heavy strain brought upon the cable ?—The difference of strain was about 7 or 8 cwt.

1281. Was that effect shown by the dynamometer ?—Yes.

1282. (*Mr. Bidder.*) What was the speed ?—Nearly five knots regularly. On one occasion the wind blew hard aft, and we were running at more than six knots.

1283. (*Chairman.*) Do you think the dynamometer was affected by inertia, so as to prevent your ascertaining the full amount of the strain ?—I think the dynamometer showed very nearly the true amount of strain that we had upon the cable.

1284. (*Mr. Saward.*) You do not think that the pitching and subsequent upheaving of the stern would affect to any appreciable extent the records of the dynamometer ?—To a very small extent no doubt, but not to any appreciable extent. The pitching of a large ship of the length of 250 feet like the "Agamemnon" in the long waves of the Atlantic is slow and gradual.

1285. It would be by a gradual motion, and on that account would be shown upon the dynamometer ?—Yes.

1286. If it were a short ship, with a quick pitching motion, the inertia of the dynamometer would prevent a true record, would it not ?—It might do so to some extent; to a great extent no doubt; but still the motion of the vessel is not so quick, except in the Channel, as to bring any very sudden strain upon the cable.

1287. (*Mr. Bidder.*) Had you the wind generally in your favour in coming back, or was it adverse ?—We had the wind partially with us, and for some portion of the time dead against us, so strong that we feared we should not have coals enough to get home.

Sir C. T. Bright.

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1288. Had you much head sea?—Until the last two days of the laying, the weather was exceedingly bad, so much so, that it was difficult to stand upon the deck.

1289. Was that from a head sea, or an after sea?—Partly from a head sea and partly from an after sea, at different times.

1290. Did you find any difference in the dynamometer when you were going head to sea, to when the sea was following you?—None that we observed.

1291. The pitch would be more rapid when you were meeting the sea than when you were running off it, would it not?—There are other things, such as the different weight of the ship at different times, which might affect those results.

1292. Do you mean by the discharge of the cable?—From the consumption of coals and unloading the cable.

1293. Do you think if cables are laid to any great extent in deep water it is desirable that a vessel should be specially constructed?—I think it most important that a vessel should be specially constructed.

1294. In that case would you recommend the cable being paid out over the stern or through the bottom?—I should recommend its being paid out over the stern even then, from the many inconveniences connected with paying out from the bottom.

1295. You mean practical inconveniences in the operation?—Yes.

1296. Otherwise, of course the pitching, and all that sort of thing, would be very much moderated if the cable were paid out through the bottom?—No doubt of it; but the difficulty of attaching buoys and many other things occur to a man who has been engaged in such work. I think it is most important for any large undertaking of the sort that a ship should be specially constructed, and I should prefer a paddle-wheel steamer to a screw.

1297. In your expedition in 1857 had you tested the cable shortly before the fracture took place?—Yes.

1298. Was the test quite satisfactory?—I believe so; but the electrical operations of the company were altogether under separate charge. The cable was working exceedingly well before the fracture.

1299. Who was at the wheel at the time of the fracture?—A mechanic from Manchester. I had been there myself before, watching its operations, and causing it to be eased when the strain was excessive from the pitching of the ship. I left the machine myself for a moment, and heard it slow, and called to the man to ease the wheel; it immediately stopped, and the cable broke; the break wheel was found set tight upon examination.

1300. (*Chairman.*) You said that the cable worked successfully after it was laid; for how long did it work?—Until the early part of September.

1301. Is it your opinion that the cause which led to the final silence of the cable arose from anything which happened during or after the paying out?—Certainly not from anything that happened during the paying out, for the cable was paid out with very little strain or pressure whatever. My own opinion of the cause of the silence of the cable is, that it was not properly tested, electrically, before we started, and that currents of too powerful a character were used after the cable was laid. I should state that my connexion with the cable ceased altogether upon the ends being brought on shore, so that I cannot give any very reliable information as to the nature of the currents employed. I was asked by the directors to go to Valencia after the stoppage in September, to advise them as to the position of the fault, and when there, was much surprised to see the force of the currents said to have been used when the cable was working.

1302. Can you give any opinion as to whether the pressure of the deep water would have affected the cable if it was otherwise perfect?—I think not; I think that a properly constructed cable would not be liable to much injury from pressure.

1303. What grounds have you for that opinion?—We have a good deal of evidence on the subject; we might take the working of the Cagliari and Algiers cable. It is true that there were three wires defective when the cable was laid, but one wire was in working order when the cable was laid, and has continued to be so ever since. Then again we might take the cable from Spezzia to Corsica; although the depth there is not so great, still there has been a pressure of about one hundred atmospheres since 1854, and all the wires have always worked well.

1304. What is the depth?—About 550 fathoms.

1305. (*Mr. Bidder.*) What is the depth in the other instance you mentioned?—Upwards of 1,600 fathoms.

1306. (*Mr. Saward.*) You are familiar as a matter of history with what has been done in the laying of the Mediterranean cables?—Not of my own knowledge, from what I have been informed and have read.

1307. Do you consider that a great many of the failures that have recently taken place in submarine cables have arisen from the mode in which the contracts have been let?—I think so in the case of the Algerian cable, but I could not give an opinion upon the other cables.

1308. Should you think it a vicious principle to make a bargain with a contractor to lay a cable that shall work for a week or a fortnight for so much money and at the same time allow the contractor to be the sole, or almost the sole judge as to the description of cable to be used?—I should most certainly think it very objectionable.

1309. You are familiar with the plan proposed by Mr. Allen for submarine cables, are you not?—I am.

1310. The principle of that is to place the strength in some central portion of the cable, is it not?—Yes.

1311. A modification of that principle also has been suggested, which consists of laying steel or iron wires round the core in a longitudinal direction, and passing the cable so constructed through plastic material, so as to present a smooth surface on the outside to avoid any spiral lay in any portion of the cable; should you approve of that principle?—No, I should not.

1312. Will you be good enough to state your objections to it?—There would be objections in coiling it, and paying out it would also be so rigid that there would be a liability of the strain coming upon the wires separately unless they were all laid in exactly the same manner.

1313. If a core of that description took a form from pressure or otherwise would it not be difficult to recover it?—Yes, that is one of the objections which I speak of in coiling.

1314. That would be a great objection in its paying out, would it not?—Yes.

1315. Would there be any corresponding increase of strength or capability of bearing strain in such a cable that would compensate for that defect?—No.

1316. With regard to the Atlantic cable and its failures; would you not attribute them in some considerable degree to the hurry with which everything was done consequent upon the determination to lay the cable in 1857?—Yes; no doubt there should have been further experiments tried before adopting the form of cable, and more experiments also should have been tried before determining upon the form of the paying out machine.

1317. I believe you are familiar with electrical questions as well as with those of telegraphic engineering?—Yes.

1318. I believe you made some experiments upon the question of induction some years ago, did not you?—Yes, in 1854, and since.

1319. And the effect of it in retarding communication by electric telegraph in circuits?—Yes, my

experiments were principally upon subterranean wires.*

1320. Will you state what your opinion is as to the cause of retardation, and the best means of overcoming it?—The cause of retardation no doubt is, that you charge the outside of your insulator at the same time that you are charging the conductor, and the effect of that is to lessen the rate at which you can dispatch separate signals one after the other through the conductor. The simplest mode of obviating it no doubt is by increasing the thickness of the insulating material.

1321. Concurrently with an increase in the conductor?—Concurrently with an increase, to some extent, of the conductor.

1322. At all events, you think that that is a difficulty that can be very much overcome in a new cable?—Yes.

1323. Have you any doubt as to the permanence of the deep sea cables, if they are perfectly constructed, and perfectly laid?—Upon a soft bottom I have no doubt of them, provided they are worked with proper currents.

1324. Do you agree with what a former witness has suggested, that it would be very desirable to have one or more further sets of soundings across the Atlantic, before laying another cable?—I think it is very important that closer soundings should be taken, especially at some points at certain distances from the preceding soundings.

1325. In between the soundings already known?—In the neighbourhood of and between the soundings already known.

1326. (*Mr. Bidder.*) Do you hold that opinion from a feeling that there are great changes in the configuration of the depths of the ocean?—I think in the depth of the soundings made by the "Cyclops," where there is very considerable change between two soundings 8 miles apart, that portion of the bottom for one should be sounded very carefully again.

1327. (*Mr. Saward.*) Independently of that, it would be very desirable, and a great advantage, would it not, to have as correct a contour of the bottom as possible?—No doubt.

1328. (*Chairman.*) With reference to the fracture of the cable, which took place at different times, did the cable break on any occasion, except when it was nearly in a *vertical* position from the stern of the vessel, either on an experimental trip, or on any other occasion?—Yes; it broke when it was at an angle of about 12 to 15 degrees, on the first expedition in 1857.

1329. Without any defect in the machinery?—Through defect in the machinery.

1330. Through its becoming jammed?—Through its becoming jammed by inefficient manipulation.

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* Extract from E. B. & C. T. Bright's Patent of 1852 for Improvements in Telegraphic Communications, &c.:—

"With reference to our seventh improvement, we claim the means of ascertaining the point of any fault in the conducting wire or wires from a distant station without the necessity of proceeding to or near to the faulty spot.

"The manner in which our seventh improvement may best be carried into effect is as follows:—It is well known that in the ordinary mode of testing for a fault, those testing have gradually to examine down to the very spot before ascertaining the point of defect, which is found very tedious and slow, and in underground wires very expensive and at the same time dangerous to the wires; this we propose to obviate by the use of apparatus, so contrived as to indicate, when applied at a distant station (even if a hundred miles or more removed), the position of such fault, showing the number of miles it is distant from the station at which examination is made. Figure 1 is a plan of such a detector. A is a detector, with degrees and pointer for indicating the same. B, a series of coils for resistance, with terminals for connection, *b*, *b*, *b*, *b*, so that the coils may be brought successively into action when required. As it is well known that electricity selects the shortest route, should there be an earth or other contact on the line, by connecting one pole of a galvanic battery to A, and B to earth, and also placing an ordinary galvanometer in connexion with the line wire and to the same pole of the battery, the other pole of the battery being placed to earth, then, by interposing such a number of the resistance coils B as will make the two indicating needles or pointers coincide in their variation from zero, the distance of the fault can be determined from the amount of wire in coil for resistance thus interposed, the amounts being marked at the terminals. Figure 2 shows diagrams of the connections for testing. C, ordinary galvanometer; A, B, detector with resistance coils; D, battery; E, line wire in faulty state. Also Figure 3, A, B, detector, as before; D, battery; E, line wire. In this latter instance the ordinary galvanometer is not used."

Fig. 1.

Fig. 1.

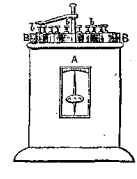


Fig. 2.

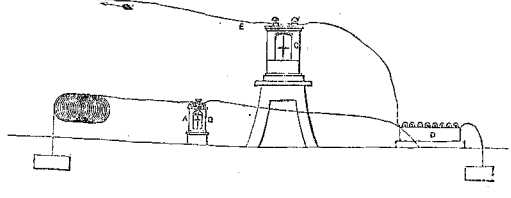
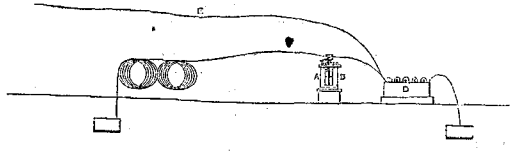


Fig. 3.



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1331. Upon the occasion when the cable broke, without any defect in the machinery, was it at an angle, or nearly vertical?—It broke upon one occasion in the experimental trip, when we were paying out for experiment, at an angle of about 15 degrees; but that was through the cable being defective. It was a piece of cable that was rejected on account of rust, and we took it out for experimental purposes only.

1332. You consider that it was in consequence of a flaw in the outer covering?—Yes, in consequence of a flaw in the cable, which had been rejected.

1333. (*Mr. Bidder.*) Have you had any instances of the cable breaking when perfect, or when the machinery was operating properly, except when it was nearly vertical?—None, under such circumstances, either when vertical or paying out at various angles.

1334. (*Mr. Saward.*) Do you consider the Atlantic cable too heavy for its work?—I consider that it would be desirable to get greater strength; but I do not think that you would gain by making the cable very much lighter.

1335. Do you approve of that form of construction (*No. 2, deep sea Gibraltar cable*)?—This is very much the same form of cable that I recommended to Her Majesty's Government for this work in May and June last. I should have the outer wires a little larger gauge; but with that exception I should approve entirely of it. I have made three reports upon the subject to the Treasury, by request from Sir Stafford Northcote. (See Appendix No. 3.)

1336. It would be a stronger cable than the Atlantic cable, and yet lighter in weight, would it not?—Yes, stronger and specifically lighter.

J. W. Brett, Esq.

JOHN W. BRETT, Esq., examined.

1337. (*Chairman.*) You have been long connected with the subject and science of telegraphy?—I have been a great deal connected with the establishment of submarine telegraphs, certainly from the first.

1338. You were the original projector, I think, of the submarine telegraph between Dover and Calais?—I was in conjunction with a younger brother. The first project, so far as I am aware, was brought forward by us for the submarine telegraph.

1339. You have also been connected with several telegraphs in the Mediterranean?—With some; with the line between Spezzia or Piedmont, and Corsica, and Sardinia, and the coast of Algiers, and also with certain lines which are not yet carried out, but which I suppose will not enter into the present discussion, from Ragusa to Alexandria.

1340. All the submarine cables with which you have been connected have been heavy cables, have they not?—Yes.

1341. Containing more than one wire?—In some instances, six; I have brought sections of the cables laid down in the Mediterranean. This is the form of the two cables that I laid down in 1854 (*producing the same, of six wires*).

1342. What weight would those be?—About nine tons to the nautical mile, or about eight to the statute mile.

1343. In what depth was that laid?—I can only give you the depths of the first section, inasmuch as of the first line laid between Spezzia and Genoa, no soundings have ever been taken across the route, therefore we ventured on it at unknown depths.

1344. Do you know now what the depths are?—The vessel that preceded us made some casual soundings. I have not my memoranda with me, but I think from 500 to 700 fathoms was about the deepest that we had upon that occasion.

1345. The next cable was between Corsica and Sardinia for a short distance of 12 miles?—Yes; I really did not think it necessary to sound on that occasion.

1346. I believe there was no very great depth?—I imagine not, I do not recollect that we did sound.

1347. Do you recollect what quantity of slack you paid out in each of the two cases you have mentioned?—I have not the figures with me, it was comparatively very small, certainly. In that short distance very little slack, we ran out in a strong tide; I think 12 miles were laid in something under three hours. Mr. Canning happened to be a volunteer at the time of the laying, and he can give you valuable evidence upon the subject. We laid at the rate of from five to six miles an hour nearly.

1348. The next cable was from Cagliari to Galita towards Bona?—Yes.

1349. It was in greater depth, I think, than the other?—Yes; in that we passed over 1,600 fathoms.

1350. Was not the cable laid in that case a heavy cable?—We first essayed it with a heavy cable, and gave it up. We found we should not be able to reach land.

1351. The cable broke, did it not, on that occasion?—It did not break from an accident, but it broke in this way: we hung on for the night, we passed the cable through the hawsehole, where it came to a positively up and down strain, and it broke from being subjected through the night to tension over this perpendicular strain from the hawsehole in that depth.

1352. What depth were you then in?—That I cannot give from memory; but not in the greatest depth, it was a large depth.

1353. Was it after you had passed the greatest depth?—Not in that case; but it broke, I must tell you, while we were lying by to determine whether we should cut it during the night or not, or what plan we should adopt in the morning; and with the idea that we should positively be obliged to cut it to save the remnant of cable on board, about 84 miles, which was of considerable value, knowing that it was impossible to reach land with it from the great slack that had been paid out in consequence of the inefficiency of the vessels, and the want of power to conduct it. The French Government had only supplied us with a small passage boat.

1354. Was that cable laid from a steamer?—It was laid from a sailing vessel towed by a steamer that could not make 5 knots an hour. It was a little coasting steamer called the "Tartar" that had been in the habit of coasting from one part of Algeria to another.

1355. Then you do not attribute the fracture on that occasion to the depth of water?—Not at all; entirely to the fact of our laying through the night with it at the stern; not caring, I might say, to protect it, knowing that we should have to cut it in the morning, there being in fact no other alternative.

1356. (*Mr. Bidder.*) Was it cut through or was it fractured?—It broke short off, almost just at the point where it had this very great strain.

1357. (*Chairman.*) What was the class of cable that you used in your next attempt?—A very small one, a very poor cable; this is a section of it (*producing the same*). It was a starved cable (because the funds were running very short), both with regard to gutta-percha and iron. It was anything but what it ought to have been, but as we were obliged to cut the coat according to the cloth this is a specimen of it.

1358. How many wires has that cable?—Three.

1359. What is the weight per mile?—I think it would be somewhere approaching 3 to 4 tons. I cannot give the exact weight, but the result of that cable is very satisfactory.

1360. In what condition was it when it was first laid, electrically speaking?—Perfect.

1361. What condition is it in now?—It has been gathered up; it failed for want of length.

1362. Will you give an account of the failure of the second cable?—This cable was the cable that we were told it would be impossible to lay on account of the great depths; it was the first time that such depths as 1,600 fathoms had been attempted.

1363. Had not the other cable been then laid?—We do not believe that we arrived at those depths with the heavier one; I question if we did. With this one we know that we passed them; we passed all the great depths with perfect safety in the night, at a time when we had a very inconvenient vessel, and had to shift from the upper to the lower hold, and remove all the planking in the very midst of the great depths; we passed them with perfect safety, and arrived within 10 miles of the land. (*The witness explained the chart to the Committee.*) From some reason or other we either drifted out of our course or got out of our course to the west, and at daylight in the early break of the morning, I saw the French vessel decorated with flags and masts at the stern. I had been up all night at the machine, guiding the men and encouraging them in paying out, knowing the risk that we were running, for it was to solve the problem of passing great depths, which were thought impracticable. We passed them, as I say, with a perfect state of the cable, knowing that we were then getting into 600 or 700 fathoms, all our difficulties were overcome, and, according to the reckoning of the French captain, we were safe, and should land our cable with some miles to spare. They decorated their vessel as a triumph, and they were drinking champagne. Our captain was a coasting captain who never had been out of the British Channel, for we had taken the "Dutchman" steamer, the best we could get at the time. He had given us warning in the night that he thought we were drifting very much out of our course. This I communicated to Monsieur De La Marche, the officer appointed by the French government, and he, replied, "We know what we are "doing." I thought very probably they did. By the next morning our captain said, "Certainly, sir, we "have been out of our course in the night; ask the "French captain what his bearings are, to give us the "latitude and longitude." He did so, and it was found to be wrong, and to agree rather with the opinion of our captain that we were ten degrees out of the course. In giving us the figures he gave what he thought the exact position in which we ought to have been. I then begged our captain to give his figures; he did so, and I told him to get me a large board, and I chalked them up, I think about two feet long, so that there might be no mistake about it. I saw, on the part of the French officers, something like consternation; they retired to the cabin, and went through the calculation. When they returned they said, "We find that we are wrong and you are right." That morning Monsieur De La Marche offered to guarantee that we should arrive with ten miles to spare at Galita. I now addressed him and said, "I am no seaman. I am only here to lay my cable "and follow the route you have marked out." I told my man Kell, a very valuable man who assisted in the Atlantic cable, and who went out with me as purser at first, to go down directly and take the exact quantity of cable on board; he did so, and it turned out that we had something like eleven statute miles. I asked the captain of the Dutchman, what distance we were from land? he said we were from 11 to 12 nautical miles. I then addressed Monsieur De La Marche, and said that it was impossible to reach land at a distance of 11 nautical miles with 11 statute miles. I would leave the rest of the route to him, as I had not been to rest for 30 or 40 hours, or away from the deck; I would go down to the cabin, and they could do their best to arrive at the land; it was impossible for me to give them a route. I went down to the cabin when we had 11 miles. On coming up at four o'clock I found instead of making for the land we were going direct eastward. I then asked our captain what was the object of going away from the island, and he said there had been a consultation, and they were trying to reach the line of soundings, knowing that if they reached our line of soundings, that we should have shallow water, whereas to the westward they knew there were great depths. He added, "I think it is right; if we get to the line of sound-"ings we can buoy the cable." When our cable was nearly exhausted, and we were within the last mile, I then told Kell to take the cable, pass it round the vessel, and fasten it with ropes, so that there should not be any great strain upon it, and to hold on, and to signal to the French captain to give us the exact depths. He came on board. I said, "What depths "are we in"? "400 fathoms." I said, "Are we in 400 "fathoms, if so, we are safe." He said, "I think so; "but we have not a very good line." I said, "I think "you had better go off to Bona to get some help." He said, "What help will do?" I said, "If you could "get a lightship to buoy the end of the cable, we "might save it till we can send to England for more; "or get some very large buoys that would protect "the end of the cable. But I should like you to sound "round here to see if you can find a lesser depth; we "have still a mile, and if we can get to anything "like 200 or 300 fathoms, I think we are safe for "stopping the cable or buoying it." He said, "No, "I will send our line. I had better be off to Algiers "at once, the only place where what you require can "be procured." I said, "How long shall you be gone?" He said, "Five or six days." The question then, of course, became hopeless; we did the best we could to hold on for five or six days; we took the end of the cable, passed it round the vessel, and stopped it, so that no strain could come upon it. We sent, among many other messages by the cable, one to London to Messrs. Glass and Elliott, to immediately put in hand from 30 to 50 miles of cable, and send it out to us as rapidly as possible. That message was received. We held on for five days and nights in a perfect state of the cable; the two last days there was a very violent strain, and a very heavy sea, the vessel pitching and rolling, but yet not breaking the cable. Most of the young clerks, who were Italians, were sick, and I was alone on the deck watching the instruments, when I saw a message coming. I got up one of the young clerks, as it came in Italian. It was a message from Messrs. Glass and Elliott on the fifth morning, saying that several miles of cable were in progress, and would be rapidly sent out to us. Within a few minutes afterwards the vessel gave one of the sudden plunges which had been repeated through the night, and the cable broke. I immediately said, "The cable is gone; it is broken at the "bottom." The strain of the cable was still so great on the vessel that she lay as if she had been riding at anchor, showing that there was a great length overboard. We put on all the power we could, and we finally got up the end of the cable, proving that it had gone at a depth of 400 fathoms by friction on a rocky bottom.

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1364. (*Mr. Bidder.*) At what speed had you been going through the night when the vessel passed over the deep water?—At different speeds; but I should think not above three to four knots an hour.

1365. At what angle was the cable?—We did not take the accurate angle. It went out sometimes taut; indeed, all the latter part we paid out as taut as we could.

1366. (*Chairman.*) Was the angle about 15°?—Yes; indeed when I found that we were in this state, I said to the men, "Now, there is still hope of lessen-"ing it; it is the last 11 miles. Do not give an "inch to the water, if you go almost to breaking "strain. We may reach so near land as to make a "safe job of it," and the 11 miles was almost all paid out, I may say, at nearly breaking point.

1367. When it broke, it was more nearly at an angle of 45°?—I suppose it would have been. I did not take the exact angles. The satisfactory point which always occurred to me was this, it was a bad cable. I thoroughly admit that it was not such a cable as, with funds sufficient, I should have made, but it was a very strong cable. We hung on by it for five days and five nights, and we passed at the first experiment the greatest depths, almost equal to those of the Atlantic, and we were in a perfect electrical state down to the last hour.

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1368. You then commenced a new cable for that line, I believe?—I entered into a contract with Messrs. Newall upon this basis: that they should make, at their own risk, such a cable as they would undertake to lay at their own risk. I left the form of cable to them. I only stipulated for a three wire cable, but they, wishing to adopt a cable of their own, or one that had been recommended, and that they had faith in, (on the opinion of Mr. Siemens, I believe,) preferred to give a four wire cable to a three wire cable, and it was a four wire cable.

1369. Did the specification upon which that cable was made, provide for a certain amount of business to be done through it?—No; I do not think the question at that time was even mooted with regard to cables.

1370. The specification did not provide that it should be in working order for a certain length of time after it was laid?—That was not the practice of Messrs. Newall; if they delivered up a cable, as in the case of the Ostend cable, they expected me to take it within five minutes after the electrical state had been proved. We were ready also to take this cable from them, and a very able man, who has long served us, and been with us from the first, a gentleman of the name of France, was sent to Bona to receive the cable immediately if he approved of the electrical condition.

1371. What was the weight of that cable?—It would be, I should think, something over three tons per mile.

1372. It contained four wires?—Yes.

1373. What was the condition in which those wires were respectively immediately after being laid?—Messrs. Liddel refused to allow the electrician I had sent to test it, unless he would not adopt anything but a test by a positive current, as he had usually been testing by both currents, he would not test it. He came back, and Mr. Siemens, who was on board, did test it, and Messrs. Newall declared it perfect, which Mr. France dissented from, and refused to receive it. The tests, therefore, were taken by the French government employés, and those were known to my agent there, who, by consent of Mr. Gordon, of Newall and Company, was sent expressly by their request and approbation from England to test it. They refused to allow my agent to test unless he would test it by the plan pointed out by Mr. Siemens, therefore we can only give the test of the French government.

1374. What was the test?—One wire was perfect, the second was imperfect, but would send a current, and the other two were totally imperfect.

1375. Do you know the present condition of the wire that was perfect?—It is very much in the same position.

1376. The one which was then perfect is now perfect?—Yes.

1377. Has it suffered any deterioration since?—None whatever; the second is also workable, but not to any practicable extent, you can send a message through it.

1378. (*Mr. Saward.*) That cable has not been officially accepted by you?—It has never been accepted; Messrs. Newall maintain that it was perfect, we have said that it was not; it has led to a lawsuit, which is lying over more by consent of Messrs. Newall than by our consent, that is not yet decided.

1379. (*Chairman.*) That cable has been used by the French Government, I believe?—They have used the one wire from the beginning, constantly.

1380. (*Mr. Saward.*) Is there any reason that you know, or have you heard any reason for supposing that the injury to the two wires which are bad, has been caused by the pressure of weight of the sea?—I have no doubt, in my own mind, of what it is caused by.

1381. What is it caused by?—It is caused by the want of a sufficient serving and of gutta-percha, and with a strain that came upon the cable in laying it down.

1382. In fact, to imperfection in the design and manufacture of the cable?—Yes; I told Messrs. Gordon and Newall when I went to Birkenhead to see the cable, that there was not enough gutta-percha to protect the cable. Mr. Gordon replied that he had had an opinion which he would not submit to any other opinion in the world, that was the opinion of Mr. Siemens. I said, "Well, I am very anxious that you "should succeed, my opinion is that you have not "enough gutta-percha."

1383. You considered it altogether an imperfectly constructed cable?—It is not a cable that I should ever have manufactured.

1384. Yet the one wire has continued under that pressure as perfect, at least, as when it was laid down?—One wire has been as perfect.

1385. (*Chairman.*) Were you present at the laying of that cable?—No; it was laid by Messrs. Newall entirely on their own account.

1386. The general result of laying the last cable is that it was what is called a heavy cable?—A comparatively heavy cable; I should not call it a heavy cable.

1387. It was laid in deep water, but not very successfully?—Not successfully as to insulation.

1388. Out of four wires one wire alone is perfect?—One wire alone.

1389. You attribute that defect to the injury it received in laying, and not to any subsequent injury from the depth of water in which it is laid?—Decidedly; the testing proved it. The same mistake was made in the length, and there were two voyages to complete it. I cautioned Mr. Newall that he had not length enough. I knew from this first trial of deep water how much more slack there would be required than was ever accounted for; and when I was at Birkenhead I begged him to delay even a fortnight that I might get permission from the French Government for him to take a greater length. I told him "It is a point I am anxious about, not on my "own account, but on yours. Any failure as regards "the French Government and England will be "serious to you." He said, "We know that we "have enough, that is, if the distance is 125 nautical "miles as you state." I said, "I stated 124; "still I tell you that you have not enough." That was my conversation with Mr. Gordon at Birkenhead. In coming back, I met Mr. Newall, and I made an excuse for returning. I said, "Mr. Newall "I discover, much to Mr. Gordon's annoyance, that "you have not enough cable for this deep water." He said, "Who says so?" I said, "I say so, I assure "you that you have not enough." He said, "We "know that we have enough." "Well," I said, "the "risk is yours, I am anxious for your success." On arriving at the place for the shipping of the cable, I again urged it. I said, "I will write to the French "Government and get you a fortnight, but let me beg "of you to put any cable that you have on board, for "you will surely fail for length. He said, "Why do "you think so?" I said, "From my experience "the last time." I then went to Liverpool to my hotel, and thinking it should not rest on conversation, I wrote them a formal letter of caution, begging them to have more cable. I believe it had this good effect—though they would not have more cable, they did put on board the vessel some 12 or 15 miles of gutta-percha wire, and it turned out as I feared it would, they fell 12 miles short of land. This one gutta-percha wire enabled them to join up one wire and complete the communication, and at a late time in the same year they were able to carry out the 12 miles. It is possible the defect may have been in the joining of that 12 miles, that might have been joined at a time when there was a storm, when it would be difficult to join a wire at sea, and the fault of the two wires might be there.

1390. (*Mr. Bidder.*) Did not they naturally try it?—If they took the pains to lift and re-examine it, they would ascertain it.

1391. (*Chairman.*) In what depth would it be?—That is not a great depth. I doubt whether they ever have lifted it.

1392. (*Mr. Bidder.*) You have stated that you gave

them the exact length of 124 nautical miles. You said that it came out that they had not provided enough; how much had they provided?—I cannot state precisely; my impression is that they had only something like seven to ten per cent. surplus.

1393. (*Chairman.*) Was their contract to be paid in a lump sum, or in detail?—In a lump sum, 50,000*l.*

1394. (*Mr. Bidder.*) How much were they short?—12 miles. I do not know whether I made a memorandum at the time; but I felt convinced that it was not enough.

1395. (*Chairman.*) Had you anything to do with the Mediterranean extension line?—No; that has been entirely carried out by Messrs. Newall, except that the cable was shown to me and the same reasons were given by them, that they undertook the risk, and were satisfied with the kind of cable.

1396. (*Mr. Saward.*) They dictated the kind of cable to be used on that occasion?—They presented that, as the one they were willing to take the risk of in preference to any other.

1397. (*Chairman.*) Did you approve of that cable?—I did not, further than as a director accepting it as the one Messrs. Newall preferred to risk.

1398. Has that cable subsequently failed?—Yes.

1399. What was the cause of failure?—The first of my conjectures is, that it might have been on the Adventure bank, where it is supposed to have been interrupted by the disturbing of the bank from volcanic action. I do not know whether it is so with regard to the Corfu line; we are ignorant of the fault, but my conjecture has been, there is no foundation for other than that it is a leakage which has developed itself.

1400. We have been informed that it was supposed to be an accident from lightning?—I do not think there is any evidence of that.

1401. (*Mr. Saward.*) Do you think it was starved as regards the gutta-percha?—I consider that both of those cables were such as nobody should venture to lay, especially in deep water.

1402. (*Chairman.*) Are you aware whether the leakage is in deep water or in shallow water?—The leakage of the Corfu cable is believed to be in deep water.

1403. In what depth?—Somewhere about 400 fathoms, not in very great depth.

1404. If the leakage were due to the pressure of the water would it not be more likely to have taken place in the deep water?—From pressure certainly if there is a fault.

1405. Not from water penetrating into the gutta-percha?—No; I have no fear of that and never had. I do not think there is anything to prove it; but if there is a fault in the gutta-percha, certainly the water would penetrate.

1406. (*Mr. Saward.*) Should you consider it a good system to allow the contractor to dictate the kind of cable that is to be made?—I think it a most injurious system. I do not know why a contractor should assume to dictate the form of cable, which depends a good deal upon experience and scientific principles.

1407. Is it not a very costly practice to allow a contractor to decide the form of cable, who at the same time guarantees the working of it for a fortnight or some small time?—When a contractor guarantees you are inclined to yield very much, because you are naturally disposed to think that he will adopt the best thing for his own safety. I do not doubt that Messrs. Newall felt very great confidence in their own cable.

1408. Would not a contractor be tempted to adopt a cable that was the best to lay?—Yes; and I have no doubt that Messrs. Newall had great confidence in their cable.

1409. Their concern would not be so much as to its permanence?—No; they recommended, at the origin of the Atlantic Telegraph scheme, a similar cable to the Malta and Corfu as a preferable cable to all the others.

1410. (*Chairman.*) Do you recollect the weight of the Malta and Corfu cable?—2¼ cwt. The heavy cables which were laid by me early in 1854 have never required an outlay of a shilling to the present time, nor have they ever failed in any single instance; now they are quite perfect.

1411. That is to say, the one from Spezzia to Corsica, and the second from Corsica to Sardinia?—Yes; they have never required anything for repair. All the alarms that have been given with regard to their failure have always proved to have been failures in the land lines, and never, in any one instance, in the submarine. I am sanguine that they will go on for 20 years in the same way.

1412. (*Mr. Saward.*) You are a director of the Submarine Company?—Yes.

1413. Has not the Dover and Calais cable, which was laid in 1851, been recently taken up to be examined?—Yes.

1414. Did you find the iron corroded to any injurious extent?—In some parts the iron is very much corroded, where it has come in contact with the lime and the chalk rock.

1415. So as to affect its strength very materially?—In time it would.

1416. In what state was the gutta-percha?—It is perfect. Mr. Canning will give the details, which are interesting and very satisfactory as to future submarine cables, showing the importance of what I have ever believed is the great point, namely, perfect manufacture. The Gutta-percha Company, to whom we owe an immense deal, have, with great ability and unlimited means, brought gutta-percha to a perfection which we might still have been without, as far as gutta-percha is concerned. I do not think, if gutta-percha had never been found, that we should have been without good submarine cables, but in the first cable laid down by me from Dover to Calais, we have had faults increasing to the total cessation of the current by one wire, and it has, of course, been totally useless; in this way—because we have had it examined, and we have found that it failed from a fault in the original manufacture; when perhaps the eyes of the engineer might have been turned away, some mischief or some bad management had crept in. In one case, the gutta-percha had been cut through, and a bit of common tape had been wound round it. There was another fault, of almost a similar kind, proving that in time the wire had been absolutely destroyed by corrosion and oxidation, from the water having penetrated at those places. After those have been cut out, the cable is as perfect, I am told, as it ever was.

1417. (*Chairman.*) The water having penetrated to the copper wire, was that wire still capable of transmitting signals?—It had ceased altogether.

1418. After it was entirely corroded?—It was entirely a mass of green oxide; the wire had perished, as you will see from the specimen.

1419. (*Mr. Saward.*) The Dover and Calais cable is now perfectly repaired?—Yes; and the four wires which were laid in 1851 are as perfect, I believe, according to the tests, as they were the very first day.

1420. (*Professor Wheatstone.*) Has the failure of that injured wire been gradual or sudden?—It has been gradual.

1421. (*Chairman.*) Was it perfectly clear that the water had penetrated to the wire, or was the failure from any other internal cause?—You will find it perfectly clear, when you see the specimen, that the water had penetrated, from the state of oxide on the copper. These instances I think afford evidence that after seven or eight years the only fault that we have suffered from has been a fault in the original manufacture, and not in any deterioration of the material; for instance, there is a very curious specimen in the Ostend wire. I found that in the Ostend wire, which I have been examining, when there was a want of continuity, the gutta-percha presented only a deep indentation as if a piece had been chipped out with the nail, almost down to the wire. There was oxide on the wire, although at first I was doubtful about it; but on taking the wire out and holding it to the light, I found that you could see through the gutta-percha; it did not appear to me that the water had much penetrated. Therefore that is another instance, and a very curious

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one, of the continuity ceasing from a hole in the gutta-percha.

1422. You mean that the current must have escaped through the gutta-percha?—Yes.

1423. What thickness of gutta-percha is there between the wire and the water at the point where the piece had been removed?—It is completely through. I took out the wire from the other side and could see through it.

1424. Had the water penetrated to the wire?—I thought it had, but the serving had protected it from the water, not as in the case of the Dover and Calais cable, where you can see that the water has evidently corroded the wire.

1425. If there had been air and not water, air being a non-conductor, would it not have acted as an insulator?—I cannot say that, but it is evident there was formed an earth by the escape of the current at that point, although I question if water had penetrated. I am only giving evidence upon my own conjecture, without questioning Mr. Canning at all upon it, or having any information from him; he took out this very piece, and he will be able to give you some interesting facts about it.

1426. Do you believe gutta-percha to be the best insulator for submarine cables?—I believe it to be one of the best, to a certain extent,—in the way it is at present used; still there are things to be done to render submarine cables perfect.

1427. (*Mr. Saward.*) Has not the Submarine Company several other cables besides the Ostend and Boulogne?—We have now 21 wires across the Channel.

1428. Dover to Calais?—Four wires.

1429. Folkestone to Boulogne?—Six wires.

1430. Dover to Ostend?—Six wires, and Weyborne to Embden two wires.

1431. Weyborne to Heligoland, three wires, and thence on to Denmark?—Three wires; in all 21.

1432. (*Chairman.*) Are the annual expenses of the maintenance of those cables great?—Our annual expenses of course will increase with the number of cables, but for the seven years I think they have not exceeded 5,000*l.* as a sum total.

1433. For the whole time?—For the seven years.

1434. (*Mr. Bidder.*) That applies to the Ostend and Dover line?—Yes.

1435. (*Chairman.*) To what are those expenses attributable?—Principally to damages received from anchorage; we suffer a great deal from anchorage.

1436. If a cable is out of the reach of anchors, do you think that it will suffer?—My impression is that a cable out of anchorage, even where the iron wire has been comparatively destroyed, will be safe. The first impression which I formed upon that point is borne out by experience; I asked Mr. Andrews,—and Mr. Canning will confirm the fact, whether it is correct or not,—whether in taking up a cable he did not find a crust formed upon the oxide of iron by the lime and sand at the bottom; I am told that in most cases he did. Of course naturally the spring of the cable in raising it would break the crust and throw it off. I have specimens where the iron has been allowed to lie for a length of time, it becomes gradually encrusted. I believe that will be the case in still water at great depths.

1437. The Dover and Calais cable has suffered from the nature of the bottom?—Yes, where it crossed a rock and had the sweep of the tide or current, it was very much corroded, but where it was embedded in sand and covered, it was found scarcely affected.

1438. The general conclusion to which you have arrived is that if a cable is made thoroughly well in the first instance it will last for a very long time without any need of repair, provided it is not injured by anchors?—My impression is that a cable can be so thoroughly well made as to last 50 years, if laid down in quiet water, without further expense. I am sanguine now that no expense will be required for those two Mediterranean cables that have been laid down, for 20 years, unless from anchorage in the shallower parts or some disturbance.

1439. Are you satisfied with the form of the Atlantic cable?—No, not so. I believe it too flexible.

1440. You assisted to some extent in determining upon the form?—I was one of three or four who decided upon it, but it was not the cable that I wished to select; it was selected as the second best rather to meet the convenience of the time, because the cable that I selected was from a specimen made at Messrs. Glass and Elliott's, which would bear an extraordinary strain.

1441. What was that cable?—A steel wire.

1442. Similar to the strands of the Atlantic?—No, single wires; if I recollect rightly it bore a strain of something like 20 tons. I am speaking from memory, but I believe that cable would have nearly anchored the "Niagara" in the depths of the Atlantic; that is to say in everything but a storm there would have been no fear of breakage, but fortunately we had not to complain of that in the cable that was laid.

1443. (*Mr. Saward.*) Do you consider that the early proceedings of the Atlantic Company were a great deal too hurried to insure success?—Yes; the question was settled upon the lighter cable between myself, Mr. Cyrus Field, and Mr. Statham, in going down to Liverpool, when we wanted to get the list of subscriptions; the whole question between myself and Mr. Cyrus Field, who had the go-ahead character of an American,—and to some extent I think that was very valuable in the first starting of the thing,—was, whether it should be 1857 or 1858; I wanted 1858, and the other cable to be adopted. It could not be made under two years; Mr. Field said it must be 1857, and as a matter of policy in getting the subscriptions together, he was right. But the question had been if we had had 1858 and could have laid the other cable, whether it would not have been safely at work now and for many years to come.

1444. As it was, you pledged yourselves to the subscribers of the capital that an attempt should be made in 1857?—We did, and with the experience we have had, I think the cable adopted in 1857 was the very best we could have adopted for that year.

1445. From all you have seen from constant application to the business of the Atlantic Telegraph Company, should you consider that the troubles of the company arose not from anything inherent in the nature of deep sea cable, but in all probability from imperceptible injuries, which the cable received during the repeated coilings and uncoilings?—As far as my impression goes, I believe that neglect in the electrical department is the thing to which our great misfortunes in the Atlantic telegraph are due.

1446. Do you mean before the cable was laid or after?—Before the cable was laid; it is a point upon which I think so much depends, namely, an assurance of the perfect manufacture in every inch of the cable from the time of its starting till it is laid down.

1447. You think the tests from time to time were not sufficiently careful or continuous?—I do not think that they were with a work of the length of the Atlantic cable; and experience proves to us now that the neglect of one workman might spoil the labour and science of the most able men in one inch of it being overlooked.

1448. From the information which has been acquired by the Board, should you say that after the cable was laid that information tends to show that it was further injured by the application of very great battery power?—That is the conviction upon my own mind, and I believe it is the general conviction.

1449. Is it not borne out by evidence which has been brought from time to time before the Board?—I think clearly it is.

Mr. SAMUEL CANNING examined.

Mr. S. Canning.

1450. (*Chairman.*) You are a civil engineer?—Yes.

1451. You have been engaged for many years in the construction and laying of telegraphic cables, have you not?—Yes.

1452. Will you state the several cables that you have laid successfully?—The Newfoundland and Cape Breton Island.

1453. In what depth is that cable?—Between 150

and 200 fathoms. The Prince Edward's Island and New Brunswick cable a short length, and the Weyborne and Embden.

1454. In what depths?—In shoal water for 25 to 30 fathoms.

1455. What length is that cable?—Upwards of 200 miles; the Lowestoft and Holland, the Boulogne and Folkestone, the Weyborne and Heligoland and Denmark, the Sweden and Gotland, and the Atlantic.

1456. Have you been connected with all those lines which were made by Messrs. Glass, Elliot, and Company?—Yes.

1457. You are connected with them, I believe?—Yes.

1458. How many of those cables, which you have mentioned, are now in successful working?—All of them, except the Atlantic.

1459. Are you aware what has been the cost of the repairs of those lines since they have been laid down?—Very trifling indeed; only in one or two instances, one at the shore end of the Newfoundland line and also at Embden, crossing the river Ems.

1460. In both cases close to the shore?—Yes. I believe there were some repairs made to the Holland cable, an anchor broke it, but it was repaired very shortly afterwards.

1461. What is the weight of the Newfoundland and Cape Breton cable?—From 38 cwt. to two tons a mile.

1462. How many wires are there in it?—One wire.

1463. Have all those cables single wires?—No, they have various numbers of conductors; in the Boulogne and Folkestone there are six, in the Embden there are two, in the Danish there are three, in the Holland there are four, in the Sweden there is one.

1464. What is the general character of the outer covering of all those cables?—All solid iron wires.

1465. The principles of construction, I believe, adopted by Messrs. Glass and Elliot is that of a heavy cable, or what is usually termed a heavy cable?—Yes, for shoal water.

1466. What is the greatest depth in which any one of those cables has been laid?—The Newfoundland is the greatest depth, between 150 and 200 fathoms.

1467. You have recently, I believe, examined the Dover and Calais cable, with a view to repairing it?—Yes.

1468. Can you give us an account of the condition in which you found it, both as to the outer covering and the insulated wires?—In many places the outside wires appear to be very bad indeed, and eaten away, but to a very small extent in others; it is very much rusted, and in fact so much so that there is a very small portion of wire holding or remaining. Next as regards the serving, where the cable is loose and the iron wires appear to have been untwisted or unlaid, so that the water can wash freely through the iron wires and to the serving, the yarn is rotten.

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1469. The pitch has been washed away?—Yes; you may pull the yarn off very easily; in other instances, where it is surrounded by the wire, it is as good as ever it was, it smells quite fresh of the tar, and the gutta-percha is in a perfect state of preservation.

1470. (*Mr. Saward.*) In all instances?—Yes.

1471. (*Chairman.*) Is the gutta-percha in contact with water in any of the cases that you have seen?—In some instances we have cut out parts which have been caused by kinks or under-running in repairing, where the gutta-percha has been exposed, and it is quite as sound as it was on the day on which it went down. In the Dover and Calais cable there is a peculiarity in the gutta-percha, which, I suppose, is owing to its having been the first one that was made, as they had not then got into such a good way of covering as they have now; we find that the two coverings separate, and do not adhere; but in the Ostend cable, we do not find that the case.

1472. (*Mr. Saward.*) That you discover only when you cut it?—Just so.

1473. (*Chairman.*) From the experience you have gained, would you consider a hemp serving to be a sufficient protection for a cable if you wished to protect it in deep water?—No, I should not.

1474. You would consider that *it* would decay after a short time, however well saturated it might be with tar, or anything else?—I think it would, from what I have seen of the Dover and Calais cable.

1475. I believe you were concerned in 1855 and 1856 in experiments intended to indicate the kind of cable that would be suitable for submersion across the Atlantic?—Yes.

1476. Will you give us an account of those experiments?—I have not the particulars with me; I can furnish you with them.

1477. Where were they made?—All the specimens were made at Messrs. Glass and Elliot's, and the tests at Messrs. Brown and Lenox's.

1478. (*Mr. Saward.*) Is not the table in existence showing the nature of those specimens and the breaking strain?—Yes.

The same was delivered in, and is as follows:

Size of C. P. wire and weight per mile.	Serving with yarn, and weight.	Gauge of wire.	Strands of solid wire.	Number of strands or wires.	Weight per mile of wire.	Total weight of cable per mile.	Breaking strain in tons.	Diameter of cable, in inches.	Reduced diameter.	Length between Clamps, in inches.	Sectional metal area of wires.	Strain per square inch of metal section, in tons.	Rates of breaking strain to weight.
						cwt.							
5-8ths	—	20/ 11	Solid, Horsfall's	18	—	40.2.22	18	½	—	49	·20338	64	6·4 to 1
5-8ths	1 thread yn.	20/ H.H.	Strand, charcoal	18	—	—	—	—	—	—	—	—	—
5-8ths	2 ,,	20/	Strand, Horsfall's	18	—	32.3.3	11	½	—	60½	·121212	90½	6·7 to 1
5-8ths	3 ,,	19½	Solid, Horsfall's	18	—	48.0.7	18	½	—	51½	·253854	71	7·5 to 1
5-8ths	—	20/	Strands, Wright's pat.	12	—	36.1.24	4½	1	—	60	·119946	37·6	3·5 to 1
5-8ths	—	21½	,, ,,	12	—	38.3.25	4½	1	—	73	·067396	66·26	3·1 to 1
5-8ths	—	20½	,, ,,	15	—	24.0.7	2½	½	—	85½	·043290	57·75	2·07 to 1
3-8ths (3.0.14)	5 (1.5.0)	13	Solid, charcoal	18	19.1.20	24.1.6	4½	¼	—	55½	·127584	35½	3·66 to 1
3-8ths	9 thread yn.	21/7 H.H.	Strand, charcoal	18	12.1.20	23.1.3	4½	¼	—	58	·126864	40½	3·86 to 1
3-8ths (3.0.14)	1.3.0	19/9	,, ,,	15	—	23.1.3	4½ but not fair.	¼	—	65½	·124650	—	3·89 to 1
3-8ths (3.0.14)	5 threads 1.3.0	22½/ W.H.	,, ,,	18	—	18.0.17	4	¼	—	53½	·077490	51·62	4·44 to 1
7-16ths	—	17½/	Strand, Wright's	9	—	20.1.25	4	¼	—	65½	·071334	56·00	3·2 to 1
7-16ths	—	18½/	,, ,,	11	—	18.0.25	3 not fair.	¼	—	79½	·062238	—	Not fair break
7-16ths	—	19½/	,, ,,	11	—	13.3.9	1 not fair.	¼	—	63	·043705	—	,,
3-8ths	—	19½/	,, ,,	9	—	13.3.18	1 not fair.	¼	—	67½	·037395	—	,,
3-8ths	—	20½/	,, ,,	9	—	13.2.28	2	¼	—	69½	·025974	77·00	3·17 to 1
3-8ths	—	19½/	,, ,,	9	—	14.1.14	3½	¼	—	50½	·050922	49·10	3·47 to 1
3-8ths	5 thread yn.	19½/ W.H.	Strand, charcoal	14	—	26.2.22	6½	¼	—	67½	·126736	—	4·90 to 1
3-8ths	10 ,, (tarred).	22½/ stout H.H.	,, ,,	18	—	26.0.15	4½	¼ bare	¼	69½	·077616	—	3·69 to 1
3-8ths	2 thread yn.	17½/ stout to No. 5 gauge.	Strand, Wright's	8	—	21.1.8	4	¼	¼	80½	·079632	—	3·76 to 1
3-8ths	—	20 to No. 12 gauge.	Steel, Wright's	13	—	8.0.0	1	⅛ stout	—	66½	—	—	Not fair break
3-8ths	—	18½ to No. 8 gauge.	Strand, Wright's	8	—	14.3.5	2	⅛ stout	—	74½	—	—	,,
3-8ths	5 thread yn. (tarred).	22½/ W.H.	Strand, charcoal	18	—	21.3.19	broke at clamp.	¼	¼	69½	·045264	—	3·72 to 1

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1479. (*Chairman.*) Those experiments were solely directed to discovering the cable of the greatest strength, I suppose?—They wanted to get the greatest strength with the least weight.

1480. Were any experiments made upon hempen cables?—A combination of hemp and wire.

1481. Not hemp alone?—No.

1482. Were they submitted to the action of water?—No, they were not.

1483. Will you state the reason which influenced Messrs. Glass and Elliot, with the concurrence of the projectors, in the adoption of the form of cable subsequently used by the Atlantic Telegraph Company?—It was the cable which bore the greatest strain with the least weight, or the ratio of the breaking strain to the weight was the greater in that cable.

1484. Out of water?—Yes.

1485. (*Mr. Saward.*) Did you try any cables constructed of steel wire?—Yes, but they were thrown on one side on account of our not being able to get a supply of steel sufficient to make the cable in the time specified.

1486. Otherwise you would have preferred a steel cable?—Certainly.

1487. (*Chairman.*) You were engaged by the Atlantic Telegraph Company in 1857 and 1858, were you not?—I was.

1488. Were you occupied in managing the coiling of the cable, and its subsequent carrying out?—Yes.

1489. In what ship were you?—The "Agamemnon."

1490. With Sir Charles Bright?—Yes, I was in the "Niagara" the first year, and commenced with that ship; the second year I was in the "Agamemnon," with Sir Charles Bright.

1491. Was a portion of that cable made by Messrs. Glass and Elliot?—Yes.

1492. (*Mr. Saward.*) You commenced from the Irish shore on the first occasion, and that was the reason of your being present on board the "Niagara?"—Yes.

1493. (*Chairman.*) Will you give us your opinion generally upon the question of the early failure and the subsequent stoppage of the current passing through the Atlantic cable since it was laid?—I think, of course, in the first instance, that it was made too rapidly; there was not sufficient time given to make the cable in a proper and efficient manner. I think that has been proved by the joints in the copper wire and gutta-percha, from the specimens which we have cut out of the cable since.

1494. Do you mean the joints of the gutta-percha?—Yes, the joints of the gutta-percha and the copper wire; in some instances the copper wire is close to the outside, and there is only a thin film of gutta-percha over it. Mr. Saward has seen the specimens; and in other instances the copper wire has been found to be close upon the outside of the joint, to have come out from the centre; whether it was done in the covering I do not know.

1495. In what lengths was it made?—It was sent down to our works on drums; about three miles.

1496. (*Mr. Saward.*) I believe none of those defective joints of which you have spoken were discovered in the cables manufactured by Messrs. Glass, Elliot, and Company?—I have not heard of any.

1497. (*Chairman.*) Were the joints made by the Gutta-percha Company's people or by the contractor's people?—That we do not know, being cut out of the cable afterwards.

1498. I mean generally were the joints made by Messrs. Glass and Elliot's men, or by men from the Gutta-percha works?—By men from the Gutta-percha works. I do not anticipate that there was any failure of the Atlantic cable from the process of laying. I do not think that was a cause, so far as I could judge from what came under my own knowledge, in our own ship.

1499. You think the cause of the failure is due to imperfect manufacture?—I think it was from the imperfect manufacture, and from what I hear—I do not speak positively—from extraordinary battery power being used afterwards in finding out those very small faults.

1500. Have you any observations to offer to the Committee upon the paying out of the cable or the break machinery?—As far as we saw it ourselves on board the "Agamemnon," it acted very well certainly; we should now make some alterations, or we should not use the same apparatus again.

1501. You have had a considerable experience in the paying out of cables?—Yes, I have laid down all that I have enumerated.

1502. (*Mr. Saward.*) I believe all the early cables that were laid were laid down simply with a drum and lever, were they not?—Yes, they were. In the Corsican line, which Mr. Brett has been describing to you, there were two drums and levers. Of course our system now is much better, and our system of coiling is better; we go at greater speed; in the Corsican cable we were five days in going 70 odd miles. We used to anchor at night, and hold on by the cable, and proceed the next day by daylight.

1503. (*Chairman.*) Did you find any inconvenience from kinks in the former mode of laying cables?—Yes, in the earlier cables.

1504. (*Mr. Bidder.*) And defective coiling?—Yes.

1505. (*Chairman.*) In fact, in the earlier cables a stronger class of cable was necessary, in consequence of the mode of laying?—Yes, they were very stiff cables indeed, and of course not so easily handled.

1506. Do you prefer a cable to be heavy for paying out, or light?—I like a heavy cable.

1507. (*Mr. Bidder.*) Not for the mere operation of paying out, but you like it when it is laid, because it is more durable, I suppose?—To some extent I like a stiff cable for paying out; a light cable is likely to overrun itself in paying out, and very liable to fly into kinks; as it uncoils itself on the surface it will form a letter S, and there is a fear of its overrunning, which with a stiffer cable would not occur.

1508. (*Chairman.*) A flexible light cable, you think, would be an objectionable one to pay out?—Yes, I would sooner have a stiffer cable. The Boulogne and Folkestone, which is the heaviest cable that has been laid at all, weighing 10 tons to the mile, we had not the slightest difficulty at all with. I never saw a cable run out better.

1509. (*Mr. Bidder.*) I think in the Holland cable you threw out very little slack?—We did not lose much, and in the Folkestone and Boulogne we lost less.

1510. (*Chairman.*) Whether the bottom is soft or not, do you prefer laying a cable very taut?—I do.

1511. In any case, whatever the nature of the bottom is?—Not at least, if you go amongst rocks.

1512. (*Mr. Bidder.*) You prefer a cable being taut if the water is very deep, because you would not have a heavy strain on the cable where the water is deep?—No, so that it would not overrun.

1513. So far as your experience goes, up to 2,000 fathoms what should you call a fair allowance of slack, so as to prevent your not having a strain of cable?—From 20 to 25 per cent.

1514. (*Chairman.*) For deep sea cables?—Yes.

1515. (*Mr. Bidder.*) In the narrow channels, for example, in the Holland cable you had not five per cent., had you?—No.

1516. The depth was 30 fathoms?—30 to 35.

1517. (*Mr. Saward.*) You know the principle which Mr. Allan recommends for the construction of cables, do not you, with the strength in the centre?—I have seen a specimen.

1518. A modification of that has been suggested, that steel wires should surround the core, laid in the direction of its axis, and that the cable should then be passed through some plastic substance, so as to become smooth on the outside; is that the cable which you would recommend?—No, it is not.

1519. Will you be good enough to state your objections to it?—I do not see how you are to coil that cable well to begin with, and in paying out, I think it would be objectionable; it would not come out of the coil properly.

1520. If it took a bend it would be permanent?—Yes.

1521. If a wire broke in the inner circumference, would it not have a tendency directly to push itself into the core?—It would do so.

1522. (*Chairman.*) In paying out a cable, have you observed a twisting of the cable?—Yes.

1523. In what direction does it twist?—It turned, in the Atlantic, to the right hand.

1524. Each section was laid differently?—Yes.

1525. (*Mr. Bidder.*) Did the cable untwist, or twist up tighter?—It was turning in the direction of the lay.

1526. To what do you attribute the twist?—I do not think there is any twist, except what was given to it in the coiling. I have seen other cables laid, and there is always a certain amount of turn put in the coiling, which goes out in paying out. I have picked up some cables, but I have never seen a cable opened at all from untwisting.

1527. Do you think that the kinks which have been observed, or are supposed to exist in a cable at a great depth, are attributable to twisting or untwisting, or are they attributable to the fact, that the cable was paid out more rapidly than the vessel was going, and therefore was deposited in coils?—That is my opinion.

1528. (*Mr. Saward.*) Do you think the dynamometer used on the last occasion of laying the Atlantic telegraph gave a true indication of the strain upon the cable from time to time?—I do not say that it gave an actually true indication, but it gave it approximately.

1529. In the case of the pitching and upheaving of the vessel at the stern, do you consider that you would have an indication on the dynamometer of a less strain, while the fact was, that it was a greater strain than existed previously to the pitching?—No, it indicated more.

1530. It was approximately correct, even in that case?—Approximately, I believe it was.

1531. (*Mr. Bidder.*) In the Dover and Calais cable is there a large proportion of cable exposed to the water that is not bedded in the sand, and therefore subject to corrosion?—Yes, it comes so up in places; for some distance from the shore, the cable is quite encrusted with the shells of marine insects; in fact, it is more a diameter of two inches than one inch, it is so covered; in those instances the cable is not so rusted as where it is exposed to the action of the water.

1532. (*Chairman.*) Is any part of the bottom rocky over which it passes?—There is chalk rock near the South Foreland.

1533. There it is completely exposed?—Yes.

1534. And there it is that the cable is injured by the action of the water to the greatest degree?—Yes, it is more eaten away by rust; but still where the cable is not on the rock itself, and there is no particular friction to wear it away, it looks very well indeed.

1535. In some parts it is corroded and the wires are partly gone?—Almost eaten through; I have seen them drawn almost to a point.

1536. Is that on the chalk?—I do not know the bottom; we could not get soundings, the bottom varies so much; it comes up for a few fathoms covered with shell and looks very well, and then for a distance we find it eaten away again.

1537. In no place is it so much gone that you cannot pick it up?—No; I have relaid nearly the whole of it again which has been picked up; I have taken it on shore, repaired it, and relaid it, and the cable works very well now.

1538. (*Mr. Saward.*) It has been suggested that after the cable is completely manufactured, as it comes out it would be desirable to apply water pressure to it, and test it under that pressure equal to about four tons to the square inch; do you think that is practicable, and if so, would it be desirable?—I do not think it practicable.

1539. It has been suggested that it might be done in long gas pipes, and in that way pressure might be put upon it?—It may; but it would take a very long time to make a cable of any length in that way.

1540. You do not think that that could be practically carried into effect?—No, I do not think it could.

1541. Do you attach any importance to the supposed pressure of the water in the deep sea upon a properly manufactured cable?—I do not think it would do it any injury.

1542. You do not believe in the permeability of gutta-percha by water?—No, I do not.

1543. (*Chairman.*) Have you any record of the electrical testing of any of those cables which have been laid, during the laying and after the laying?—Nothing, only the ordinary tests.

1544. Showing the condition in which the cables were immediately after they were laid?—Yes, but they are very imperfect records; I can only give you the mere result.

1545. Have you any reports upon the subject?—No; while we are laying cables we do not take so many notes.

1546. In the case of the Red Sea cable there is a very interesting report from Mr. Siemens?—We could only give you the tests every quarter of an hour, or ten minutes.

1547. Did you observe any loss of insulation during the laying of the cable in any of the cases?—In the Holland cable.

1548. Which was subsequently recovered?—Yes.

1549. To what do you attribute that?—It was a nail driven in.

1550. In the cables you have laid, in the Mediterranean for instance, have you observed a loss of insulation at the time that the cable first passed into the water?—I was not officially connected with the cables in the Mediterranean. I was there as a volunteer.

1551. Were you with the experimental expedition, which was made in the Bay of Biscay previously to the laying of the Atlantic cable?—I was.

1552. Is there anything that took place which bears upon the subject of our inquiry?—I think it proved what we could do with a cable of the description of the Atlantic cable in great depths.

1553. In fact, was it a satisfactory proof?—Very satisfactory I think to those who were then present and knew the particulars, for in that depth of water, 2,000 fathoms, we lowered the bight of the cable to half its depth, and hauled it up again perfectly well. We wanted to prove whether we could in that great depth haul a cable on board and repair it, supposing a fault had been paid over the ship, and that was successfully accomplished.

1554. Should you prefer a cable of that description, (*No. 2, deep sea, Gibraltar,*) to the Atlantic cable for laying. The specific gravity is very much lighter than that of the Atlantic, and it will bear its weight in 5,000 fathoms?—I am not an advocate for so much hemp.

1555. Because you have observed that the hemp perishes?—Yes.

1556. That is Mr. Gisborne's proposal for the second deep-sea Gibraltar cable for the deep water across the Bay of Biscay?—I am of opinion now that there is too much hemp about this, and I should prefer a larger wire.

1557. What is the injury that you anticipated would arise from the presence of too much hemp?—I do not think the cable would bear a flattening pressure coming upon it, it would get out of its cylindrical form.

1558. In the process of coiling?—Yes, in the process of coiling, and also in paying out. Supposing we had any great vertical strain over the drums, it would flatten this cable much sooner than it would a more solid cable. These cables have never been tested yet in coiling. I do not anticipate that there would be any difficulty arising from that, but still it may open.

1559. Could not some small specimens be subjected

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to tests previously?—Yes, by making some lengths of it.

1560. What length would be sufficient for testing it properly?—I should say a few hundred fathoms.

1561. That ought to be submitted to the same pressure that it would receive in the coil?—Yes, and also to be run off quickly to see how it pays out, because there are some cables that will not lie in the coil so quiet as others, they begin to rise up. The great secret is to keep your cable quite still till it is wanted.

1562. Might not mechanical arrangements be made to keep down the several portions if they have a tendency to rise up before they are wanted?—I think it would be far better to adopt a cable which will not rise.

1563. You would prefer a cable which will remain quiet from its own formation?—Yes, I should try to obviate all those difficulties in the form of the cable. I should not like to trust to any mechanical contrivance to obviate them.

1564. Do you prefer the spiral lay to a cable made like that specimen of Messrs. Wells and Hall's?—I prefer the spiral lay most decidedly.

1565. Would not the result of that lay be that it would assume the spiral form?—Yes, it would; in coiling it you never could keep the same surface uppermost.

1566. It would be a rope with a long spiral?—Yes; when you came to uncoil and pay it out you never could keep the same side always uppermost.

1567. Therefore there is no reason why you should not make the covering spiral in the first instance?—No.

1568. Have you turned your attention to the question of the insulation of cables?—No; we have had a great many things presented to us of course, but we have never tried any, not practically.

1569. Do you think experiments could be made upon them to lead to a satisfactory result?—The only way that you can do it is to put the material under pressure and for a lengthened period.

1570. That is assuming that the question which you wish to determine is whether they are permeable by water?—Yes, and also what effect the water would have upon them; we know that gutta-percha will last under water well for eight years.

1571. And you know that india-rubber will resist water perfectly well?—Yes.

1572. You think it may be a question how far those compounds may last?—I think that is to be proved.

1573. Are you aware whether the compound called Chatterton's compound is durable under water?—No, I am not, because I have not seen any that has been tested. The cables in which it has been used have not been under water sufficiently long, I think, to give any evidence of what their durability may be.

1574. Have any of Glass and Elliot's cables been made with Chatterton's compound, alternating with gutta-percha?—Yes.

1575. Then you considered it would be a good thing, or you would not have used it?—Of course, we considered it to be a good thing from experiments that had been made by electricians who reported favourably upon it to us.

1576. (*Professor Wheatstone.*) Have you any objection to the employment of india-rubber as an insulator?—I am speaking of the mechanical part of paying out, or in the making of the cable. I have not turned my attention so much to electrical matters as I have to the engineering. But as regards heat affecting gutta-percha, there was a question which arose about a part of the Atlantic cable being laid in the tanks at Greenwich exposed to the sun. After that I made some tests by putting it in water, and heating it up to various temperatures, and attaching a weight to it. I found that up to 125°, with a pressure of about 75 lbs. to the square inch, the copper wire remained in the centre, just the same as when it had been drawn in a straight line from the machine at the gutta-percha works. The upper flakes, as it laid on the coil, were damaged. I chalked all the upper part of the top flake which had been exposed to the sun, and on cutting the cable in various places, I found the copper wire to be sometimes close to the outside of the gutta-percha on the bottom of the flakes, sometimes on the top of it, sometimes on the inside and sometimes on the outside of the turn in the flake. Therefore I do not think it was the action of the sun in the tanks at Greenwich which caused the copper wire to leave the centre.

1577. Do you imagine it to have been a defect in the original manufacture?—I think it was the effect of running it too quickly over the drums after it had been covered, because I could not reconcile in my own mind how it was that it must have repelled the copper in some instances, and in other instances have attracted it to the outside of the gutta-percha.

1578. When you are saturating hemp with tar, at what temperature do you do it?—We boil it; it is put on gratings and drained, and allowed to remain there for some hours; when put on the cable it is cold.

1579. (*Chairman.*) This is a specimen of Mr. Latimer Clark's process?—Yes; this has been melted and the gutta-percha passed through it in tanks.

1580. Has it injured the gutta-percha?—Yes.

1581. Is there no means of applying this process without injuring the gutta-percha?—Yes; it must be put on at a certain temperature of course, for the pitch to be in a sufficiently liquid state, but it can be put on at that temperature so as not to hurt the gutta-percha.

Adjourned to Wednesday next at Three o'clock.

Wednesday, 14th December 1859.

PRESENT:

Captain Douglas Galton.
Professor Wheatstone.
Mr. Saward.

Captain DOUGLAS GALTON in the Chair.

Mr. T. Allan.

14 Dec. 1859.

Mr. Thomas Allan examined.

[*Note.*—The following letter has since been received from Mr. Allan.]

To Captain Galton, R.E., Chairman of the Electric Telegraph Cable Commission.

1, Adelphi Terrace,
Jan. 2, 1860.

Sir,

On the 14th ultimo I attended, at your request, the Electric Telegraph Cable Commission at the Board of Trade.

Considering circumstances, and the peculiar position in which I stand relatively to the matter in discussion, as the originator and promoter of two important undertakings long since constituted as joint stock companies, (viz., the Great Indian Submarine, and the Great Ocean Telegraph Company,) for the purpose of carrying out direct telegraphic communication from Falmouth *vid* Gibraltar and Malta to India, and from Falmouth to Halifax (N.S.)

direct; as also being the patentee of the only means suitable for efficiently and economically carrying out ocean telegraphy,—I was much surprised, in such a branch of science, to find myself under the examination of the secretary of the late Atlantic Telegraph Company, as also that of a Mr. Siemens, who, I find since, forms no part of the Government Commission.

As many of the questions put to me on that occasion were in no ways such as to elucidate the subject, I must, in justice to myself, the Government, and the public, request that this and the following statement as to my systems of ocean telegraphy be annexed to my evidence on that question.

I am, Sir,
Your most obedient,
THOMAS ALLAN.

ALLAN'S SYSTEM of OCEAN TELEGRAPHY.

The principal feature in this system of telegraphy, besides the production of electricity most effective for working extra distances of submerged wire, is a deep-sea rope or conductor, combining the maximum of conducting power and strength with the minimum of bulk, weight, and specific gravity, thereby rendering its safe submergence in ocean depths a simple mechanical operation.

The mechanical principle in the construction of these ropes is the reverse of those hitherto used, the peculiarity being that the whole metallic strength is placed in the centre of the insulating medium,—thus forming an inextensible core, and so preserving the insulation from injury by tension, in place of the self-destructive spiral wire casing as heretofore.

In a cable constructed on these principles, suitable for the distance between England and America, the core or conductor is composed of a solid copper wire, surrounded with *steel* wires, in itself capable of bearing a strain of 21 cwt. without *yielding*. The weight of the cable is 8 to 10 cwt. to the mile, and under 2 cwt. in sea water; its weight, therefore, could not in any degree injure its electrical qualities, as the core or conductor alone is capable of bearing upwards of 10 miles of the cable, hanging vertically from the ship, before the insulating and conducting medium could in any degree be injured by tension.

The conducting power is threefold greater than that of the late Atlantic cable, whilst the insulating medium is much thicker, and prepared so as to withstand heat and pressure.

From its lightness and portability, upwards of 3,000 miles can be conveyed in one vessel; and the paying-out gear being dispensed with, it can be veered out at the rate of 8 or 10 miles an hour with facility.

From the very great increase in the conducting power and insulation relatively, besides dispensing with the iron envelope hitherto used, a very much higher rate of signalling can be attained.

Under this system, it is calculated there will be comparatively an economy of 40 per cent. on the first cost of construction, besides 50 per cent. on the working.

1582. (*Chairman.*) You are a civil engineer, I believe?—I believe so.

1583. You have devoted considerable attention to the science of telegraphy, I think?—Yes.

1584. And have taken out several patents?—Various patents.

1585. And I believe recently you have devoted your attention to the construction of submarine cables?—It is very long since I turned my attention especially to submarine cables.

1586. Will you describe to the committee the system of telegraphic cables for submerging in deep seas which you consider to be best adapted to the purpose?—In the first place I may explain that the experience I had in laying the line between Holyhead and Dublin, which gave way the next morning, first drew my attention to the mechanical philosophy of the construction of submarine cables.

1587. In what year was that laid?—In 1852. It was quite clear to me as a mechanical question, from the strength and weight being placed outside in a spiral form, it admitted of a great amount of stretching, and that stretching upon the copper thread or conductor and gutta-percha was very apt to destroy the working condition of the rope; and looking to what we might have in future to do, I turned my attention to remedying this defect; in the first patent I took out on the subject of submarine cables I reversed this mechanical construction of the rope by, putting the strength in the interior in place of the exterior.

1588. That was in 1853, I think?—Yes; they were both in 1853; that was in the early part of 1853; that rope admitted of no stretching as compared with the principle of the rope that was adopted as the first type of submarine cables. The Dover and Calais cable upon a great strain would stretch considerably before it broke; my cable would rather break before it stretched, if I may so use the expression; of course metal will stretch.

1589. Do you make a difference between cables for shallow water, and cables for deep water?—Certainly.

1590. What difference do you make?—The cables in shallow water, more especially in tideways, ought to be very strongly mailed indeed (there are a variety of ways of doing it), in the first place to make them heavy, to make them stationary, and so to speak to protect them from friction; when you come into a little greater depth, you can submit to have less of that power which is required to resist the friction of stones or anything that may occur at the bottom. When you come into deep water you require a rope of as light a specific gravity as will in its own nature sink, it must not be too light; and in that way take the strain off itself.

1591. In any case, if I understand you, you would put the strength in the centre, but for shallow water you would put an outer covering of hard metal?—It would be useless to put all the strength in the interior in the shore ends,—quite the contrary, because you want your insulating material, whatever it may be, to be protected from being cut and damaged.

1592. For shore ends or for shallow water you do not object to the construction which has been adopted in the Dover cable?—I would have a modification of that. I would not follow that exactly.

1593. What modification would you suggest?—I should prefer something that I have seen of Mr. Walker's. I should prefer for the shore ends to have a complete spiral at right angles to the conductor, and then make the longitudinal spiral wires over that; that forms a tube and prevents the exterior wires crushing in, or even themselves elongating, and you would have then exteriorly the same effect as the first patent I took out, in which the iron wires were interior. My first patent was for a cable to carry three wires. A strand, as you are aware, is made up of seven circles. In that case I took four iron wires, No. 1's or No. 2's, and three gutta-perchas; so that the three iron wires that went spirally over the centre one could not stretch on account of being placed on a metal core, and therefore the three conducting wires in the intervening spaces could not be either stretched or crushed.

1594. What was the central core?—Iron. Three gutta-percha and four iron wires made into a thick strand.

1595. What would be the thickness of the central core?—In that case, instead of taking four No. 1 iron wires, I took three No. 1's, and one No. 2 for the centre. You do not probably at first see the object of that, but it was to throw the gutta-perchas in from the circumference, so that anything coming across it touched only the iron wire, and did not come upon the exposed gutta-perchas.

1596. The iron wires being larger in diameter than the gutta-percha wires?—Yes; it did not make an exact circle from that reason, and there might have been also other small wires laid over the gutta-percha,

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1648. You propose to do away with breaks or any retarding force?—I do not need them. You required them absolutely in laying the Atlantic cable. The success of its laying was very much owing to reducing the strain and sailing faster.

1649. Would not you pay out a great quantity of slack?—I do not think you would need to do that; if you go too slow compared with the specific gravity of your cable, you will pay out too much. I think the speed and specific gravity are possible to be made relative quantities.

Instead of the table handed in showing the difference between the system of deep sea cables I propose and that hitherto in use, I have selected for comparison the cable as specified by the Board of Trade for a line to Gibraltar.

On my principle, which is a solid copper wire encased in steel, I am able to obtain a conductor equal to that as specified by the Board of Trade for Gibraltar, with no more inductive surface, and therefore with the requirement of no greater amount of insulation, and forming a cable relatively threefold stronger.

Further, it is perfectly inextensible, and the steel alone can bear a strain of 24 cwt., or 7,000 fathoms of itself in water. It does not weigh more in ship than 9 cwt. per mile; in sea 3 cwt.; specific gravity 1·50; cost per mile, 130*l.*

In the case of the cable as specified by the Board of Trade, with the same amount of conducting power and insulation, the deep sea part weighs over 2 tons per mile, and loses very little of its weight in water, and cannot be made for less than 200*l.* per mile, which is exclusive of the great expense in shipping and great risk in submerging.

(*Witness.*) The great advantage of having the strength in the middle, like the bone in one's arm, is to prevent the conductor being attenuated, or the insulation stretched; if it is stretched, it is apt to become porous; in fact, the object is to try to conserve what you have already made good, so as to prevent its being injured during the process of submergence, which is the time when the accidents occur. An iron covered cable may be perfectly good when made, and would do if we could just take it to the bottom and lay it down; but, unfortunately, the untwisting of the wires, the kinking, and stretching very much deteriorate the electrical conditions of the cable during submergence; that is the reason I select an inextensible core, and can adopt a solid copper wire in place of a strand of wires.

1650. (*Chairman.*) You put a solid wire inside because there is no extension?—Yes; take the largest sizes in all these specimens, the conductibility is equal; when you come down to a smaller gauge you have more steel and more strength, proportionately to the copper, than you have in a larger one; therefore, I do get so far into the mistake of having a little more inductive surface. But again, as compared with that, you give yourself ample conductibility; it is the same current that you are passing, and you do not have the mal-effects from that induction.

1651. (*Mr. Saward.*) As regards expense, how will your cables compare with other cables?—These are about 30 to 40 per cent. less than the Atlantic cable. I think, in the Atlantic cable about one-half of the cost was for the exterior wire coating. The great object of an inextensible core is the conservation of the vitality or working integrity of the rope; that is, the conductibility and the insulation. Having once got them perfect, it helps you to prevent injury.

1652. What amount of strain should you expect that cable would bear?—About four times what the Atlantic does relatively.

1653. (*Chairman.*) In what depth would you commence to lay conductors like that?—I would begin at 100 fathoms; you do not need any outward covering beyond that.

1654. You would not be afraid of any movement of the water operating?—No; I am not afraid of any movement; at 100 fathoms I believe the water is as quiet as it is at the bottom of a well.

1655. What insulator do you consider the best?—I have not turned my attention to insulation; I have left that to others. I have taken generally gutta-percha as my unit. I have made my specimens of rope with gutta-percha, and covered them either with that material or with hemp; if I were going to do any great work I should certainly investigate the question of insulation much more closely than it has been done, but I believe that this committee which has been appointed by the Government is going into that subject, and it will save us all a world of trouble. It is a very important point; the great point is to get it perfect and cheap.

1656. Have you turned your attention to paying-out apparatus for cables?—If you have a rope of light specific gravity, and you pay it out at a speed equivalent to its specific gravity, you do not require any paying-out apparatus at all; at least you do not require any friction break drums, you ought to have *paying-out* machinery in the real acceptation of the word; it ought to be paid out by men's hands, or something as a substitute for them; you do not require the friction break drums to stop it or arrest it, or anything of that kind. When there was a gale of wind you might require something to give you a sort of control over it, either to hold on to a certain extent, or to throw it out. I think one of the great points is to dispense with this, I will not say clumsy affair, but I think it is a thing *de trop*.

1657. Have you seen Mr. Longridge's model?—No, I recognize it; I have had it explained to me.

1658. Do you consider the principles which Mr. Longridge advocates sound?—Well, I think they are; only I still say to him that I scarcely think it is necessary.

1659. Would not you be afraid of paying out a large proportion of slack?—You must put on your steam and go a little faster.

1660. There would be a limit to the speed at which you go, would not there?—I do not know; you can go nine or ten miles an hour, what you have to do is to pay and go.

1661. (*Mr. Saward.*) It has been suggested as a modification of your plan that iron or steel wires might be laid longitudinally across the second coating of gutta-percha, and the third coating put on over that, so as to produce greater strength in the inner portion of the cable; would you concur with that view?—Do you mean to insulate the copper first, and put the steel wires outside?

1662. Covering them so as to make an external surface?—I should think that very bad indeed. In the first place you would be more apt to create induction by putting the metal between your copper and the water, whatever may be the exterior; and the wires, if they were put straight, would be sure to be cutting through.

1663. The cable would also be much heavier?—It would be heavier. I do not think it is at all a good mechanical arrangement; I use steel in the place of iron, because I can get the same strength in less bulk, though it is more expensive; it becomes more economical in the end, because you require less gutta-percha, and you have less surface.

1664. (*Chairman.*) Is there any other statement which you wish to make?—The form of cable I propose is a great saving of material, it is more economical than the usual form of rope; conductibility and insulation being equal, there is a saving of about 30 or 40 per cent. in the cost; it is light enough to carry extreme lengths in one ship; it is light in specific gravity, and it requires no friction break drums in in the operation of paying it out. The electrical question depends upon apparatus to be used and other things. The problem we have to solve is how we can construct and lay a cable without its injuring itself during the process of submergence. We can make cables perfect, but after we have got them made perfect, we destroy them in attempting to get them to the bottom; the consequecne is, that the working

integrity frequently gives way afterwards from the injury that they have sustained during the process of submergence. We had had several instances of that, but if we could get the cable to the bottom of the sea in the same condition that it was in when we started it, it would lie there without any chance of injury at all, as quiet as at the bottom of a well. I am not against iron covered cables for shallow waters, there they are necessary; but for deep sea purposes they are self-destructive.

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Mr. LEONARD WRAY examined.

1665. (*Chairman.*) Have you turned your attention to the best substances for insulating telegraphic cables?—I have.

1666. Will you give the committee an account of the substances which you consider best adapted to the purpose?—Perhaps you will allow me in the first place to tell you how I came to turn my attention to this subject. I am not an electrician; but when in America I heard that the Atlantic cable that was made was supposed to be capable of sending only five words a minute, I revolved that in my mind, and thinking that there must be something very wrong, I jumped to the conclusion that the insulation must be at fault. I said, "Either the cable is erroneously "constructed, or the insulating material must be bad, "consequently we ought to have a better material." When I returned to England, I turned my attention to that subject; I waited upon Mr. Saward, and talked the matter over with him amongst others. The result was, that after trying a great number of things all in the same direction, I produced this material (*producing samples*). There are several combinations included in my patent, but after I had satisfied my own mind, I took the advice of Mr. Saward and others, and those gentlemen said the best way is to get the opinion of some electrician in whom the public had confidence,—I think that was the expression, Mr. Saward,—and it was likewise the advice I received from many others. I therefore took specimens to Mr. C. F. Varley, a gentleman indicated to me by these advisers. Being a perfect stranger to him, I said, "Mr. Varley, this is a material that I have in- "vented as an insulator; I come to you as a stranger, "and hope you will not take it as a liberty, if I "put this in your hands to test." I left it with him for two or three months that he might have full time to test it; he then wrote to tell me that he found it so remarkable that he should like to see me on the subject; subsequently I furnished him with further specimens, and after I had given him ample time (I am talking of as far back now as August and September 1858), I said, "Well, now, what do you "think of the specimens that I have sent you; would "you give me your opinion?" He said, "Well, in a "day or two I will write and let you know what I "have found them to be," and he wrote me the following letter, dated April 4th, 1859:—

"DEAR SIR, "April 4, 1859.

I HAVE tested the samples marked 1, 2, 3, 4, and 5, against Chatterton's g.-p. wire and Atlantic strand.

The results are extraordinary. No. 1 tests very well, but the piece I have is so short, viz., 6 inches, that I can only say I think it equals specimen No. 4.

No. 3, the softest specimen, is the least good of all of yours, but it is 700 or 1,000 times as good an insulator as Chatterton's line.

No. 4 is twice as good as No. 5, and three times or four times as good as No. 3.

No. 2 almost equals No. 4.

In a word, Nos. 1, 2, and 4 are very much better than No. 5, and this better than No. 3.

The worst being 700 or 1,000 times as good as Chatterton's wire, which is better than Atlantic strand.

I am, dear sir,
Yours very truly.

L. Wray, Esq. C. F. VARLEY.

1667. (*Mr. Saward.*) That means better as to insulation?—Yes; in fact he tells me that he has not found a limit to some of the specimens which I have given him. I am able to hand you the three specimens, 1, 2, and 4, mentioned in that letter (*producing the same*).

1668. (*Chairman.*) Have you tested this material with a view to discover whether it is permeable by water?—Yes, it has been in water a considerable time.

1669. Under pressure?—I have not tested it under pressure. I have every reason to suppose that it would be just as impermeable as gutta-percha.

1670. (*Professor Wheatstone.*) What is the composition of your best specimens?—That requires explanation. I have found that the best insulator is not the one which, perhaps, a large company would like to accept for a cable, because we have so many qualities to provide for; we want tenacity, we want hardness, and we want exceeding strength. Some of these specimens which test so remarkably, (even 6,000 times better than gutta-percha), are not strong enough; if they are twisted backwards and forwards for a considerable time, they will break; but you may twist this as much as you like, and you will never break it, though you may break the copper wire. Here is a wire that has been made ever since April last, and you will observe there is perfect adhesion between the copper wire and the material; the copper does not act on the material in any way whatever.

1671. (*Mr. Saward.*) Have you obtained opinions as to the chemical destructibility of your material?—Yes, it will not be destroyed, it is perfectly safe under all ordinary conditions.

1672. (*Chairman.*) Has it been tested by exposure to the atmosphere for any length of time?—Yes, all these specimens have been exposed to the atmosphere, to water, and to other tests.

1673. (*Professor Wheatstone.*) Is it cheaper or dearer than gutta-percha?—It is cheaper.

1674. (*Mr. Saward.*) Can it be put on by a die?—Yes, some of these specimens were put on by dies; that larger specimen which the chairman has in his hand was put on by our new die machine; it was merely to try the machine.

1675. (*Chairman.*) In a single coat, or two?—Two coats.

1676. (*Professor Wheatstone.*) Can you get perfect centering with this material, this specimen is very imperfect?—Yes, that piece was done by hand machinery and rollers. We did not use water to cool any of these specimens as they came from the machine.

1677. (*Chairman.*) Was it done at the gutta-percha works?—No, at the old Caoutchouc Company's works at Tottenham, by the firm of William Warne and Co.

1678. Is it made under a patent?—Yes.

1679. Of what is it composed?—There are several combinations; there are three classes of substances, the gum-elastics.

1680. What do you mean by the gum-elastics?—India-rubber, gutta-percha, and so forth; shellac or other resinous substances; and siliceous matters, and aluminous; those specimens (the cubes) were made of the material that happened to be at hand; that is, flour of glass, instead of powdered flint. I prefer powdered flint.

1681. (*Mr. Saward.*) Are those substances combined by heat?—Yes; by means of large steam-heated iron rollers.

1682. (*Chairman.*) Is it a chemical compound?—I do not think it is chemically combined; there are very few substances that are chemically combined; persons may suppose such combinations to be, when they are not. We have had tests of electrical currents going through our specimens for months, and we do not see the least alteration in them in any way whatever. This specimen has a little gutta-percha in it; but it is a singular fact, that the more gutta-percha we put in, so does our insulation decrease; we found that out as we were going on with our experiments.

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1683. What are the proportions of the several materials that you use?—That certainly depends upon circumstances; we have so many different uses for it.

1684. What proportion would you use for a submarine cable?—This particular piece (*holding up a sample*) is a combination that combines so many good qualities; it still insulates, I believe, infinitely better than gutta-percha, so that we can afford the loss of insulation which results from the gutta-percha we have put into it; the gutta-percha makes it lose its insulating properties to a certain extent, but we consider that combination sufficiently good for all purposes of insulation.

1685. Is your compound made in layers of the different materials?—No, that appearance in the specimen arises from the cutting-machine.

1686. What proportion has that specimen of the different materials?—I do not like to give the exact proportions; these are the only little things that we can keep to ourselves; there are so many dishonest people trying to cut the ground from under one that they will add a pinch of one thing or a pinch of another to our combinations, and then they say that is not your material, and they appropriate your invention.

1687. In fact, you prefer to keep your proportions a secret?—I have given good workable proportions in the patent.

1688. What proportions have you given in the patent?—I think I stated two or two and a half of india-rubber, one and a half of shellac, and one of powdered flint or powdered glass; but those that I have stated in my patent really form a most excellent material; still we vary the combination.

1689. How were you led to the discovery of this material?—I thought we wanted a better insulator; when I commenced I was thinking of soluble or malleable glass,—Mr. Saward may remember this;—but I found that that was an impossibility. I ultimately hit upon this combination.

1690. It was endeavouring to discover a soluble glass that you mixed these other materials with powdered glass?—No. I thought we could get malleable glass of some kind, but I found we could not; then I said we must have a mixture of india-rubber, shellac, and silica; I knew that those things were the best insulators in the world; you cannot have a better insulator than shellac. India-rubber is also a capital insulator, and the combination is so extraordinary that I prefer it to india-rubber, or everything else. Nothing has the least chance with it.

1691. (*Mr. Saward.*) Are your prices published yet for comparison with those of the gutta-percha company?—No, they are not. We have two 50-horse power engines,—call them two 100-horse power engines, now erected, the fly-wheel up, and all the other machinery rapidly progressing.

1692. (*Chairman.*) As I understand, you do not intend practically to give such a good combination with respect to insulating power as that which you submitted to Mr. Varley on account of the advantages which a less perfect insulator possesses in a mechanical point of view?—Precisely. Immediately we begin to use gutta-percha the insulation goes down; in exact proportion to the quantity used so does the insulating power decrease. We sacrifice that for the greater hardness it gives and the greater toughness. We lose in insulation, and we lose in temperature; it will not stand in so high a temperature with gutta-percha, as without.

1693. (*Mr. Saward.*) At what temperature would that material become plastic?—Much higher than gutta-percha; I am not prepared to tell you. There are a series of experiments going on at the works to prove that, but it is much higher. I may state to the committee that I have prepared pieces bent in this manner (*showing the manner*), and pieces of gutta-percha of equal diameter under exactly similar circumstances, and immersed them in boiling water. In the gutta-percha the wire has come completely through, but mine has not altered its shape in any way. In another case we tied them together and suspended them in a kettle of water, and boiled the water for five minutes. The gutta-percha completely boiled out of all shape, but mine has not been injured in the least. It also insulates up to 150°, and even higher. Perhaps I may be permitted to make a suggestion quite apart from the question of insulation. I am sure that the committee wishes to arrive at everything that is useful with regard to electric cables. I would suggest that the committee make very careful tests of aluminum and its compounds for conducting; the tests that have been yet made, show that it is a most excellent conductor of electricity, and its tenacity is very great.

1694. It shows that it is equal to copper in its conducting power, and equal nearly to iron in its tenacity?—Yes, so it is said, but I think that it is a question that should be very carefully investigated.

1695. Is it not very much a question of price?—That is a question that can be very easily solved. I do not know if you are acquainted with the recent processes that have been brought forward. I have been in communication with chemists on the subject, and I believe that it can be supplied very cheaply. We must bear in mind that it is five times lighter than copper, and that is a very great thing.

1696. It would be useless as an external covering, would it not?—It could not be employed as an external covering, because it is destroyed by salt water.

1697. (*Mr. Saward.*) Is not aluminum destructible?—If it is exposed to salt water, and so in reality is copper; nevertheless I think we shall find our conductor for electric cables in an alloy of aluminum.

1698. (*Chairman.*) An alloy with what?—Copper or silver; a little silver, I think you will find, will improve it very much.

1699. Is your material uniform in the mass, that is to say, are the ingredients so intimately mixed that if you cover a mile or ten miles of wire you can always depend upon having the same mixture?—Perfectly; nothing could be more satisfactory to the committee than to cut off portions here and there, and submit them to the highest chemical authority in the kingdom. The mixing is very carefully done on a large scale. I do not think there can be the least doubt of it; if you were to see the process of mixing, you would be of that opinion.

1700. (*Professor Wheatstone.*) Have you any objections to the employment of india-rubber as an insulator, and if you have, what are they?—In the first place, india-rubber brought in contact with copper is decomposed; and in the second place, india-rubber, even where it has a material next to the copper to protect it from the copper, does not insulate in any degree like this compound which I am now laying before the committee, for I have tested it. I have tested the most carefully made specimens of india-rubber (with cotton or what not between) that have been supplied to me; and I find that they do not bear the slightest comparison with this material of mine; they are better than gutta-percha, but no comparison with this. India-rubber, again, is a softer material. I do not wish to speak against any other material, all I wish is to lay before the committee the facts connected with my own.

1701. (*Chairman.*) What will be the price of this material as compared with gutta-percha?—It will certainly be less than gutta-percha; I should not like to say how much less;—and we have works to carry it on to any extent.

1702. (*Mr. Saward.*) Materially less?—I presume so. We must allow ourselves a margin, inasmuch as these materials will go up as the demand for them increases; for instance, shellac is now 140*s.* per cwt., and in my experience, but very recently, the same quality of shellac could have been bought for 75*s.*

1703. (*Chairman.*) Do you think there will be a sufficient supply of shellac if there is a great demand for your material?—Yes, a great quantity of shellac comes from that part of India where the disturbances

have been: along the Gogra and those other districts that have been devastated by war, an immense quantity is produced, and I presume that the war must have interfered with the supply.

1704. You think the increase of the price of shellac is attributable somewhat to the war?—I think so, and to increased demand, those two causes combined. The position that I have taken up, in respect to my material, is to supply every one and any one. It is not locked up in the hands of any one particular company, or set of men, and I am in opposition to no one.

1705. If, for example, we wish to put an extra coat on a portion of the Gibraltar cable, would there be any difficulty in laying it on the gutta-percha?—No, I have already ordered the dies for it. I understood that such a thing was possible, and I have ordered the dies to be made, so that I shall be in a position to give some of it in the course of a few days, as a sample.

1706. Will not there be an objection, inasmuch as your material must be worked at a higher temperature than gutta-percha?—No, it passes only through the dies, it is a momentary process; it is not like passing through a cylinder; before it can be affected it is out again.

1707. Your material is not contained in a cylinder through which the core passes?—No, the contact is momentary; I have taken the opinion of those gentlemen connected with me who have dealt with india-rubber for a great number of years, and they join in opinion that it will not affect gutta-percha in the least. The day before yesterday I took to the works a piece of Gibraltar core, and I said, "In passing "through the dies, do you think the higher tem- "perature will affect it?" I was told, "Oh dear no, "not at all. In fact we are convinced that it will "not do anything of the kind." I said, "I shall want "some dies made than," and they are having the dies made now.

1708. You never would have to stop, so as to allow the core to remain in this heated place?—No; it would be continuous. The higher temperature at which our material works is one of the greatest advantages it possesses. We may call it a stubborn material, but there is a great advantage in its stubbornness, for it is not influenced by heat so much: this specimen passed from the machine at once over the drum, yet you see it has not lost its centre. With gutta-percha we could not have done it; it would have been a perfect impossibility; but the committee can try pieces at any time in boiling water; and they will also find that it does not lose its insulating power, even up to 150°.

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1709. At what temperature would you propose to put it upon the Gibraltar core?—I should like to ask the gentlemen who are preparing me a table of all these little matters; I haven't them at present with me.

1710. You have persons assisting in your experiments?—Yes, at Messrs. W. Warne and Co.; it is a very large establishment, there are 300 people working there; it is now a waterproofing establishment, but in taking up a thing of this kind largely they would probably do away with the waterproofing branch of their business, and throw a large force upon the insulating material.

1711. If there is a demand for it?—Yes; and if not, they have their business still going on.

1712. Have you made any experiments for the purpose of showing the inductive capacity of this material?—There have been experiments made by Mr. Varley, and he tells me that he entertains very good hope of it, but the experiments are not in a state that they could be mentioned at present, they have not arrived at any very conclusive results; he says he believes that the induction will be lessened. My own idea always was that by producing a thoroughly good insulator, in long distances you could use a much smaller core, that is a smaller conductor; for I imagined that the reason why a very large core of copper was required was that it required a larger amount of electricity to compensate for the great leakage; that was the crude idea that I formed, and this was how I was led on to attempt the discovery of a very perfect insulator; I do not pretend to be an electrician at all.

Adjourned till To-morrow at One o'clock.

Thursday, 15th December 1859.

PRESENT:

Captain Douglas Galton.	Mr. Saward.
Professor Wheatstone.	Mr. Varley.

Captain DOUGLAS GALTON in the Chair.

Wildman Whitehouse, Esq., examined.

W. Whitehouse Esq.
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1713. (*Chairman.*) What is your profession?—I am a member of the College of Surgeons, but not now practising; lately I have devoted myself to electro-telegraphy, and that must be called my profession.

1714. I believe you have given great attention to electrical questions, and especially to deep-sea telegraphy for some years?—Yes, I have.

1715. Since when?—For the last eight or nine years.

1716. I believe you made experiments with Mr. Brett?—I did. One of the Mediterranean cables, for which Mr. Brett was the contractor, was the first one that I experimented upon. The object of my experiments was to determine as many points as I could connected with deep-sea telegraphy; the laws of retardation, and induction, and the most suitable form of instruments to be used; in fact, I spent almost night and day for three years, working constantly, in trying to determine all the points that I could.

1717. What was the longest of the cables that you experimented upon previous to 1856?—160 miles was the length of the cable, as well as I remember, containing six wires.

1718. Was that cable coiled?—Yes, it was coiled in a tank at Messrs. Glass and Elliott's works, at Greenwich.

1719. What were the general results of those experiments?—It is difficult to give them in a few words. The first object was to ascertain, as nearly as I could, the law governing the retardation of the current at different distances, in order that I might discover the limits, if any, beyond which deep sea telegraphy would cease to be practically attainable.

1720. (*Mr. Varley.*) I think you said that the cables were 160 miles in length upon which you experimented; did not you experiment upon a greater length than that?—At a later period I did, but not at that time.

1721. Was each wire 160 miles in length?—166 miles, I think, was the exact length.

1722. (*Chairman.*) Did you join the wires to-

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gether ?—Yes, I joined them together. The whole cable was 166 miles *in* length, and it *contained* six insulated conductors.

1723. Therefore you got six times that length ?—Yes.

1724. What was the nature of your experiments ?—The first was with regard to the velocity of the electric current under varying circumstances, trying the length of time that it occupied in travelling the 166 miles, then comparing it with the time required for two lengths of 166 miles, and three, four, five, and six lengths.

1725. Were the results satisfactory ?—They were remarkable, certainly, and their invariable uniformity under like conditions was in the highest degree satisfactory. I experimented upon different forms of current, and upon different modes of using the same form, I mean the voltaic current as continuously employed by interruptions or repetitions of similar currents, or otherwise, alternately, a positive and a negative. Then I tried the magneto-electric current from the induction process, and from permanent magnets also. My object was to work out, as far as I could practically, the best form of instrument for use in submarine lines, the best form of current and the best mode of using it.

1726. What result did you come to ?—I found that the voltaic current was at first very difficult to use with advantage in so long a length, rapidly. I found that its rate of transmission was slower than the magneto-electric current ; I found if used in repetition instead of alternating, that the charge of one current was blended with the charge of the next current, and that it came out almost as a continuous discharge at the other end of the whole length of the cable. I had, in some of the experiments, a seconds pendulum beating, giving the contact every second at one end of the circuit, which gave the voltaic current punctually as the pendulum beat ; at the end of the whole distance, through the six wires joined up in a single length, it came out as a continuous discharge, without interruption. There was no variation in the current perceptible on the galvanometer receiving it, so that I then tried reversing, making the pendulum reverse the current at each beat one second positive and one second negative ; I could then get distinct signals through the whole distance from that end, the negative current discharged the remains of the preceding positive current, and left the wire comparatively empty and ready for another current, so that you got a small portion of each current ; a considerable portion of the current was consumed in discharging the preceding one ; but a portion passed through the cable and was perceptible as a signal through the whole distance.

1727. Did you employ weak currents or strong currents ?—The widest range which I had at my disposal.

1728. (*Mr. Varley.*) Can you state the limits that you started from ?—I started from a single pair, the size of a sixpence, and I recorded signals through 1,000 miles with that distinctly, beating seconds alternately. I think the highest number of cells I had was 360, of the ordinary zinc and copper plates.

1729. What sized plates ?—I used four-inch, the usual size.

1730. How were they charged ?—They were charged with acid ; it was the ordinary sand battery, such as is used at the gutta-percha works.

1731. (*Chairman.*) You also tried, you say, the magneto-electro currents ?—Yes ; I tried the voltaic current, beginning with the very weakest I could send. It required an exceedingly sensitive instrument and very perfect adjustment to show anything from a single pair. Then I gradually increased the number of pairs till I got to 360. I tried as far as I could to get at the ratio of the velocities of those different powers.

1732. With what general result was that experiment attended ? Of which current was the velocity the greater ?—Up to a certain point there seemed to be a question whether a sufficient amount of current arrived to actuate the instrument. It was rather a matter of the arrival of a portion of the current than the arrival of the maximum current, because beyond that no increase in amount or intensity appeared to augment the velocity at all. With about 18 or 20 cells I always found the slips recorded as high a velocity as with 360 cells ; below this the observations were extremely critical and liable to instrumental error.

1733. You required a more delicate instrument ?—I required a more delicate instrument. Although I could adjust the instrument to receive a signal from a single cell through 1,000 miles, yet I could not depend upon its accuracy to a minute fraction of a second for measurement of time.

1734. (*Mr. Saward.*) Were those signals received by relay ?—Entirely. I have records of hundreds.

1735. (*Chairman.*) Will you be good enough to communicate those records ?—Yes. In order to ensure accuracy I made the current on leaving the battery before entering the cable pass through a relay to make its mark, and on leaving the cable and entering the earth it passed through another relay, and made its mark ; these marks were made by two styles on the same slip of paper.

1736. Was that in order to measure the speed of its travelling ?—Yes, the recording mark was the electro-chemical decomposition.

1737. (*Mr. Varley.*) You have not stated the size of the wire nor what form the insulating material was ?—It was an ordinary sixteen gauge wire, double covered with gutta-percha of the usual thickness.

1738. (*Chairman.*) Were the wires joined together at the ends ?—I had the command of the whole of the twelve ends ; I could either make them go in circuit in the same direction or alternate them in opposite directions ; I could test the effect of those different arrangements one against the other.

1739. (*Mr. Varley.*) What was about the speed that you obtained with the six times 166 miles in one continuous circuit with the battery power ?—A retardation of about 2½ seconds.

1740. What was the relative speed of the different battery power used compared with the magneto current ?—The magneto currents was usually from 2½ to 3 times as rapid as the voltaic.

1741. If I understood you rightly you stated that you found, after you got up to 20 cells, that increasing the number of cells did not alter the speed of the current ?—Not perceptably.

1742. Did it accelerate it at all ?—The 20 cells were sufficient to make the distant relay respond readily with moderately careful adjustment, and it required no subsequent adjustment at all. The experiments were perfectly satisfactory, and the velocity seemed to be the same from 20 cells to 360 ; it is true that I was working then with the sand battery, which has so much resistance ; although you multiply the cells you do not increase the current in the same ratio.

1743. I should have anticipated with 300 cells you ought to have noticed a retardation of the speed ?—I did not in any instance observe an increased retardation as the result of increased battery force. The slips are all marked with the memoranda taken at the time, showing exactly what was used.

1744. Do you think increasing the surface is objectionable ?—I do not ; but I think there may be a limit beyond which if you increase it you fail to attain any proportionate results.

1745. Supposing you increase the surface to an infinity of plates whose surface is a mile square ; do you think you would do any harm by decreasing your speed if you had proper insulation ?—I do not understand the meaning of the question.

1746. Supposing you increase the surface infinitely great, merely keeping the same number for tension, do you think you would cause retardation or weakening of the signals through the cable ?—I do not think so ; the general results of my experiments have gone

in the opposite direction; whenever I have used induction coils with a larger secondary wire than before I have got a more rapid current in every instance, when I have not sacrificed the adequate amount of intensity. You must have a certain amount of intensity to overcome the resistance of the circuit, and if you go beyond that amount of intensity you retard your current; in the first few miles you get a more rapid current, and in the last part of a long line it exhausts itself much more rapidly, the retardation being under those circumstances nearly as the square of the distance.

1747. You are alluding now to small-sized plates?—In this instance I was speaking rather of the induction coil current, because I could not vary the size of the plates adequately in the voltaic battery; but I was able to avail myself of coil currents of various characters, depending upon the sectional area and length of the secondary wire employed in the coil.

1748. In the sand batteries, not when you use the resistance battery?—The results were very remarkable, when I came to use the induction coil current, the secondary wire of which was 40 gauge, the early part of the journey was very rapidly travelled, while the latter part was even at a slower rate than the sand battery. I am speaking of the rate of retardation of induction coil currents; I was going to say of different sectional areas, but having been generated in a secondary wire of a different size, I found that the current generated in a 40 gauge wire travelled very rapidly the early part of its journey, the first 100 or 150 miles, but after it had travelled 1,000 or 1,200 miles, it charged the wire even more slowly than the ordinary voltaic current that I had been using, whereas a current generated in a wire of larger gauge, 28 gauge for the secondary wire for instance, would traverse the cable with equal rapidity for a much greater distance, and did not get exhausted so quickly; in fact, I have found it capable of charging a much greater amount of surface. I was, therefore, led to have the secondary wire for the Atlantic coils unusually large, as you probably know.

1749. What was the size of the wires in those secondary coils?—18 gauge in the secondary.

1750. And in the primary?—The primary wire upon each pair of coils consisted of forty-eight 14 gauge wires, arranged in parallel circuits as one large wire.

1751. Of what length?—About 100 yards each.

1752. (*Chairman.*) I believe you subsequently experimented upon the gutta-percha covered subterranean wires of the Magnetic Company?—I did; we had but very few experiments upon them, and they were not very satisfactory ones. When the wires of a company are in constant use commercially, and the stations are at a distance from each other, it is difficult to get a satisfactory experiment; you cannot be certain, not being at both ends, that the arrangements at the other end are what you wish. Sometimes an earth wire is left in imperfect connexion with the circuit, which defeats your object entirely.

1753. You formed a circuit of nearly 2,000 miles, did not you?—Yes.

1754. Did you arrive at similar results generally with those in the other case?—We were not able so carefully to test the velocity. We had a galvanometer put in at each junction, so that we could trace the progress of the wave from end to end; we were able then by a relay put in circuit at our end, to record through the whole length of the 2,000 miles.

1755. Do you consider that such a circuit represents faithfully a submarine circuit?—Not rigidly; it represents the nearest approach that we could get to it. Wires buried in the earth are subject to induction of the same character as those submerged in the ocean, though not to the same degree; it was very difficult without comparison to tell exactly how much less the inductive influence would be felt in those wires.

1756. Were those experiments made with battery power, or magneto-electric currents?—Magnetic electricity derived from induction coils.

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1757. (*Mr. Saward.*) Generated by Smee's batteries?—Yes; and also one of Groves'.

1758. (*Chairman.*) What were the induction coils?—They were induction coils of my own; the peculiarity of them consisted in the secondary wire being placed next to the iron instead of outside, and the primary over that, as I had found by actual experiment that that arrangement will give you a much greater quantity of electricity with less intensity, and that is what I wished to get.

1759. (*Mr. Varley.*) With regard to under-ground wires you stated that you thought when they were joined up at the distant end you were not sure that your request had always been complied with, and that you had the wires joined up as you desired them?—It may have been so; indeed I know by testing that the wires had been joined up as desired, but as I was not on the spot to see that every arrangement was perfect, and that the conditions were carefully maintained during the experiment, it would have been more satisfactory to me had it been repeated with greater care.

1760. Would not your galvanometer show whether they were right or not?—Yes, to a certain extent; but I could not be sure whether something had not happened during our experiment at the other end diminishing our circuit.

1761. The earth would be a total one or a known wire in the office unless the office was extremely slovenly, or a wire being connected with one of the terminals, or else the insulation, the testing box, whatever they used at the other end must have been imperfect?—There are other things that might have occurred, there might have been some imperfect insulation between the wires that I know nothing about, it might never have been detected in the ordinary working of their instruments; it would require extreme care and repeated testings to ascertain it, but for such an experiment as this, one ought to have been at both ends; I ought to have gone to the other end to have tested everything there first.

1762. Have you reason to suppose that any such fault existed?—I was not perfectly satisfied with the result, it was too brilliantly successful in point of speed; in truth the experiment was hastily made, it lasted a very short time, and I felt the necessity of repeating it again and again before we relied upon the result; so far as a mere preliminary experiment goes, it was satisfactory.

1763. Can you give any idea of the speed of transmission of the current through that line of 2,000 miles; I mean the length of time each current took; the speed of transmitting words?—I cannot tell you the speed of transmitting words; the induction coils were made to reverse the current alternately; the relay at the distant end was made to reverse the contact, it was merely enough to work a Morse instrument, so that every reversal of current gave us a dot.

1764. (*Mr. Saward.*) What was the form of signals used on that occasion?—Mere dots.

1765. How did you get your time?—It was counted by the minute; you could make so many dots in a minute. We got a large number of dots in a minute.

1766. (*Mr. Varley.*) Did you try different lengths?—Yes.

1767. Will you be good enough to give us those results?—We had only two evenings to try the experiments on those wires, and on neither evening had we all the time I could have wished. It was very late before we could get possession of the wires.

1768. You did not transmit any words through the length of 2,000 miles?—No; we did not attempt it; we had not arranged any code for transmitting. We used simply a reversing the key to work the induction coils.

1769. Were the induction coils that you used upon that occasion similar in construction (I do not mean

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as to the size of the wire) to those subsequently used for working the Atlantic cable, or were they simple Rhumhorff coils?—They were similar in construction to those used for the Atlantic, but smaller in size, and with solid iron bars instead of cylinders.

1770. Were those solid bars joined together in a continuous iron loop rope?—Yes, at each end.

1771. (*Mr. Saward.*) How many waves of electricity did you succeed in getting through per minute?—We got two, and two and a half a second; but I was not quite certain whether, during the whole of that experiment, the last 400 mile loop of the circuit was perfect. I do not think it was. I think there was during the most apparently successful part of the experiment from some cause, lateral contact, producing short circuit between the ends of that loop.

1772. (*Mr. Varley.*) That was two and a half reversals, which if converted into dots, would have been one and a quarter dots?—If converted into dots in your usual way. If you require a positive and a negative to make a dot, it would have been a dot and a quarter; but on that occasion we made every reversal print a dot.

1773. Did you attempt dots and dashes?—In truth the instrument would not give dashes; it was merely to record the number of currents. It would not have been able to give dashes; it was not so arranged.

1774. Did you at a subsequent period try experiments over shorter lengths of subterreanean or submarine wires?—I tried every length I could get of submarine wires down to four or five miles, as the Atlantic cable was made from time to time.

1775. Were not some experiments tried between Dublin and London with regard to printing?—I spoke on another occasion direct from London to Dublin.

1776. Did you transmit dots, letters, or words?—It was a dial instrument, and every reversal of the current threw it one step forward; we did not try to signal words. I had only about ten minutes or a quarter of an hour, when the time came to put the instrument in circuit.

1777. You merely sent reversals through?—We sent alternately currents received upon a relay to actuate a step-by-step dial instrument. The total length of circuit was 660 miles, all subterranean or submarine.

1778. At what speed did you get your reversals through it?—I can hardly tell you; I have no accurate account. I am quite sure that we got them through as quickly as one in a second; that was the longest length of subterranean or submarine wire at that time laid down.

1779. Do not you think there would be a great deal of difference, between the simple transmission of reversals at equal periods, and the transmission of signals, requiring unequal times; for instance, such as would be required for the transmission of dots, dashes, and spaces in the ordinary Morse Alphabet?—There is the greatest difference the moment you begin to make your signals irregular, either in duration of time or strength of current; you introduce another element; you get your wire discharging itself unequally, and you are very apt indeed to lose a part of the signal; it requires very careful adjustment, and you are obliged to compensate, as it were, for the difference that time has introduced; you send into the wire, currents equal in amount; you send a second current, and if you give them an unequal time to discharge themselves, you are obliged to introduce some system of compensation, in order that the currents may arrive at the other end in equal strength, without interfering with one another.

1780. Do you think the simple reversal of currents, and the formation of dots at the distant end would give any clue, as to the speed of transmission that may be expected through a line of 2000 miles, similar to the one you experimented upon?—Yes, practically the very best clue you can get, but you must make proper allowance. I marked that carefully as to what allowance we had to make, and I found where I could get through a certain number of dots, I could not get through more than a certain number of the usual concerted signals at varying intervals. Having found the number of equal reversals I could get through in equal time, I could deduce from that, in what time I could work off the alphabet; there was about 12 per cent. difference; for instance, if I could get through 100 equal signals at equal times, I must reduce the speed of the instrument to 88, when I came to work the alphabet. I do not think there was a greater difference than that; but it was when the currents were compensated in the way I speak of, which requires extremely careful arrangement. In the Atlantic instruments there was arranged a system of compensation, which would take off from any given signal just so much as would allow for the lapse of time.

1781. (*Chairman.*) Did you find the results at which you arrived in your experiments upon those subterranean and submarine wires corroborated by the experiments which you subsequently made upon the Atlantic cable?—Most perfectly; we got 120 signals through from Newfoundland by these induction coils in a minute.

1782. Were those signals or words?—Simply reversals; we never got words through at that rate; those were the quickest signals that we ever had.

1783. Those were merely reversals?—Yes; that was their way of calling us; on some occasions we could see that they were trying their instruments, and it gave us the opportunity of adjusting ours, when they came to send words they did not attempt to send them as fast as that; I always explained to the clerks that it required a lower speed, and they knew exactly how much slower to work.

1784. (*Mr. Varley.*) When you say that you had 120 reversals a minute, do you mean 60 positive and 60 negative?—Yes, the highest speed of words we ever got actually was two words and three quarters per minute. I have an exact record of it, I think it is 41 words in fifteen minutes, it was a message occupying 15 minutes, which came through at the rate of two words and three quarters a minute, with spaces between the letters, and double spaces between the words.

1785. The usual allowance of spaces?—Yes.

1786. Do you think that speed could be maintained throughout the day without interruption?—They could have worked faster than that—the currents came faster than that—with ease, for the instruments when they got 120 reversals in a minute could have sent four words easily by our arrangement with the antecedent compensation that I introduced. The mode adopted was, the induction coils gave us at every reversal of the primary current, of course, an equal amount of secondary current; but when they were to make a dash, this, occupying time, required that there should be the compensation. I had so arranged that the key to the instrument making the dash produced the compensation exactly to the amount that was required; we could arrange or adjust it so that there should be either more or less compensation. I could see on the mirror galvanometer distinctly on one occasion that they had overdone their compensation in Newfoundland, and I sent them word.

1787. Was the over compensation by putting resistance into the circuit or cutting out some wire?—No, cutting out or neutralizing too much of the current. The truth is, if you send equal currents into an empty wire, or a wire which is charged with the previous current, they will travel at different speeds; the new current which is to charge the wire will, if it go into an empty wire, tell its tale more quickly, it would tread upon the heels of the previous signal in working too quickly, and wipe out a part of the previous signal. To prevent that, I had to send in a small fraction of the current preceding, and I had to make antecedent compensation in that way, because I found that they travelled at different velocities. These facts came out very curiously in some experiments I was making at Keyham, and they were borne out when the line was laid across the Atlantic. Currents of equal strength from the same apparatus

will travel at different speeds, in accordance with the antecedent state of the wire, so that the mere varying lapse of time alters the rate of travelling of different signals.

1788. Suppose the wire be charged with positive electricity, and you are sending a positive signal after that, will that increase or decrease the speed?—I never did that; the construction of these instruments was such that it must always be a negative current to go next.

1789. Would the signal go quicker when the wire was empty or when it was partly negative?—Quicker when empty. In fact, it was to retard the current and prevent it taking off the end of the dash, that I designed the antecedent compensation on purpose to remedy it, and put it into its place again.

1790. Do you think that system of compensation could be depended upon for that purpose?—Yes, most perfectly; and when once arranged, it is the easiest thing in the world. It is a mere piece of mechanical clock work, which acts automically the instant you put down the key, and releases itself at the proper time.

1791. (*Chairman.*) It is entirely self-acting?—Entirely, without that arrangement our limitation of speed is much greater.

1792. (*Mr. Saward.*) Is it attached to the working key?—Yes; it is in fact a part of the key; the difference between simple reversals and working signals is much greater than 12 per cent. unless you can compensate in that way. With great accuracy of compensation you make the working speed nearly approach to the most rapid reversals, so that when we got 120 reversals through in a minute, we could have signalled words at high speed if very accurately compensated in that way; but without such compensation there would have been a reduction of speed to the extent of probably upwards of 40 per cent. required to signal words.

1793. (*Mr. Varley.*) Did you allow two currents for a dash?—The same time as two currents; we send a positive, and allow a lapse of time for what would have been the next negative.

1794. It was occupying the interval of three currents with one positive and one negative?—Yes.

1795. Did I understand you rightly to say when you were sending a succession of dots, take the letter V, which is composed of three dots and a dash, that the wire is empty after making the first dot?—No, because the blank and space following that dot has been produced by a current.

1796. It is a longer blank than usual, as if you were beginning a new word?—We will assume that all the currents are short circuited for four or five seconds before commencing a despatch. If then the first signal required were a dot, it is necessary that in this empty state of the wire it should be compensated antecedently, else it would be converted into a dash by marking too soon.

1797. On producing your first dot you send in a current which produces its mark, and then you send in after that dot, a short portion of the opposite current?—An equal amount exactly, in order to terminate the dot at the proper time.

1798. An equal amount, and a little more, do not you?—We send in the alternate currents as they come from the induction coils to make the dots; when you come to the difference of time between the dot and the dash, you must compensate for them.

1799. You do not compensate for the spaces?—We compensate for them by the key exactly as we do for the dash, while the dots produce themselves; you have two keys at work; each key is held with the mechanism exactly the right length of time; you could not put it down before the proper time, at the time when the change took place the key was released; you could only raise it again at the proper time; there was the same mechanism for the dash key, and the blank key. If you printed the end of a word, you kept the blank key down during two intervals, the letter-space being one interval; the word-space two intervals which would have occupied the time of four currents; it is an extremely simple arrangement.

1800. In that way you got two and three-quarter words in a minute?—Yes, and we could have easily sent a good deal more; there was plenty of strength in the signals.

1801. With the battery what was the speed through that cable?—I am almost afraid to say; we had six, eight, and ten seconds for contact with Daniell's battery, before they could receive the shortest signal.

1802. What Daniell's battery was that?—We began with 480 pairs, and then I reduced them, arranging them for quantity, so that we worked from 25 to 50 cells, but never more.

1803. With 50 cells of what surface?—These batteries of Professor Thompson's were about 12 by 9, or something of that sort.

1804. Did you get the dots through in nine seconds?—It required 20 seconds to make a single dot, 10 seconds positive current to indicate the dot and 10 seconds negative current to discharge the wire.

1805. For a dash could you work through it?—It was worked; the Queen's letter went through with the Daniell battery; we did not send a single message from this side with the induction battery; whereas every message from the other side was sent by the induction coils except these four words, "Daniell's now in circuit." Those were the only words that came from Newfoundland, except by my own induction coils and apparatus.

1806. Have you any idea of the speed of a message through, with 50 pairs of Daniell's battery, the number of words in a given space of time?—I doubt if at any time they exceeded the speed of one word in a minute; there were many stoppages in it, and many parts required repeating; they read off by half a degree of deflection upon their horizontal galvanometer.

1807. (*Mr. Saward.*) On the Newfoundland side?—On our side they were almost all read upon Thompson's mirror galvanometer. Some were received both by relay and by galvanometer, some came by relay alone; for the greater part of the time afterwards I kept Thompson's in circuit, and threw the relay out of circuit, simply because the other was more convenient and less liable to derangement in its action from the terrestrial currents. I was extremely pleased with it, and I printed some messages with my own hand from Thompson's galvanometer; some of them were recorded by relay with perfect accuracy; there were several recorded by relay and by Thompson's galvanometer on the same slip, so that there can be a strict comparison, and the relay in one or two instances is quite more accurate than the printing from Thompson's instrument. (*Several original message slips handed in illustrating these points.*)

1808. (*Chairman.*) Can you give any idea approximately, of the velocity of the messages received from Daniell's battery?—It was with very great difficulty that we could get anything through, as the signals from this end did not produce a deflection at Newfoundland of more than half a degree; all that was sent from this side we were obliged to work very slowly; if they had worked from that side with a Daniell's battery to us, we should have a fair comparison; it was not fair to compare the working from the lame side of the cable with the working from the perfect side; we did not get anything like half a word a minute through to them at the best time during my stay at Valentia.

1809. (*Mr. Varley.*) Do you think a word might have been got through in four minutes with Daniell's battery with such a cable as that if it were perfect?—Yes; a word in four minutes could have been easily got if the cable had not been injured. It ought to be done unquestionably.

1810. With regard to the cable at Keyham, did you try any experiments with Daniell's battery there before it went into the sea?—Yes. The Daniell's

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battery was fitted up and used on board the "Agamemnon."

1811. Did you record any speeds there ?—I tried the velocity. I did not try to record words.

1812. What was the difference of speed that was noticed by the induction coil in the whole Atlantic cable at Keyham, and with Daniell's battery ?—Daniell's battery occupied considerably more than double the time of the induction coil; it never was less than five seconds to get a signal adequately.

1813. And the other ?—A second and three quarters. I tried it with extreme care; I made a great many experiments, taking various lengths. I took the very first movement produced by the Daniell battery on a large sensitive galvanometer.

1814. It was about as one to three ?—Yes.

1815. (*Chairman.*) Was it the experiment which you made upon the Magnetic Company's wires which induced you to believe that a telegraph could be successfully laid across the Atlantic ?—Yes. It was those experiments which conclusively satisfied me by confirming the results of all my previous researches. I can hardly say that I felt satisfied of it beforehand, because that was the first opportunity I had of experimenting on a large scale with wires actually laid down. I relied greatly upon the results obtained in working from London to Dublin 660 miles in a single length, not having the ends brought back again. It is far more satisfactory to work through an actual space than a virtual space, by the wires being brought back again.

1816. What results did you find in these experiments ?—Though I was only ten minutes at work, I got signals through rapidly and stronger than I expected. I had sent my assistant to work the instruments at Dublin, and he received my signals at Dublin while I received his in London.

1817. You were one of the early promoters of the Atlantic Telegraph, I believe ?—Yes, in conjunction with Sir Charles Bright and Mr. Cyrus Field.

1818. You were concerned in designing the Atlantic Telegraph cable, I believe ?—I was very little concerned in designing it; I must disown a good deal of that; I had nothing to do with the designing the outside iron wire at all; I sent the committee a specimen of cable which I thought would have answered the purpose better.

1819. (*Mr. Saward.*) Did not you approve of the electrical conditions of the cable ?—I am responsible for it up to a certain point, but before I gave an opinion at all as to what would be the best kind of insulated conductor, I said, "I want three months for experiment, I do not think that such an important subject "ought to be decided off-hand. I do not say that I "withhold my opinion, but," I said, "I do not think it "would be fair to ask my opinion suddenly, in that "way."

1820. Did not you think that a small conductor offered the best chance of overcoming retardation ?—That is saying rather too much; a large conductor of the size supposed necessary to fulfil the theoretical requirements of the case was deemed impossible of manufacture, and I was asked pointedly this question, "Do you think it can be done with a conductor "of the usual size ?" I said, "Yes, I am certain it "is possible." The result has proved that I was correct, but I did not select the size of the conductor; it was rather selected on commercial grounds.

1821. You would have preferred a smaller conductor ?—Larger, but not nearly up to the supposed theoretical requirements, for I had satisfied myself that the law which regulates the size of conductors in over-ground circuits was utterly untenable in rigid application to submarine wires, as proposed by me; and I still think so from experiments I have recently made, by which I can show the effect of surface induction alone in almost annihilating the current as compared with the effect of resistance on the same current.

1822. What were those experiments that you have made recently ?—Some important ones: taking the magneto-electro currents that I purpose to work with, testing their force through a certain amount of resistance with Weber's Dynamometer, or an instrument made on that principle, I got 20° of deflection with a certain amount of force, the needle generating the current revolving at a certain speed. Then, without altering the resistance at all, I increased the surface by induction plates and I lose nineteen-twentieths of the whole current by the mere effect of the surface.

1823. Can you explain how you increase the surface ?—By plates of tinfoil insulated by gutta-percha, representing a surface equal to about five miles of the Atlantic cable. I found that in a surface of five miles of the Atlantic cable you can show the absorption of nineteen-twentieths of the entire current which was previously traversing through a resistance equal to about 120 miles of the Atlantic cable, showing that the surface is of immense value in consuming the current. This effect is observable only when the currents are alternately in opposite directions.

1824. Will you describe the apparatus you adopted for those experiments?—It is very simple. A magnetic needle is made to revolve rapidly within a helix. The needle and helix are therefore the generator of currents, which are equal in force and of opposite polarities alternately, positive and negative: although the currents are in opposite directions, yet the deflection produced upon the dynamometer is always in the same direction. With the revolution kept up at a certain speed you command uniformly a certain deflection; maintaining that speed you know that you have got an equal amount of current that will stand for any length of time, like a constant battery. I find that with a given deflection, if you apply to any part of the circuit these induction plates, the induction between the two surfaces is so great as to withdraw from the dynamometer nineteen-twentieths of the whole current, which remains upon the surface of those plates, and is there consumed by the next current, each current in succession consuming its predecessor to the extent I have mentioned.

1825. (*Mr. Varley.*) What was the nature of the apparatus ?—A needle and helix; a needle about 3 inches in length, and a helix just large enough to take it round with a 36 gauge wire.

1826. What was the length of the electric generator? —Very small indeed; it is about half a mile of 40 gauge wire.

1827. That would be equal to how much of the cable tested ?—I hope I have explained myself correctly as to the principle of this apparatus. Perhaps the diagrams which I now present will aid my description. They represent the two modes in which I arranged the circuit. There are two ways in which I tried the experiments, and in both the result came out accurately. The first was passing the current before it came to the dynamometer, through a series of induction plates; it passed through one set of plates on one side of the helix, and another set on the other. It had, therefore, to pass through a condenser, so called, before it got to the dynamometer. In that way, nineteen-twentieths of the current were consumed. It was more remarkable afterwards, in the arrangement represented by the second diagram, when I took the circuit from this generating helix direct to a dynamometer, and arranged the wire so that you could only throw it into the condenser as a sort of *cul de sac* outside the circuit. When I did so it still went in the same large proportion into the condenser, although the condenser was not a part of the circuit, and only, so to speak, attached.

1828. How did you apply the dynamometer ? every current passing round the dynamometer in only one direction ?—No; the peculiarity of Weber's dynamometer is that though the currents be passed in opposite directions, yet they produced a deflexion in the same direction. Of course, you change the polarity of both parts at once; therefore, the deflexion remained in the same direction. The remarkable thing to my mind was that though there was perfect continuity in the circuit, from the generator through

the dynamometer, yet the current preferred to go into the condenser to the extent of nineteen-twentieths, just as if the condenser had been made an actual part of the circuit. Thus it became evident that surface induction takes precedence of conduction, and exerts a far greater influence on the results than has generally been supposed.

1829. You said that the tinfoil represented 5 miles of the Atlantic cable?—Taking the mean of the internal and external surfaces. There were two sheets of the thinnest gutta-percha between the sheet of tinfoil, so that there is more inductive force; but the surface charged is only that.

1830. It would be something like thirty yards square, would it not, to be a mile of surface?—The outer surface is a square inch to the running inch of the cable, as near as possible; 144 inches in length give you a square foot as nearly as possible.

1831. You do not think that increasing the size of the conducting wire will materially increase the speed of the transmission of signals?—I am quite sure that beyond certain limits, which I cannot yet define, the opposite will be the case, unless the quantity of electricity employed be increased in at least an equal ratio.

1832. A statement was made in a printed publication, shortly after the Atlantic Telegraph was established, to the effect that when the size of the conductor was increased, the speed would actually be diminished?—I think I can tell you exactly how that statement arose. Having the cable with six conducting wires in it, I was able to use the whole of the six conductors as six parallel conductors, increasing the area six-fold, and yet increasing its inductive surface at the same time six-fold. I was conscious that it was not like increasing the sectional area of a single wire six-fold, but increasing its surface only in the proper proportion; yet the results were so remarkable that I could not but be struck by them.

1833. (*Chairman.*) That produced the retardation?—In every instance, when I used two wires instead of one, the retardation was greater; when I used four it was further increased; and when I used six it was greater still. It is true that allowance must be made for the increase of surface being greater here than the fair proportion due to mere increase of sectional area would be; but the obvious increase of retardation was such that it at once arrested my attention. I had expected, as others did, and as some still do confidently, that by increasing the sectional area I should have got a higher speed; at all events, if it had not resulted in increased speed, I expected that by increasing the conducting power I should have got my signals heavier and stronger. I got them weaker and slower, in opposition to all one would have noticed in overground wires. I got literally less current, and, of course, because more remained on the surface.

1834. From that experiment you think that increasing the size of the wire would not materially increase the speed?—I do not know from what sized wire you propose to start as a minimum, nor am I prepared to say how far you ought to go in that direction; there are certain limits which would put a stop to it.

1835. Supposing the Atlantic cable was made with a conductor a foot in diameter?—First, I believe it to be practically impossible to insulate a submarine conductor of such magnitude; and secondly, if made, I believe that such a cable would consume, by its enormous surface for induction, any amount of current which could be thrown into it from an ordinary battery during rapid signalling. Clearly it would convey a continuous current with a facility strictly proportional to its sectional curve and conductivity, but for rapid signalling I think it would prove a failure.

1836. For theoretical consideration do you think you would increase the speed materially by such an increase of the conductor?—I think you would have to increase your battery power to a fearful extent, in order to allow for the great loss produced by surface induction.

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1837. In intensity?—Chiefly, but not solely in quantity; unless you did that you would have an absolute loss of working speed.

1838. You have stated that you had requested to be allowed to make certain experiments before you should be asked to pronounce upon the core of the Atlantic telegraph; will you explain the circumstances which prevented those experiments being tried?—Chiefly the lapse of time; I was then speaking to Mr. Cyrus Field, who was the most active man in the enterprize, and he had so much steam that he could not wait so long as three months, he said, "Pooh, nonsense, why the whole thing will be "stopped, the scheme will be put back a twelve-"month, cannot you say now that you know that "will do;" I said, "But I cannot at all tell you "that that is the best; I think that we are bound "to find out what is the best before we go into it." It was then determined that if I took the three months it would lose the year; he then pressed me about it and he said, "We hope that you are not "going to stop the ship in this way," I had pretty nearly determined that I would not give an opinion, but he extorted the opinion that I was quite willing to say "that it could be done with that," but I was determined to withhold anything like an official opinion till I had made the experiments; I had got the permission of two companies who had under-ground wires of different sizes running between the same towns to experiment during the night, and I could have seen the different ratio of retardation upon those two lengths of wire in similar circumstances, when I should be better qualified to give an opinion.

1839. Did you test the core of the Atlantic telegraph?—Yes, the whole of it.

1840. When it was completed?—During its progress, as it was made in portions.

1841. (*Mr. Varley.*) It was not tested under water at all, I believe?—The core was always tested under water.

1842. After it was covered with iron?—No, there was no opportunity afforded of testing the cable after it was made under water.

1843. (*Chairman.*) Did you test it at Messrs. Glass and Elliott's works and at Messrs. Newall's?—Yes, constantly at both places.

1844. In your opinion at the date when it was first completed and coiled on board the Agamemnon in 1857 the cable was electrically perfect at that time?—I should hardly like to say that it was electrically perfect; I believe it was as perfect as we could get it, under the pressure of the then existing circumstances.

1845. Was it electrically perfect when it left the gutta-percha works?—Quite so, although there were variations noticed under our extreme tests, yet it was as perfect as could be made, and far more perfect than any that had been previously made.

1846. (*Mr. Varley.*) You think that the Atlantic cable underwent some deterioration in the course of manufacture?—No question about it.

1847. (*Chairman.*) In the manufacture of the outer covering?— Yes, chiefly from exposure to heat after it was made; but other sources of injury existed. I will point out one that it was almost impossible to avoid; during the process of manufacture we found a break of continuity—a most serious thing; after it had been covered and spun into cable, and the cable was coiled in a tank: we thought this was impossible, and we had done our best to render it impossible by using a strand of seven wires, instead of one solid wire; but on examination we found that there had been a false soldered joint made, not one made in the usual way.

1848. Was that in the copper?—Yes; and it was cemented over. It had been most carelessly done.

1849. (*Mr. Saward.*) Did you trace the defect to the person who made it?—I showed it to the Gutta-percha Company immediately, and for their sakes I

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said no more about it. They made the most searching inquiry, adopted the most rigorous precautions, and made it impossible that anything of the sort should take place again.

1850. (*Chairman.*) What control had they over it?—The core was covered in short lengths, and put together with extreme care; but, in spite of all their watchfulness, this occurred, and the copper wire was soldered by some careless person.

1851. (*Mr. Saward.*) Do you imagine that that joint was made at the gutta-percha works?—It was a false joint; a thing made to save trouble after the first covering with gutta-percha, and the lazy rascal just patched it together before it went through the second covering, there was no real joint made. The gutta-percha went over it, and *then it* went through a second covering, and it was not discovered till it gave out in this way in the tank. I said, "If any careless "rascal has done that once, he may have done it again; "what are we to do?" There was a length of several hundred miles lying at the gutta-percha works already completed when I detected this flaw. I said, "I have no confidence in that, unless it is tested?" They said, "How is it to be tested?" It was evident that it was no use testing it by the ordinary test for continuity, and I desired it to be tested with a strain, so as to draw these ends asunder. I said, "We must "put the whole of this under just strain enough to "detect if there is a breach of continuity."

1852. (*Chairman.*) Was it done?—Yes.

1853. (*Mr. Saward.*) What was the strain put upon it?—I tried a great many experiments to determine how much strain would show these bad joints without producing the least permanent stretch; of course we could not produce any permanent stretch without injury. I tried to determine by the friction-brake, or some arrangement of that sort, how much strain could be put on to detect any break of continuity that occurred.

1854. Did you detect any other flaws?—Yes, we had a second.

1855. (*Mr. Varley.*) Do you remember how much strain you put upon it?—It was estimated at about 40 pounds. Yet, although you know how many pounds weight you put upon the friction-brake, but you cannot always tell what strain that gives; there is a source of accident you could not determine, and could not avoid. During the process of winding under strain (it could not be wound by hand, it was wound by steam) there was a large drum and a swift, from which the core was to run, when the drum began to be filled with the core, the circumference was small, and the winding took place slowly, and the core came easily off the swift; but as the drum filled, and its circumference got larger, each revolution then took off nearly three times as much from the swift as it would before; as you came to the small part of the swift, the velocity of rotation increased rapidly, and the speed of the swift was about as much as four times what it was at the beginning, so that the friction varied; we had to stop sometimes because the swift would be overrun, and there would be jerks, and the inertia then would give the core a stretch, so that it was subjected to a trial that really I should be very sorry to subject a cable to again.

1856. (*Mr. Saward.*) You think that subsequent experience would lead to very great improvement in respect to the core?—Yes, I do.

1857. (*Chairman.*) You detected most of these defects, did not you?—Yes. In some instances, however, we could not ascertain when the stretching of the copper wire had taken place, and the contracting power of the gutta-percha was put upon it; as it could not retract suddenly, it would retract gradually. I have ascertained since, that gutta-percha is to a certain degree plastic, even at a low temperature, and that is, to my mind, one of the most serious matters connected with the cable; at the same time, if any such point had escaped, it would ultimately force its way through. I have learned that you cannot be careful enough in winding or rewinding, or handling insulated conductors. If you get the copper conductor stretched beyond what the outside will admit, you are certain to have it find its way, sooner or later, to the outside. The other source of injury to which the cable was exposed, after it left the gutta-percha works, was the heat at Greenwich; the hottest day that we have had for 11 years occurred, while that cable was left exposed to the sun during the whole of a Sunday.

1858. Not placed in tanks?—In dry tanks; if it had been sheltered properly the heat would not have affected it; but the gutta-percha was seen on that day oozing out in drops between the iron wires which it was covered with. I have given the sources of accident before it was covered, but this, which was the most serious of all, occurred afterwards.

1859. From the heat to which it was exposed before it was put on board ship in the first instance?—Yes; it was actually in process of being coiled on board the Agamemnon when this inquiry from heat occurred.

1860. (*Mr. Varley.*) At Birkenhead was not it put under water?—Neither at Greenwich nor at Birkenhead was it submerged. In fact the cable was supposed at that time to be so tender that they almost thought it ought to be shut up in a glass case to keep it from rusting; the wire was so thin that there was a great anxiety to keep it from damp or water; the least rust, even *half* an inch of rust on the cable, it was believed, would have necessarily caused it to have parted at that spot, so that it was most carefully protected from wet.

1861. (*Chairman.*) Did you accompany the first expedition for laying the cable?—No; I was at one end, at Valentia, keeping up the communication. I did not go out in either the Niagara or the Agamemnon.

1862. You tested the cable after it returned from its unsuccessful voyage in 1857, did not you?—Yes; I should like to deal a little further with the injury at Greenwich, which is, perhaps, the most serious matter of the whole; if it had been tested under water after that the injury would have been detected, I feel certain. It was an intensely hot day; the hottest, Mr. Glasier at the Observatory told me, we had had for 11 years; they had sent for me early in the day to say that there was something they could not understand about the cable. I found on examining the whole of the upper layer that in parts the gutta-percha was, so to speak, exuding or sweating out in large drops, the size of a pea; it was gutta-percha evidently, softened by heat, and blackened with the tar.

1863. Was not there a serving of hemp outside?—Yes; but the gutta-percha had forced its way through the hemp and between the iron wires; it was visible upon the outside. I was in the greatest consternation at this, because they were actually paying it on board the Agamemnon; they had paid some two to three miles before they noticed this and sent for me; some had gone on board the Agamemnon that had been burnt or exposed to the sun in this way. Of course it was all most rigidly examined, and I took many pieces out, in order to see whether the internal structure was affected. Of course, if the gutta-percha had found its way to the outer surface, it was minus below. I did not know to what extent the injury had gone, whether the gutta-percha had come from the surface of the core merely, or whether, in fact, the entire core had become plastic inside; and I cut a great many pieces to examine it; every piece that I examined showed that the conductor remained central; therefore I said, these drops are, so to speak, a film that has been drawn from the outer side of the gutta-percha; the inner part next to the wire has not been rendered fluid. I thought that the thermal conducting power of the iron had drawn these drops equally from every part of the surface; all the worst pieces that I could find I cut out; in every one the conductor remained central, so that my mind was satisfied that although injury had occurred, it was not so bad as it might have been. I could not see anywhere at that time that the cable was so seriously

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injured as to stop the expedition, because all that was burnt in this way I cut out, and all that was the least suspicious, as far as I could; and I set aside between 40 and 50 miles on that occasion; wherever I could I examined it, and I found the conductor remaining central; that reassured me, however. Since the return of the last expedition and the failure of the cable, I have found other parts of the cable, where the gutta-percha had become plastic, and the conductor, instead of remaining central, had fallen down or been forced laterally through the gutta-percha, and the conductor was in contact with the tarred yarn (*specimen handed in*).

1863 a. (*Mr. Varley.*) Do you mean after the cable was put on board?—The injury was evidently in part at least due to the repeated handling in, being put on board and taken out twice.

1864. (*Mr. Saward.*) Was not there any precaution against the effects of heat?—A shed had been ordered, but it was not built; the estimates were, I believe, prepared: the only doubt was whether there should be something like a tent hired or a shed built over it. I know the directors had intended a shed to be made, but it was not done. Then there was extreme difficulty in this respect. In those coils of the cable that were exposed to the sun it was not merely the upper layer that was exposed, but the outside of every layer was exposed to be burnt; and if I had cut out every bit upon which the sun's rays had fallen, we should have had the cable cut up into two mile lengths all through—because no flake contained more than two miles, or two miles and a half—so that if I had cut out of the cable completely those pieces that the sun's rays could have touched, namely, the whole of the top layer, and the outside and the inside of each layer, that would have ruined it.

1865. (*Mr. Varley.*) Did not you notice any great increase of the escape of the current where the wires had got to the outside at any place?—I never did till after the return of the expedition trace it to the outside directly; the gutta-percha itself impregnated the tarred yarn, so that not being tested under water, we did not notice the escape; you know how difficult it is to get accurate testing when the gutta-percha is exposed to heat. In high temperatures the gutta-percha will, instead of being an insulator, become a conductor. This was being done in July and August, and it was impossible to tell how much was attributable to mere temperature, and how much resulted from that sort of accident; I do not believe any kind of research could have told you how much was due to temperature.

1866. (*Mr. Saward.*) Reverting to Captain Galton's question, if I understand your feeling upon the subject, you had some misgivings as to the perfection of the cable before it went to sea the first time?—Distinctly, but it was impossible at that time that they could be set at rest, in consequence of the temperature; that is a very important point; it had been ascertained a year or two before that in hot weather cables could not be manufactured without a large apparent defect in the insulation. On one occasion at Messrs. Glass and Elliot's they thought that some defect had occurred and that the cable had "gone bad;" they flooded it with cold water, and instead of showing that it was worse it got better; the expression was that it was all right again; the lower temperature improved the insulation directly. A similar appearance showed itself at Mr. Newall's works during the manufacture of the Atlantic cable and was reported by me to the directors; he had a steam boiler and an engine in fact where the cable was stored; it was a most difficult thing to detect whether any deterioration in the insulation had taken place, because the temperature was uniformly high; they were working, I believe, at times night and day with the steam boiler as well as the engine in the same room that the cable was stored; a large allowance evidently was to be made for the temperature, but no one knew or could determine how much; and it was impossible to say whether, in a given length, the loss of current arose from an absolute fault in the cable, or from the temperature alone; nothing but plunging it into water could have shown that accurately.

1867. (*Chairman.*) You were not able to do that with the Atlantic cable?—No; at Messrs. Glass and Elliott's it was equally difficult; it varied a good deal, and in one short length of cable the effect of temperature was something remarkable; it showed as much as 64° loss by the detector during the day, and it sometimes went down 2½° at night. The cable that had been heated during the day cooled itself at night; therefore, I saw the utmost effect of the temperature; that piece, about 6 miles in all, only showed 2½° of defect at night.

1868. On board ship was the condition improved?—Little, if indeed at all, improved. It had been coiled on board by gas light, a temporary gas-main having been laid from the factory to the ship. There were 30 or 40 men in the hold, working night and day, and all this was going on in July. There was believed to be some heating in the cable also; at all events the hold was insufferably hot, and it was impossible, unless it had been flooded, to tell whether the defect arose from defective insulation, or was merely the result of temperature.

1869. The temperature in the hold never became improved?—Not to my knowledge.

1870. In fact on board ship you could not even tell that the cable was electrically perfect?—I could not. I never could assure myself of its perfection. There was this question of temperature which nobody had worked out perfectly, and which I had not an opportunity of putting to the test there. Nothing but submergence could have ascertained it; it was like the cable some years ago in Glass and Elliott's tank, which during a hot day they said was bad, and the moment it was flooded they found it was perfect.

1871. (*Professor Wheatstone.*) If you refer to Mr. Glass's evidence, you will see that he says the effect of heat was to make the wire get out of the centre?—Of course it would in a greater or less degree arise in the manner which I have described, if it made the gutta-percha plastic enough. But it would injure the insulation even if the wire remained in the centre. Feeling this difficulty with regard to the effect of temperature on insulation, I made some time previously experiments at the gutta-percha works very carefully indeed. I took two miles of a coil of the very best insulated core they had, which at an average temperature was the most perfect we could select. I plunged it into a large tank, to which we gradually added warm water, and brought it up to 100°, and that coil of insulated core which was quite perfect at an average temperature, was utterly bad at 100°. We cooled it again, and it regained its perfection. It was as good a core as ever was made; it was not warmed enough to injure it permanently; but warmth is quite enough during its continuance to destroy the insulation.

1872. (*Mr. Saward.*) Do you think that warmth would destroy the centricity of the wire?—If the conductor had not been stretched, I do not think it would injure it seriously; it has no natural tendency to get out of the centre.

1873. (*Chairman.*) You had no opportunity, I believe, before the first expedition sailed, of testing the electrical condition of the cable?—I never had an opportunity of testing the finished cable under water at all, till after its final submersion.

1874. When it was coiled at Keyham; after it returned from its first voyage did you test it?—I had spoken strongly of the necessity of its being tested under water, and some large tanks were built on purpose; at least, they were supposed to be built on purpose, caulked, and pitched, with the idea of making them water-tight; but when the cable was put into the tank it would not hold six inches of water; the tank being of wood and built on the ground, when it came to have these 2000 tons weight upon it, it gave in every direction; the joists and timbers were

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strained and the planks gaped, so that you could put your fingers in between them.

1875. It was quite impossible to have tested it under water then?—Yes, quite; the tank ought to have been dug out in the earth.

1876. When the cold weather came, did you test the cable?—I was constantly working through it.

1877. Was its insulation more perfect than when it first arrived?—It was much more perfect; the cold weather did, as I anticipated, improve its condition materially.

1878. When it was placed on board the ships for the second expedition was it imperfect?—Do not let me give a false idea; it was, as far as I could ascertain, absolutely free from "faults"; yet the cable ought to have been tested under water, and nothing but such testing as that would have satisfied me that it was absolutely perfect. I beg to hand in a specimen as part of a length which when tested dry showed no defect, nor indeed until after prolonged immersion in water.

1879. (*Mr. Saward.*) The directors were not aware at that time of the necessity for that testing, were they?—I think so, else why had they spent 3,000*l.* in building tanks for that purpose.

1880. They were not made aware of it, were they?—They had been made aware of it long before, but my opinions had been overruled or forgotten, I suppose.

1881. The directors were not made aware that the cable was in an imperfect state, or assumed to be in an imperfect state?—The directors knew that I had wished to test it under water. This would have removed any doubts or suspicions which had been entertained.

1882. Those suspicions were not communicated to the directors or to myself, were they?—I must say that it would have been very invidious and a very thankless office to throw out suspicion, and shake confidence in the cable when I had no direct evidence that it was imperfect, and when my wishes as to testing under water had been entirely frustrated. We could work through it, and though we knew that there was a great loss upon it, in spite of all that it was as perfect probably as any cable that had ever been made.

1883. (*Chairman.*) What class of tests did you use at Keyham?—The usual modes of testing; we were also working through it constantly with our experimental instruments that were to work through the Atlantic; my chief occupation was preparing these instruments and testing them in every way.

1884. Sometimes it was worked with induction coils and sometimes it was worked with Daniell's battery, was not it?—Professor Thompson worked with Daniell's battery; all the other signals were worked with the induction coils.

1885. (*Mr. Varley.*) Did you not test the cable before the last expedition?—Yes, very frequently.

1886. Did not you notice anything to indicate that there were faults much nearer to one end than to the other?—There were, I am confident, no "faults" which could be detected without submersion. There were some parts that I was uneasy about, but I could not prove anything about them.

1887. When I saw the tests that had been made at Valentia, there were some distinct tests of a piece of 1,000 miles, a piece of 500 miles, and a piece of 200 miles, and the whole cable of 2,500 miles, and those tests all pointed to a fault existing about 500 or 600 miles from one end of the cable?—I know there were some parts not so perfect as others; we could only find that there was a general imperfection spread over a considerable part; there was not at that time, I am quite confident, anything like one definite "fault." I always said that a length of 200 miles, one particular coil, was about our worst length; we tested it in various ways, and every part tested equally; the length in question was divided into four parts, which were each tested from each end; these were again subdivided and similarly tested with great care, no one part showed more loss than another. It was evident, therefore, that that length was as a whole less perfect than the rest, and it was arranged that it should be the last to come out of the ship in paying out, so as probably not to be used at all. The great loss of cable on the occasion of the first expedition caused this length to be almost entirely payed out; about 20 miles only being brought home in the Agamemnon.

1888. (*Chairman.*) You tested the cable immediately after it had been submerged, did not you?—I did.

1889. In what state was it then?—It was most difficult to test; they left their current on at the Newfoundland side for many hours, and during that time we could not test it.

1890. What was the reason for leaving the current on?—To assure us that it was all right. They were not at that time ready either to send or to receive from us.

1891. (*Mr. Saward.*) Was that done for some hours or for some days?—I cannot say at this moment.

1892. Would it be for a few hours or a day or two?—It was almost constantly on with occasional reversals and ship signals till the fourth day. We got a strong deflection by voltaic currents from them until the fourth day, when they began to send currents from the induction coils.

1893. (*Chairman.*) When the currents first came were they strong?—Yes; it was impossible till the reversal came to say that they were not earth currents; when the reversals came then we knew they were keeping up the ship signals; keeping up the current for ten minutes and then at a particular time reversing. When they did not send us the ship signals they sent us this constant current while they were unshipping the instruments and getting them into order.

1894. At first the currents were very good and very strong?—Yes.

1895. How long was it before they began to show symptoms of weakness?—As soon as they began to send these coil currents, on the fourth day, we put a relay in circuit, the relay spoke out loud and there did not seem to be any defect at all.

1896. When was the induction coil put into circuit?—As soon as they took off their battery, and ceased to send the ship signals, they began to send the coil signals.

1897. How long did they send strong currents from the coil signals?—For several hours they did that; they simply sent us alternate currents, and now and then attempting a letter. They had not at that time got their apparatus in proper order; they had taken out the experimental one with the coils, and they just kept us alive as it were, counting these signals. The signals were very strong; they made the relay speak out loud, so that you could hear it across the room.

1898. For how many days did that continue?—When we began to find that the relay required adjustment constantly, owing to the variation of the terrestrial currents, then we got Thompson's instrument also into circuit,—as far as I can remember two or three days, and we still got signals on the relay sufficient to print perfectly for us to read from; but we were perplexed in not being able to speak to them.

1899. Could not they receive any messages from you?—They did not for several days afterwards.

1900. How many days?—I have notes of the first day when we established perfect inter-communication with them.

1901. What day was that?—They continued saying to us we do not receive your signals, we cannot read them, send slower, which showed us evidently that they were not receiving properly from us, and we could not communicate with them; they then desired us, if we understood them, to send a battery current for five minutes; of course, as soon as they had stopped, we sent a battery current for five minutes and then a reversal; they could make no mistake, and we opened a communication in that way. I then, from compari-

son of the results at both ends of the line, felt certain that there was a fault at our end, and that therefore we could not work our induction coils with advantage, and I determined to use the lowest intensity we could work with. I arranged for the signals to be worked with 25 cells; we did work with as many as 50, but never more than 50 by my desire.

1902. What size of induction coils did you use?—Five feet.

1903. Do you consider that the strong currents sent from the induction coils produced any bad effect upon the cable?—Had the cable been sound, it would have been impossible for the coils to produce any injury; but where injury already exists, there any powerful current will augment the mischief. I do not think that they had previously produced any bad effect, or it would have been manifested at Keyham. The same coils had been used there.

1904. (*Mr. Saward.*) Would not there be a difference between testing the cable at Keyham and in the water, on account of its being submerged?—Of course, both in consequence of the penetration of the water under pressure of depth, and also more especially from the fearful strain which the cable had undergone in the process of submersion.

1905. (*Chairman.*) What was the exact construction of the induction coils?—Iron cylinders very carefully insulated with gutta-percha, upon which the secondary wire was wound insulated, and over this the primary wire. The induction coils were laid parallel and connected end to end; the distance of their striking was less than a quarter of an inch. I never got them to strike at a quarter of an inch.

1906. What was the size of the wires?—Eighteen gauge for the secondary wires, and 48 parallel wires of 14 gauge for the primary; it was a large circuit.

1907. (*Mr. Varley.*) If I remember rightly, when I saw those coils they gave a spark about a quarter of an inch in the air, and that spark lasted for a considerable portion of a second?—It did, it formed an electric arc.

1908. Do you think that applying such a power as 400 cells of Daniell's battery would be likely to injure the cable?— I do; I do not hesitate to say that in my opinion it would injure it far more than a spark from an induction coil; the battery would give you an almost unlimited amount of current in consequence of the duration of the contact. I mean, as long as you continue the contact, so long you would have tension; with the induction coil current it is limited in amount; its very existence is momentary and cannot be maintained.

1909. What is the difference during the time the electricity is flowing out of the secondary wire; is the tension capable of giving a spark through the air at a quarter of an inch, and that flying from a battery of equal tension?—It becomes a mere question of duration and amount. I believe that a far higher tension can be produced in the cable by such a battery than by any single shock or spark from an induction coil.

1910. Do you mean that its tension immediately falls so very much?—Instantly; because in relation to the surface to be charged and the conductor to convey the current, the amount of electricity evolved at a single spark is almost insignificant when compared with that from a Daniell's battery.

1911. I have been informed that those who had the misfortune to touch the cable at the time when the current was discharged from the induction coil received so severe a shock, that they have been almost ready to faint from it; is that correct?—Most certainly they would suffer for their carelessness if they touched the conductor or any bare wire; but certainly not if they handled it discreetly by the gutta-percha. If there were a defective place, of course you would get a great deal. We used to get it from the battery at times; the secondary current, in some way, in spite of good insulation, used to find its way into the battery.

1912. You said you thought that there was more danger arising from Daniell's battery of 400 cells; are you aware whether Daniell's battery of 400 cells will not give a spark that will jump through a greater distance than the 200th of an inch in the air?—I am perfectly aware that you cannot get the voltaic current to take the initiative in striking, as a coil does; but under the conditions which we are considering, I believe that you can excite and sustain a much greater electric tension throughout the whole cable by the battery of 400 cells than by a coil.

1913. (*Professor Wheatstone.*) The face of your induction coil must have been enormously greater than that of a battery of 400 elements?—Such a battery after contact once made will give you an arc with greater tension effects than the coil, and with far greater striking distance.

1914. With Rhumkorff's coil, a spark is much greater than that produced by a continuous current from a battery?—The spark is longer, but I believe the real power exerted is far less.

1915. (*Chairman.*) If some slight injury had been produced by the use of the induction coils, would the subsequent use of Daniell's battery have increased it?—Suppose a slight injury existed, any strong current would injure it. That was my reason for reducing the use of the Daniell's battery to 50 cells as the maximum. The Queen's message was sent across by 25 cells at my desire. I set aside the use of the induction coils upon the same principle which induced me to limit the number of Daniell's battery to 25 pairs.

1916. To what do you attribute the failure of the cable?—I think the combination of causes I have mentioned, with the subsequent straining and stretching during paying out, is quite sufficient to account for its failure. The risk incurred of stretching the conductor is one upon which you can hardly lay too much stress in the frequent winding and unwinding, coiling and uncoiling, in the way I have mentioned.

1917. You mean stretching the conductor by which the copper would have a tendency to be pushed outside the gutta-percha?—Yes. The result of this commonly manifests itself by a sudden starting out of the conductor, but it may show itself only after a lapse of time.

1918. Had not the cable remained comparatively perfect for several days until the use of the induction coils?—No. I do not think you can interpret it in that way. I am quite willing, if it is so, to let it be so. The first time I was able to test after submersion, I found considerable loss upon the cable at the Irish end, and said, "We will go very cautiously to "work about this; we will reduce our intensity; "we shall probably not be able to signal at all with "these induction coils; we will use Daniell's battery, "at the lowest power."

1919. (*Mr. Varley.*) If you thought the Daniell's battery more liable to injure the cable than the induction coil, why did not you reduce the power of the induction coil?—I had not the means of reducing the length of the secondary circuit in the coils, nor could I increase the quantity or duration of their current; whereas I could reduce and re-arrange the Daniell as I thought necessary.

1920. (*Mr. Saward.*) Was not the other side using induction coils?—Entirely, and from the first until Professor Thompson sent out and had the Daniell's put on.

1921. They had no other, had they?—Every message came by the induction coil except the four words I have mentioned, without any injury to the cable at that end during the time they were used. The first careful testing made at Newfoundland after the use of the 480 Daniell's cells showed that injury had commenced at that end also.

1922. (*Mr. Varley.*) You have stated that an induction current would be more likely to produce injury at Keyham than under water?—No; I think you must have misunderstood me. I said that the immersion under water would not increase its liability to injury from the use of the coils at all; that the

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iron would produce induction more than the water, and the iron was equal, at Keyham, as when it was submerged.

1923. The power of the induction coil would be wholly unable to jump through such a thickness of gutta-percha; it appears to me that it arises from the passage of the current, rather through any moisture that may be in the fault, and not from its power of perforating the gutta-percha?—No; I quite feel with you, and therefore if any fault exist, it is desirable, in order to avoid increasing it, to work with the lowest power you can signal with. I did not put any limit to that; that was the principle I adopted, and suspecting a fault to exist as they did not receive our signals, I at once disused the induction coil, and reduced the battery power in that way.

1924. You do not suppose that there is any difference between an induction coil or a battery with respect to the electric tension?—I regard an induction coil somewhat in the light of a unit jar, by which you can throw in a definite amount to the larger jar (the cable), and which being alternately of opposite polarities can never accumulate therein. Whereas a prolonged contact with a powerful battery may surcharge it even to overflowing. I think it cannot excite in the whole wire anything like the amount of tension that the continued action of the Daniell battery would.

1925. In the case of a dot made with an induction coil, that dot would last for a shorter period than the complete discharge of that coil. You say it lasted for a second, and then made the dot?—The duration of the secondary current depended upon the revolutions of the commutator which made the primary contacts; therefore, if working the induction coil at the rate of 120 currents in a minute, it could only be of half a second's duration, because it would be cut short.

1926. Therefore, when you are making a contact for the dot with the induction coil, you have the current on the whole of the duration of the dot?—Yes, you have half a second contact.

1927. In making a dash you have only half a second contact, instead of the longer contact you would have if you had used Daniell's battery?—True; half a second's contact and a pause of a second, during which no current was sent into the wire. We could not make a signal in less than five seconds with Daniell's battery. I think that the lapse of time and the continued contact of the battery charges the whole cable much more highly than a single blow of the induction coil; at all events, this is proved by actual experiment; that the very Daniell battery we were using would increase the size of any aperture made by the induction coils fourfold. Where the induction current had produced a certain amount of injury, in a faulty bit of cable purposely subjected to this trial, then the Daniell battery was immediately after applied, and the injury was found to be increased fourfold; that was the result of the experiment.

1928. (*Professor Wheatstone.*) If you had not employed your induction shocks previously, do you think the Daniell battery would have been sufficient to have produced the effect itself?—I do; we had made a hole in the gutta-percha on purpose; in fact, we had pricked it; then, when the induction coil had been put on and a number of currents had passed, and it had done its worst, we put on the Daniell battery and increased it four-fold.

1929. Would putting on the Daniell battery originally have produced the same effect?—Neither the induction current nor the Daniell would produce the aperture; but when once made, either alone was sufficient to enlarge it. We made the aperture on purpose, and the current of the Daniell battery without the coils produced this effect three or four times.

1930. (*Mr. Varley.*) I have noticed this effect in testing gutta-percha under water, passing the current from a battery of excessive tension, the passage of the current softens the gutta-percha and it falls away; that is the way in which a powerful current will rapidly increase the aperture. I want to understand why, as a general impression seems to exist abroad that an induction current would not produce this effect, the induction current lasting for half a second, a current of much higher tension should not produce the same effect as Daniell's battery lasting the same time?—I believe the difference to be due to the incomparably greater heating power of the Daniell. There is also a possibility of its lasting an indefinite length of time; it is a matter of time as much as anything.

1931. 700 cells were certain to injure a wet surface?—480 cells in series were used by Professor Thompson's orders upon the Atlantic cable.

1932. Was not that used after the cable failed?—It was intended to be used when his instruments were ordered before-hand.

1933. I think you have stated that it required five seconds to get a signal through from Daniell's battery, five seconds positive and five seconds negative?—It required at Keyham five seconds before I got a movement on the galvanometer, if you charged it, the cable, with either current, it took five seconds before the first deflection was produced by the reversal.

1934. Therefore you could not get dots in less than five seconds?—There was five seconds retardation, and it required considerably more than that to make a dot and its space.

1935. I asked that question with a view to ascertain the speed obtained by Daniell's battery?—Much depends upon circumstances, and it is difficult to answer that question; we have no accurate data to proceed upon. It would not be doing justice to the battery current to estimate it, except in the way of comparison of the velocity with regard to the working speed attainable in either way, inasmuch as it would require special instruments to do full justice to it, and these we did not possess.

1936. Have you absolute proof of that fact?—In a variety of instances I have tried it, and always found five seconds at Keyham; of course, in the Atlantic cable when laid down we could not take signals in that way, as you could not be at both ends at once; but I have proof that we received 120 currents in a minute from the induction coil; and at that rate, with proper antecedent compensation, we could have sent words at the rate of 100 currents a minute, I am certain of it. I am taking merely the two operations.

1937. Did you try any experiments with the Atlantic cable to see the speed at different lengths?—Yes, and they corresponded very nearly with those I had made out before at Messrs. Glass and Elliott's, they are as nearly identical as possible. The retardation is very nearly in the simple ratio as the distance, not as the square of the distance, and the working speed bears a strict relation to the amount of retardation.

1938. Can you give the result of those experiments?—They corresponded so closely with my previous experience, that I did not think it necessary to multiply minute observations and records. I was satisfied with the information derived from the previous experiments. I had made hundreds of other experiments on shorter lengths, on the lengths that Messrs. Glass and Elliott made the year before.

1939. (*Chairman.*) I believe you made some experiments upon Messrs. Silver's mode of insulation?—Yes, I have.

1940. What is your opinion of their mode of insulation?—At first it seemed to be quite perfect, but the specimens which I have had from them have not stood the test of time. From some cause the rubber was softened and became tarry.

1941. Next the wire?—Yes, apparently beginning at the wire.

1942. (*Mr. Varley.*) Have you found its insulating powers decrease in consequence?—It is so altered in its consistence that there is danger of the wires coming through completely.

1943. (*Mr. Saward.*) Have you tested Messrs. Silver's plan by keeping the current on continuously for a long time. I have been given to understand that

a current kept on continuously for a lengthened period through wires insulated by their method injures the india-rubber?—I have not made any experiments of that sort. I know if any injury has begun, you are, so to speak, forcing it; by keeping it constantly charged in that way you can increase the injury to any amount.

1944. I am not speaking of an intense current or of a weak current, but a current of electricity constantly on?—I have not tried that, I have tried india-rubber under very considerable pressure for a certain length of time. I think for above a week I kept up the pressure of very nearly a thousand atmospheres upon a ten feet length of Messrs. Silver's wire, and it seemed to stand it perfectly. Maintained a pressure of never less than 12,000 lbs. to the square inch.

1945. (*Chairman.*) What thickness was the india-rubber?—A 16 gauge wire double covered to gauge $\frac{1}{4}$ inch.

1946. (*Mr. Varley.*) Did you attempt in any of those experiments to put windows in the apparatus to see what was going on inside?—I should have blown the windows out.

1947. That is 3,000 or 4,000 lbs. upon the square inch?—I know these would have burst. I did not expect to see anything, therefore it did not occur to me to put windows. I have made some other experiments with india-rubber, which have surprised me very much; I have noticed that india-rubber on continued immersion in water undergoes a change in its external appearance. I find that india-rubber absorbs water to a very large extent; I have specimens of native that have absorbed from 12 to 16 per cent. I have other specimens of cold vulcanized india-rubber that have been in water 12 weeks, and one specimen has absorbed $12\frac{1}{2}$ per cent. of water in that time, but then it was an exceedingly fine film, so that I should have the maximum of absorption saturation in the shortest possible time.

1948. (*Mr. Saward.*) Was that by mere immersion?—By mere immersion I found that the other larger and thicker sheets absorbed just as much but more slowly in proportion to their thickness; the greater the surface you have, of course the more rapidly it will absorb.

1949. (*Chairman.*) Do you find when it was submitted to pressure that it would imbibe much water?—I did not try that. I believe the effect to be produced by a chemical rather than a mechanical force. It seems to become hydrated.

1950. (*Professor Wheatstone.*) How did you determine the quantity of absorption?—By having every piece carefully weighed.

1951. Are you quite sure that the water went through the inner substance of the india-rubber and was not merely adhering to the surface?—First of all I weighed the india-rubber after having dipped it in water for five or ten minutes, and dried it on a soft Turkish towel and then on blotting paper. I got the whole of the surface perfectly dried, and then I weighed it again; this was the standard weight to which I subsequently referred each piece. I then immersed it, and day after day I dried it in exactly the same way and weighed it, noting the increase.

1952. (*Chairman.*) Have you experimented upon gutta-percha in the same manner?—Yes, and it does not absorb anything; I cannot get 250 square inches to absorb a single grain of water: it may be porous, but it is not absorbent; india-rubber is not porous, but is absorbent.

1953. (*Professor Wheatstone.*) Do you find the insulating power of india-rubber affected by this absorption of water?—That will require a considerable time to be tried. The insulating power is not apparently lessened until the hydration has penetrated completely through the entire substance of the rubber. It does then appear to be a less perfect insulator, but I am not at present prepared to say to what extent.

1954. (*Mr. Saward.*) Would any insulating medium which can be worked in such a way as not to be passed through tanks of water have that advantage?—Most probably.

1955. (*Professor Wheatstone.*) What do you consider to be the best insulating material?—Vulcanite, though heretofore there have been difficulties in the practical use of it for wires. I cannot get it to absorb water; the ordinary cold cured vulcanized rubber does absorb water, but I have not found anything equal to vulcanite for insulation.

1956. (*Mr. Saward.*) Are you at all familiar with the article made under Mr. Wray's patent?—It is a compound of rubber.

1957. It is a compound of rubber and shellac?—I have some now under examination by hydraulic pressure and otherwise, and am not yet prepared to offer an opinion upon it.

1958. (*Mr. Varley.*) Have you any idea as to the ratio of vulcanite as an insulator when compared with gutta-percha?—I believe it to be equal to pure and perfectly dry gutta-percha or glass.

1959. (*Mr. Saward.*) What are the component parts of vulcanite?—India-rubber is the basis; then there is some sulphur compound; they may vary it. By stopping the baking process early you get the soft vulcanized india-rubber, and by baking it further you have what is called the hard rubber or vulcanite.

1960. Do not you think that the sulphur would be an objection?—Its action upon the copper can be avoided entirely by tinning the copper wire.

1961. (*Mr. Varley.*) Do not you think the sulphur would ultimately be taken up by the sea-water, and there would be a danger of leaving the compound porous. In the atmosphere you would have the smell of sulphur very strong?—Not from the vulcanite. You may have a perfectly polished surface. I have had some for years, and there is not the slightest appearance of sulphur in the form of bloom upon it, as there is upon the soft vulcanized rubber generally.

1962. My reason for asking the question is because I have used a vulcanite comb for some time, and after the varnished surface seemed to be worn off the smell from it was very strong?—I am not aware that it is ever varnished. The constant contact with animal oils or grease may, I think, possibly have produced the effect you mention. I have had vulcanite on purpose to examine carefully into it, and I never saw the least bloom upon the surface.

1963. (*Professor Wheatstone.*) Is vulcanite flexible enough for coiling?—It is very resilient; it is not so flexible as vulcanized rubber. I have seen one specimen of wire beautifully covered, which, I think, is quite as flexible as it need be.

1964. (*Chairman.*) Who covered it?—Mr. Hooper.

1965. (*Mr. Varley.*) Would it get into a kink without danger of breaking?—I think you might snap it possibly by sudden violence.

1966. (*Mr. Saward.*) Would not it be a bad material to pay out?—I do not think it would be difficult to pay out safely; it is just a question that can be determined practically; it is so good an insulator that it is worth while to try.

1967. Do you know anything of its expense?—I do not. I think the objection to the action of sulphur upon the wire can easily be got over by tinning the wire or coating it with zinc. I have seen recently another specimen in which vulcanized soft rubber had been actually made adherent to the metal as perfectly as if it were incorporated with it. You cannot detach it. Mr. Hooper showed me some taps the other day that he used years ago for his ordinary hydraulic beds, and the material had incorporated itself perfectly with the metal.

1968. (*Mr. Saward.*) What is your opinion as to the external form of a cable for an Atlantic telegraph?—Distinctly that there ought to be hemp in it; it should be of light specific gravity; it should neither stretch nor shrink, and should be flexible.

1969. Would a cable of this description meet your view (No. 9), that has steel inside; would you consider that a suitable cable for the Atlantic?—Yes;

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it would be a very good one; it approaches very nearly the specimen I sent for the Atlantic before it was decided upon. I sent in one with iron wires coated with hemp spun into a strand.

1970. Your ground of objection to the old Atlantic cable was on account of its want of buoyancy? —Chiefly on account of its specific gravity; you may add any unlimited amount of iron, but you do not increase its strength, because you load it exactly in the same proportion; for the Atlantic I should prefer a little more hemp than that. I do not know what specific gravity that is.

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Professor DAVID EDWARD HUGHES examined.

1971. (*Chairman.*) You have had considerable experience in telegraphy?—Yes; I have made a great many experiments since I have been in this country upon different submarine cables, more with a view of trying to increase the rapidity of speed through them than with any other object.

1972. Has not the subject of insulation occupied your mind?—It has; independently of the speed of transmission, the question of insulation has occupied my mind a great deal, because I noticed the faults nearly in all cables.

1973. Have you made experiments upon the question of increasing the speed of electric currents?—Yes.

1974. Will you give the Committee a description of those experiments?—The first of them was to increase the speed by reducing the number of signals necessary to produce the letters. I took what was received as so many waves a minute, and then converted them into a greater number of signals.

1975. That is to say, you formed a sort of vocabulary or new alphabet?—Yes; that was my object at first, and afterwards I tried to increase the number of signals themselves. I have made experiments through different cables, through the Atlantic, through the Tasmanian, through the Red Sea, through the Denmark, and different others.

1976. Were those experiments made before the cables were submerged?—Yes, none after. Those experiments give very different results, and some of those results seem to fully bear out the opinion that a large conductor is far superior as regards speed than a small one.

1977. You do not agree with Mr. Whitehouse in the opinion that he has given upon that subject?—I do not at all.

1978. Have you found that increasing the size of the conductor has largely increased the inductive effect?—No; but I find in practice that I get more speed through a larger conductor than I do through a small one; through the same length in fact there is a very great difference when we change the observing instrument, or with different size conductors.

1979. What is the law of that increase of speed which you have discovered?—I can give you the results and the number of speeds that I got through different lengths of cable; for instance, with the Atlantic cable, of a length of 2,500 miles, I could only get 20 waves or 40 currents in a minute, the size of that conductor was a 14 wire. With the Tasmanian cable, of a length of 240 miles, I could get 150 waves a minute, the size of that wire was 16, it was one solid wire. With the Red Sea cable, of a length of 1,700 knots, nearly 2,000 miles, I could get 30 waves, the size of the wire was No. 11; through 1,000 miles of the same cable I could get 50, and through 500 miles I could get 120 in a minute. Through the Denmark cable, with a No. 16 wire, 300 miles in length, I could get 110 waves; but in the Atlantic cable I used the induction coil apparatus to get this high speed, with the Red Sea cable I used Daniell's battery. These were practical results counted in the transmission of messages; counting mere reversals, I could receive one-third more; the 20 waves through the Atlantic was arrived at only by using a sensitive relay, whilst the Red Sea was worked same distance easily without relay.

1980. Did you find the same difference between the speed obtained from the induction coils and from Daniell's battery which Mr. Whitehouse has described?—I did. I made some experiments whilst I was at Keyham to determine the speed and I found, as he states, that the currents were faster and were according to the experiments which I tried four times faster; but the battery which he had was not suitable for the experiments, the battery that I used was a sand battery and not a quantity battery, it did not seem fully to charge the wire. I tried the same experiment on the Red Sea cable, and I found no difference, because I had a battery whose quantity was sufficient.

1981. What was the battery that you tried upon the Red Sea cable?—Daniell's and a sand battery.

1982. (*Professor Wheatstone.*) Of how many elements did the battery consist?—I tried several; I worked with 75, 50, 25, and lower still.

1983. Did you find the speed vary with your battery power?—No, it would depend upon the delicacy of the instrument. I found that I got no more rapidity of speed with the 75 battery than with ten, provided my instrument was delicate enough to work with a current that would come with ten.

1984. In your experiments did you employ the same instrument?—Yes, I have used other instruments, but they all give the same result.

1985. (*Mr. Saward.*) In those experiments did you consider the ratio between induction on the external circumference and conduction through the cubical content of the copper?—No.

1986. Have you considered what the ratio would be in proportion as you increase your copper between induction on the external circumference and conduction, of course if you increase the size of your copper you increase the induction as well as the conduction?—Yes.

1987. I am anxious to know in what ratios, respectively, those were increased?—I am not prepared to answer that fully; but I have been making experiments with the view of trying to determine some law that would follow that out; I find that the induction is in exact proportion to the resistance of the conductor; therefore, if for instance we have a force of ten, we shall have a proportionate induction, provided the dialectric remains the same under all circumstances; if we reduce the resistance, we shall reduce the induction, provided we do not increase the size of the conductor; if you increase the size of the conductor, you would in a certain degree reduce the induction, provided the same force was used, so that with a large conductor you will get an increased speed and really less induction, though you have more surface. I think that it is borne out by experience. I think the retardation in cables is not due so much to what we might term induction, as to charge and discharge.

1988. (*Chairman.*) Do you mean the residual charge?—Yes, the charge which is absorbed in the gutta-percha. We will say the gutta-percha will rob the current of ten, so that when you put in a reverse current, it will have to be reversed before the other current can be of effect at the other end.

1989. With different sorts of insulators from gutta-percha you will get a different residual charge?—We get a certain result from every one. I believe myself that there is no limit to the speed in cables; it is only a question how fast we can neutralize the charge and discharge of the dialectric; our speed in cables will always be in proportion to the delicacy of the instruments; an instrument which only requires five of current will record twice as fast as that which requires ten.

1990. (*Mr. Varley.*) Supposing you have two apparatuses made exactly alike, but the one requires twice the power to work it compared to the other, and you put on with this second instrument twice the power; if you were sending signals, do you say

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that the speed of transmission then would be reduced to one-half or not; that is to say, with twice the battery power with an instrument of only half the sensibility, the same distance, and the same everything else?—I believe it would be nearly the same, if I understand you rightly.

1991. You said a short time ago that you believed speed to be independent of the battery power used; that is to say, whether you used 75 cells, 50 cells, or 25 cells, you invariably got the same speed?—You would, provided you had an instrument proportioned in delicacy to the amount received.

1992. You have not stated what apparatus you used?—I always used the same to get these results, viz., my printing telegraph, which has an electro-magnet of great rapidity of action, and exceedingly sensitive to small currents. I had other results with different electro-magnets; with some magnets I got 50 currents—other magnets only gave 20—proving at once that it required more to charge it. I believe it will be fully borne out, the first instant you put a current through the cable, instantly, you might say, that there is a current at the other end; the only reason why we do not see a small quantity of charge is because we have no instruments of sufficient delicacy to work it. We have instruments at work which require a certain amount of charge, the current keeps getting gradually from 1 to its maximum, say 100. A magnet requiring as its smallest amount 50 to move it, you will receive the first instant 1, and the next instant progressively from 1 up to 50; at 50 the magnet moves long after the first portion of the current has arrived.

1993. (*Chairman.*) You mean that the velocity of the current depends upon the delicacy of your instruments?—Yes, to perceive small currents.

1994. You think the current is instantaneous?—I believe so, the most minute portion is, because if a great part is robbed at first by induction it keeps getting stronger and stronger, still there will be a small, it may be an infinitesimal portion arrive the first instant at the other end.

1995. Have you made experiments to prove that?—Yes, I have made experiments; I kept increasing the speed, and the current was not lost; you may send rapid reversals; if you send rapid reversals, we will say twice a minute, you may get perhaps 60 degrees as the result of each reversal at the other end, if you halve that and send them in half the time you did before, namely, twice as fast, you may get only half the result; the faster you send them the less result you will get at the other end of the circuit.

1996. (*Mr. Varley.*) Have you found that as you increased your speed so your deflection decreased in the same proportion; if you increased your speed from a reversal of once per second to a reversal of two per second then your deflection at the other end varied from 60 degrees to about 30 degrees; do not you find them respond in a corresponding degree?—I am not prepared to state just now the exact scale.

1997. You are aware of Professor Thompson's experiments, that a current is found to arrive in a line which would describe a curve; from your statement your experiments seem to make it appear that it does not arrive in a curved line but in a straight line?—I believe it to be practically a straight line.

1998. The current goes on continually progressing in a straight line?—Yes, from infinitely nothing up to the degree you wish to get at.

1999. There is a certain period at which it will arrive at its maximum, and that is the end of it?—Yes; practically.

2000. You are aware that that does not apply with Professor Thompson?—I believe so.

2001. He states, upon the result of his experience, that it will take an eternity of time to charge a cable, and consequently it will take an eternity of time before you can approach to the maximum curve at the distant end?—I believe that the charge of the cable is in proportion to the resistance of the wire.

2002. You believe that in a given period the cable will arrive at its maximum charge?—Yes, according to the resistance of the wire, to prove that, if we have a galvanometer at the other end, the first instant we touch the battery we see nothing, the next instant the needle will commence moving from 2 to 6 up to 60, and will never go any more; it will stay there as long as we have the same current. You do not see any curve in the movement of the needle, the needle goes gradually up.

2003. You say that it will work continually up, and then must come to a dead stop; have you found it to do so?—Yes, it does.

2004. You have stated that in the Atlantic cable, 2,500 miles, you got 20 waves per minute, was that 20 positive and 20 negative?—Yes.

2005. Mr. Whitehouse has stated that he got 120 a minute?—Yes, I heard him state so; he stated so to me, that he had got that number, and I asked him to show it to me, but the most he could get was 20.

2006. (*Chairman.*) Was that after the cable was submerged?—No, before, at Keyham. I think Mr. Whitehouse was speaking of the time it was submerged. I never saw it, but I was two or three months at Keyham.

2007. (*Mr. Varley.*) In the Atlantic cable you say that you got 150 waves a minute. Was that plus and minus or 150 positive and 150 negative?—300 currents.

2008. With the Red Sea cable you only got 30 waves, that was 2,000 miles, though its conductor was very much larger, do you attribute the low speed to the fact that you used the battery current instead of the induction current?—Yes; but I worked this distance easily without relay, which could not be done with the same magnet on the Atlantic, either with coils or battery.

2009. And with the Red Sea cable I understood you to say the induction coil did not increase the speed in any degree?—It did not. I did not have on so large a quantity as we had on the Atlantic. I had small coils.

2010. What was the size of the secondary wire?—It was very thin, 28.

2011. You do not think it was to any peculiarity of the construction of the Red Sea cable that caused this difference of speed between it and the Atlantic, a difference as one to four, in the Red Sea cable you got no difference whether you used the induction coil or the battery current?—The only reason I say so is because the small experiments I tried would not permit any large induction coil to be used, as I could in the Atlantic cable. I may not have used the proper batteries to try the relative speed.

2012. Are you of opinion with suitable batteries that the speed of the Atlantic cable would have been the same with the battery as with the induction coil current?—I am of that opinion.

2013. (*Mr. Saward.*) In constructing those induction coils that were used on the Atlantic cable, do you agree with the plan of coiling the primary wire round the iron core?—I think that can only be determined by experiments. Mr. Whitehouse thinks that to be the best. I have not gone into actual experiments to try which is the best. I do not agree with Mr. Whitehouse that the induction coils could not injure the cable at all. I have worked the coils for some time and they would, as he said, give a spark through a quarter of an inch of air, for a short time, say a second. Now he states that the shortness of the time would prevent its injuring the cable. I do not think so, for this reason, we will take the static electricity, though it lasts only an instant, we may burst the cable with the smallest spark, with the battery of 500 cells the quantity cannot in the slightest degree damage the cable, the question of time has nothing whatever to do with its bursting the cable.

2014. (*Mr. Varley.*) Have you known any occasions on which telegraphic wires had been struck by lightning, and what effect it has had on the gutta-percha wires, which are connected with aerial wires, have you seen any specimens?—No, I have not.

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2015. There is a specimen (*handing a specimen of wire to the witness, burst in a series of holes at intervals, at about an inch and a half*)?—Mr. Clark and myself made some experiments with static electricity upon all the gutta-percha cables that had been made, and we found that they could all be burst with a momentary spark; we found that they could be burst easily with one spark from the Leyden jar which was charged.

2016. (*Chairman.*) Is it your opinion that the use of induction coils did injure the Atlantic cable?—Yes; I have found on testing some wet gutta-percha, that it is a worse insulator than the atmosphere; therefore if it is really a worse insulator than the atmosphere under certain conditions, if a spark will go through a quarter of an inch of the atmosphere, it would also go through a quarter of an inch of gutta-percha, this would be reduced by the resistance of the wire; but if at the other end you disconnected the wire from the earth, and at the same time were sending in currents from this end, then the resistance of the wire would have been infinite. A current will go through gutta-percha if it had sufficient power, which I believe it had.

2017. Do you consider that a Daniell battery would increase an injury which the induction coils had made?—I believe it would, if the injury was near at hand.

2018. Even with a small number of elements?—No; it depends altogether upon the number of elements and the resistance introduced.

2019. (*Mr. Saward.*) Do you think 500 elements would have injured the cable?—I do not think that 500 elements would have injured the cable.

2020. (*Mr. Varley.*) I do not believe that 500 elements had been used before the cable had ceased to speak. I said as a last resource you cannot make the cable any worse than it is; if we can oxidize it sufficiently we may as well do so?—I believe that was the fact. It is very objectionable to use inductive currents; what we want is quantity, to fully charge the conductor, and it will not have the effect of destroying the insulation. If my experiments are correct we shall work just as fast with a large quantity as with high intensity (provided we use instruments sufficiently delicate for the purpose). In the case of the Red Sea cable I got as high a speed with 25 as I did with 75.

2021. (*Mr. Saward.*) Were those records made on your patented printing instrument?—Yes.

2022. You have given a good deal of time with a view to adapt that instrument, to submarine cables, have you not?—I have devoted all the time that is essential.

2023. Do you consider that you have succeeded?—I think so. The great difficulty which I had with the instrument, which I employed for a sort of registry, was to make every wave produce a letter; we can only do that by a perfectly synchronous motion, as Professor Wheatstone knows. I have a mechanical arrangement, but the great difficulty on submarine wires is the unequal charge and duration, because in a synchronous motion you must have unequal duration between each letter. In the Morse signals we have three unequal; we have a dot, then a line and a space, you might say, perhaps, four, counting the space between words; it is a question of time.

2024. Do you think that each wave will make a letter with your printing apparatus?—Each wave will print a letter, in fact I have it arranged so that each half wave does, every current prints a letter.

2025. (*Mr. Varley.*) Suppose you were working by the system generally adopted every positive current produces a mark, but the negative current produces a space. By your arrangement I understand you reverse your apparatus after having given a positive current, so that the negative arriving shall pass aside through your apparatus in the opposite direction, and produce the like signal, so that if you could record a letter for every alternate wave and every positive wave you would have twice the speed?—I have not merely that, but I have speed by the reduction of the number of signals. On the Morse, it would take ten currents for each letter on the average, or the time of it.

2026. For one letter?—Yes, ten currents; the Morse system would require 25 waves or 50 currents for each word, whilst my system requires but 7. Now, I take one wave on the printing instrument to each letter, one half current, we will say, is necessary to print a letter; but there is the question of time between each signal, and that loss of time is on the average equal to one half the signal, so that really one and one half current is the time required for each letter.

2027. With your system you have a wheel rotating round, and you have to wait until each letter comes round, therefore you require intervals varying from one to three. If you go from Y to X, your wheel must rotate from Y till it comes to X?—Yes, 25 intervals; but from A to B is not the unit of time in my instrument. If I took A to B, than I should have 28 different intervals of time, the number of time signals in use would be 14. You can print, on the average, from this revolving type-wheel two letters to one revolution of the type-wheel on the average, taking a newspaper down, that is the average and counting the alphabet. If you were to arrange to print each successive letter then it would require 14 units of time; but if you arrange your instrument so that the shortest period of time you can print is from A to A, it would make one complete revolution before it can print one letter at all. You have one revolution to represent the unit of time which is shortest, therefore you have another letter come during another unit, so that you take the half of that and you will have a unit and a half of time.

2028. Can you rely upon the waves coming with such precision and regularity and set your apparatus with such delicacy that you can rely upon the deflection of the wave, so as to use the fourteenth part of a wave for your power in working through such a cable as the Atlantic?—Yes, perfectly.

2029. Have you tried any experiments from which you have reason to hope that you will get to such refinement?—I have upon the Atlantic and many other cables; it depended upon the accuracy of the instrument in sending the waves, because they always recorded them just as upon an air line; the unequal character of sending different waves, would change the time of their arriving; that was the greatest difficulty. If I always sent the letter H., it would always print the letter H. if I stopped one revolution, instead of printing H. it would print G., though I touched the same key; the reason is because the wires become charged; the next wave came faster before it came round to H.; that is the great difficulty, and for which I had to compensate, it is the charge and discharge of the wire, it takes a certain time to charge, and a certain time to discharge; if that is unequal, and I do not compensate for the time, it will of course become a different time according to the difference of the time allowed; but I have a mode of compensation which always keeps the wire in the same state, so that every letter has the same amount to overcome exactly, no matter what the time is, the speed of each one is exactly the same, no matter what the distance is, because the act of charge on the wire is exactly the same, the only difference caused by the difference of time is because it is charged or discharged higher; if you keep the wire charged to a certain degree, the wave will always travel in equal times corresponding to the amount of charge.

2030. Supposing you get one wave through per second, how fast might you let your wheel rotate to work it accurately?—The wheel would revolve once a second.

2031. Therefore to get any particular letter you would have to rely upon the 28th part of a wave?—Yes; I should have, as the smallest amount, one current; any addition to this in length would determine the letter intended.

2032. And you think you can do that with certainty ?—Yes.

2033. (*Professor Wheatstone.*) If you send a wave, say the 300th part of a second in duration, from one end of the wire, are you certain that it would be exactly of the same duration at the other end of the wire ?—Provided there was no charge or discharge; it always depends upon that condition; if there was no induction and no charge, for instance, if it was an air line, you would get accurately the same; as they are sent in they must go out, because you can divide a current into hundredths, and divide those parts into hundredths and thousandths each; that will be as accurate as before, provided you have something which will register them.

2034. (*Chairman.*) Can you depend upon a submarine line like the Atlantic being always in uniform condition ?—I believe the Atlantic cable can be just as perfect in its condition as any other line.

2035. (*Mr. Varley.*) Have you done actual work with your apparatus through any submarine lines ?—No; I have not had an opportunity.

2036. Is not there a difference in a submarine line after it has been laid as to the regularity of its working ?—There is the question of equality of temperature, earth currents, and all those things.

2037. The variation of batteries requiring very great sensitiveness in the apparatus ?—Yes.

2038. Taking all those sources of irregularity into consideration, which so embarrass telegraphy at present, do you still think that you can work to the 28th part of a wave ?—Yes. Those causes of irregularity at the same moment compensate themselves in the way I use them. I would much rather show you than tell you.

2039. (*Mr. Saward.*) I believe you obtained that result partially by not working up to the full strength of the current received ?—I did not.

2040. You can cut off a portion of a current ?—I think I can explain how the compensation is done. In a given case of charge and discharge it takes a certain time to charge up to a given state, and it takes a certain time to discharge; that would represent a given current. If you have instruments by which you can record every portion of the synchronous motion, if you regulate the proportion you put in in proportion to the discharge of the cable, you can see that the compensation will be exactly the same, and in every case the cable would be in the same state, with variation of time between the signals.

2041. (*Mr. Varley.*) That is, assuming that the intervals between the letters are the same ?—No, not at all.

2042. If there be a longer interval between the letter, during that interval part of the electricity will have escaped ?—Yes; if not compensated for; it is a mere question of time and mechanical arrangement.

2043. (*Chairman.*) Have you also turned your attention to the insulation of cables ?—Yes.

2044. Have you adopted a peculiar mode of insulation ?—Yes.

2045. Can you describe it ?—First the wire is coated with gutta-percha, or any other insulating medium; then there is a coating outside of semi-fluid, between that and the outside, that is to restore any mechanical injury that might occur, either in making the cable or any injury that might occur afterwards. If we cut through the cable at any place, or make a mechanical injury down to the wire, we find that we shall have total loss of insulation as long as the knife is there. This coat of semi-fluid fills up the injury as soon as the knife is withdrawn, and the wire will be as perfectly insulated as though the gutta-percha were upon it.

2046. (*Mr. Varley.*) Has it ever occurred to you that using such a semi-fluid insulator, if the cable got damaged during the process of manufacture, it would be hurt for the time, and you would make the injured cable good by that same fluid compound; have you tried any means for testing that fact as to remedying it before the cable is shipped ?—The semi-fluid is put on with pressure, and any fault in the manufacture would be seen at once by its oozing out; if it restores the injury, it never goes bad again; it cannot be made to go bad again by any treatment whatever. If there is a fault in this cable, if it is punctured with a needle, or if there is a defect in the manufacture at that place, and it is healed at all, the battery cannot affect it. We have tested it under a pressure equal to 3,000 fathoms at Messrs. Easton and Amos's works, with one foot of the wire laid bare to the water, showing total loss of insulation; at the end of ten seconds the insulation was perfectly restored, and could not be made to go bad again. I have kept it under battery for some fourteen months, and it shows not the slightest loss or the slightest deterioration after it has become once healed.

2047. (*Mr. Saward.*) You only put it forward as an additional precaution, not to supersede all the care that can be taken ?—Not at all.

2048. (*Mr. Varley.*) Would it not leave a loophole for carelessness ?—There is no reason, because you put a safety valve on a boiler that a person should be careless in other respects, and blow up the vessel.

2049. (*Chairman.*) Have you had this compound chemically examined to see whether it injures gutta-percha ?—I have gone through many experiments myself with that view.

2050. What have those experiments been ?—In the first place, I wanted to test whether it would dissolve gutta-percha in the ordinary temperature, and it will not. This semi-fluid contains not the slightest particle of naphtha, or anything that will injure gutta-percha. The proof of that is, if you take a rotten pipe of gutta-percha that will break to pieces directly you touch it, and submerge it in the semi-fluid, it will become plastic and as good as new. You can restore old gutta-percha by this semi-fluid.

2051. For what period of time have you submitted gutta-percha to this action ?—Thirteen months. It has a peculiarly hard and flexible appearance about it which new gutta-percha has not.

2052. (*Mr. Saward.*) Are its component parts a secret ?—At present, for commercial reasons.

2053. (*Chairman.*) Is not it patented ?—Yes; I have no objection to mention the ingredients to the Committee in private. The insulation of these pieces, immersed in it for 13 months, is far superior to anything made at present, so much so, that the wire I have drawn out of these tubes, when tested for insulation, is four times superior to the best gutta-percha.

2054. (*Mr. Saward.*) What tests did you apply for insulation ?—Static electricity, because it is impossible on a small scale to test with battery power sufficient.

2055. (*Mr. Varley.*) Do you find it four times as perfect as the ordinary double covered gutta-percha ?—Yes; when a wire is made at the gutta-percha works they are very careful to cut out any faults; there it goes to the manufacturer and is tested for insulation, and they cut out any faults again; then it is manufactured, and if there is any faulty place they cut it out again; after the cable is laid it is too late to cut out any faults. The object of this compound is to provide some remedy for that; and no possible sort of injury can occur to the insulation of the wire if the outside covering is not broken, and the insulation of an Atlantic or other cable might be cut through its entire length to the conducting wires, and still remain perfectly insulated.

2056. (*Chairman.*) What would be the extra cost of this process ?—I do not think it would cost any more than the ordinary covering; the semi-fluid is much cheaper than gutta-percha.

2057. (*Mr. Saward.*) Can it be applied in the same way as gutta-percha, on their dies ?—Yes, the Gutta-Percha Company have made two miles with it.

2058. In what manner mechanically is your semi-fluid applied ?—They would not allow me to see the machinery.

2059. In what length of time does that semi-fluid

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material become solid?—Never so far as I can tell from 13 or 14 months' experiment.

2060. What effect has water upon it?—It will not mix with water at all; you may boil it with water or test it in any way, if you put on 500 or 100 cells of battery you will not see the slightest loss of insulation on the injured place.

2061. (*Mr. Varley.*) Do you think you could arrange it so that in the event of a wire getting out of the centre from heat it would keep the wire sound after the wire has ceased to be in the centre of the insulating covering?—I have tried several experiments with that view, and if the wire is covered with yarn and then exposed to heat, the wire, it is true, will fall, but the semi-fluid gets round the wire because of the hemp holding it, and it will not wash away; the coating is sufficient at that place to hold a film of semi-fluid; if there was any sort of fear of heat it would be better to coat the inner wire with some substance which would not be affected by heat.

2062. (*Chairman.*) Does the semi-fluid become very much more fluid with the increase of temperature?—No, it does not.

2063. It does not alter its condition much from increase of temperature?—No, it does not absorb oxygen from the atmosphere, it is a hydrocarbon, and it has never been frozen yet by any cold whatever.

2064. (*Mr. Varley.*) You do not anticipate any danger from it in the way of rendering defects for the time being invisible, and so escaping the ordinary tests, and then letting the electric current out?—In the first place the places that are healed cannot get bad after being once restored; secondly, it can be very easily tested by applying high statical electricity to the place which will show immediately the effects of the semi-fluid. The insulation by means of the semi-fluid shows also a remarkable low specific inductive capacity; you have two miles and they are better than any that have been made before.

2065. (*Chairman.*) You mean the wire made for our experiments?—Yes, they take less charge and discharge.

2066. Except one?—Except none; one side of the semi-fluid cable shows less inductive capacity than any other that has yet been made. The wire coated with 20 coats would take a charge of 25, whilst this mile, with but two coats, would take only a charge of 20; this mile I speak of is now at the Gutta-Percha Company's works, and I presume you will be allowed to test it there.

2067. (*Mr. Saward.*) During your stay at Keyham did you test the Atlantic cable?—I did.

2068. Did you form any opinion as to its insulation, as to whether there was any evidence or appearance of injury or loss?—The loss seemed to be so great after a certain distance that really you could form no estimate; there seemed to be no one faulty place, but it was generally bad. The Red Sea cable was much superior.

2069. Did you form any opinion as to the cause of those indications in the Atlantic cable?—No; at the time I thought it was owing to so many joints being made in the cable.

2070. Have you on subsequent reflection, and from a good deal of acquaintance with what has happened, formed any opinion as to the efficiency of the Atlantic cable at the time it went out, as to what the causes of the supposed inefficiency were?—I think there were three causes; one was the heat to which it had been exposed, another the numerous joints, and the third the induction coils which had made many imperceptible holes, those coils may have been put on when the other end was open, as I have seen it done many times.

2071. Is there nothing in the causes which have led to the disappointment of the Atlantic Telegraph Company which would militate against the permanent establishment of a telegraph between Ireland and America in your opinion?—I think that we really do not estimate highly enough how well we shall be able to work in the next cable and how easily it will be worked; the cause of delay in a submarine cable is owing to the resistance of the wire, the induction of the current, the imperfect insulation, and the number of signals required for each letter—we shall have the next wire certainly with at least 30 per cent. better copper for conduction, and we shall probably have less resistance by having increased wire; we shall perhaps have a dialectric that will not take so high a charge, and I hope far superior insulation, therefore under these circumstances I shall expect to see at least four times the number of currents with the same instruments which we were able to get before.

Mr. Hugh Adams Silver examined.

Mr. H. A. Silver.

2072. (*Chairman.*) You have turned your attention to the insulation of telegraphic cables by means of india-rubber?—I have.

2073. Will you describe the process which you recommend for that purpose?—I should tell you, perhaps, that my experience is based upon the manufacture of from 70 to 80 miles of wire covered with india-rubber made mile by mile, and almost quarter of mile by quarter of mile, included from one to two hundred experiments as to the method of applying the india-rubber in the way in which it is done. I cover the wire with spiral laps of india-rubber, one, two, three, four, or as many more thicknesses as may be desirable, submitting them to a moist heat. The object of submitting the india-rubber to a moist heat is to produce consolidation.

2074. Does the heat which you apply to india-rubber alter its character at all?—Not at all.

2075. Is the india-rubber, when it is first put on, the pure bottle india-rubber?—No, not the pure bottle india-rubber; it is masticated, manufactured rubber.

2076. You are aware that the copper wires that have been covered by you exhibit a peculiar decomposition of the india-rubber close to the wire in frequent cases?—Some of them do; I have seen specimens.

2077. To what do you attribute the decomposition?—I think that an action goes on between the copper and the rubber under certain circumstances when exposed to the atmosphere.

2078. Do not you think that that would be detrimental to its use as an insulator?—No, because even in this state it insulates admirably, and the action does not go on when the wire is laid under water or under ground.

2079. Have you observed that if any action goes on, the decomposition increases with time, and therefore is not it a question whether at the end of a sufficiently long time the whole of the india-rubber might not become decomposed, and the wire sink through it to the outer covering?—I could not answer that question exactly, but I may state that under ground or under water, from my experience, that action does not go on. And further, when the action has commenced, its progress is entirely arrested if the wire be placed under water or under ground.

2080. Have you had any india-rubber submerged for a sufficient period to enable you to decide upon that point?—For a period of many months.

2081. And no wire so submerged has decomposed in that manner?—No, neither under ground nor under water.

2082. Has no action gone on near the wire which has been submerged?—None whatever; it has been perfectly sound and hard; whereas a portion of the identical wire (cut off of the coil from which the lengths were taken and laid under water and under ground) exposed to the atmosphere has softened, and presents the treacle-like appearance referred to.

2083. Will you describe the manner in which you lay on the india-rubber?—The india-rubber is cut

into strips of varied widths, from half an inch to one inch. It is then wound round a bobbin which is attached to a machine,—I might term it almost a slide and pulleys on a drum,—and drawn through a hollow maundrel, and the bobbin is made to revolve and deliver the rubber on the wire. There is a drum on the other side which draws the wire through, and that process is multiplied as many times as it may be needful to do it.

2084. You have no fear that the very large number of joints which take place in your mode of putting on the india-rubber may lead to defective insulation ?—None whatever.

2085. (*Mr. Saward.*) Is it put on in a state of tension ?—Yes ; this is a specimen of the rubber as it is put on (*producing the same*). There are sixteen laps of the india-rubber ; I do not mean to say that there are sixteen different tapes, but sixteen different laps. There are four tapes, each lapping four times.

2086. (*Chairman.*) By what experiments have you tried to test the electric capacity of this mode of insulation ?—My attention has been more directed, I may say entirely directed, to the manufacture of it. I have tried very few experiments electrically.

2087. (*Professor Wheatstone.*) Have you had any experiments made upon the relative insulating power of india-rubber and gutta-percha ?—No, none to any extent. I am not able to speak upon that subject.

2088. (*Chairman.*) What led you to consider that india-rubber applied in this way would be so much better than gutta-percha, if you have not had experiments made ?—I have heard a great deal upon the subject. I am not now speaking of my own personal knowledge.

2089. You have consulted other people upon it ?—Yes.

2090. (*Mr. Varley.*) I tried experiments on a piece of wire containing one hundred joints, and the result as to its insulating power was, that it was seventy times as good as that of the ordinary double covered gutta-percha wire with Chatterton's compound ?—That is a piece of the identical wire to which you refer, and with which we supplied you (*producing the same*).

2091. (*Chairman.*) What number of laps had that wire ?—That had sixteen laps.

2092. Were there sixteen separate layers between the wire and the air ?—There were sixteen separate layers, but only four separate tapes ; each tape was lapped four times over and brought back.

2093. Have you had wires covered with this material submitted to pressure in water ?—Yes.

2094. To what pressure ?—Up to 11,500 lbs. upon the circular inch.

2095. That would be equal to about 14,000 lbs. upon the square inch. What effect did that produce? —It stood it perfectly ; it was the thinnest film we ever put upon wire.

2096. How was the pressure applied? in what sort of apparatus was the wire tested ?—It was in a tube.

2097. With the ends exposed?—Yes, a current was passed through it during the operation.

2098. Was that calculated pressure or pressure obtained by raising a definite weight ?—Calculated pressure from an index.

2099. The pump did not at the same time raise a dead weight equal to 11,000 lbs. ?—No.

2100. Did you see anything in the course of these experiments to lead you to the conclusion that any greater amount of pressure than that would cause any alteration in the wire ?—I saw nothing to lead me to that conclusion. *Mr. H. A. Silver.* 15 Dec. 1859.

2101. How long was the pressure continued ?—For about half an hour.

2102. (*Mr. Varley.*) That is the longest pressure to which you submitted it ?—Yes.

2103. You have stated that it was a very thin film. I presume it was a bladder or something of that sort ?—No, it was laid on spirally in this way.

2104. (*Chairman.*) How many laps would it have? —Two laps.

2105. Have you made any experiments to see whether india-rubber imbibes water ?—Only the experiment I alluded to just now. We had for some six or seven months under water a length of wire some twelve miles or more.

2106. Did you find it imbibe any moisture ?—I found the insulation was as good when we took it out as when we put it in, and from that I infer that it imbibed no moisture.

2107. You have made no definite experiment with a view to ascertain that ?—No. But this question is, I think, practically answered by the various instances on record, such as the india-rubber covered line across the Hull Docks, which has been submerged for about 10 years, and that from Key Haven to Hurst Castle, which are in good working order after being under water for many years.

2108. (*Professor Wheatstone.*) Mr. Willoughby Smith stated that he had tested some india-rubber covered wire, and found it very faulty ; can you explain the cause of that ? He stated in evidence the other day that it was furnished by you to him?—We have not supplied any of our wire to Mr. Smith or the Gutta-Percha Company directly ; if otherwise obtained, and it was found imperfect, it must have sustained some damage after it left our hands.

2109. (*Mr. Saward.*) Mr. Willoughby Smith said, "I have had one mile of Silver's covered wire of "india-rubber, and compared it with a mile of gutta-"percha covered wire of the same size, and in trying "my experiments with the india-rubber core it broke "down after a day's testing. I think I had not more "than one day. I divided the core into five lengths, "and each length broke down." Then I asked, "Was the same battery power used in each case ?— "Yes. What amount ?—504 pairs of plates ; the "ordinary sand battery ; six inch ?—No, four by "three." Then the chairman asks, "What length "was the wire ?—16 inch copper wire covered to "number 4 gauge. Was Silver's wire covered by "themselves as a model specimen ?—Yes. To what "do you attribute its giving way ?—I still have the "pieces, and intend to trace out these faults ; I have "not been able to do so yet. Messrs. Siemen's are "trying some experiments at our works. I see they "have half a mile of rubber wire in the canal, which "I tested the other day, and it seems pretty good ; "but that is only once testing." Have you any observation to make upon that evidence ?—I cannot say at all where Mr. Smith got the wire from ; certainly not from us ; I repeat, we have not supplied any to Mr. Smith or to the Gutta-percha Company.

Mr. Charles West examined. *Mr. C. West.*

2110. (*Professor Wheatstone.*) You are concerned in the manufacture of india-rubber covered wire with Messrs. Silver, I believe ?—They are working under my patent. I have turned my attention to the subject of insulation for many years, and I have always, from the commencement, before gutta-percha was known in this country, used india-rubber, and at that period in manufacturing it I was compelled, as Mr. Silver has described, to put it on the wire by winding it spirally round, in order to make a solid tube of it. I have had recourse to solvents ; sometimes it answered, and at other times it became faulty, so that there was no dependance upon it ; in the meantime gutta-percha has come up, and gutta-percha has been extensively used, and india-rubber, from the causes I have stated, lost caste. Although I have always found it to be a very excellent insulator when put on correctly yet from the causes I have named, it was thrown aside, especially as gutta-percha was considered at that period a far better insulator and more easily put on the wires, as it was put on through a die with great facility. In the year 1845-46 I laid a cable across Portsmouth

Mr. C. West. 15 Dec. 1859.

harbour, made of india-rubber. That portion of the cable I made for Sir Joseph, then Mr. Paxton, and Mr. Charles Dickens, who were about to start a morning journal. Captain Taylor was with me, and he went over to Paris to get a concession, or rather to get permission, for we did not ask for a concession; but he was delayed there some months, and before he could get any official reply, the object for which these gentlemen wanted the cable had passed away. I then began to manipulate it for the purpose of laying it between Dover and Calais; I took a mile of it to Portsmouth harbour, and laid it across by the desire and with the sanction of the Lords of the Admiralty; I have a portion of it now, which I will show you (*producing it*). This has been in Mr. Hay's possession ever since the year 1846, and will speak as to the durability of india-rubber.

2111. I observe that there is cotton or some other material placed between the india-rubber and the wire?—At that period I first covered it with cotton, steeped in shellac, and then put layers of india-rubber.

2112. How long did this remain submerged?—That has been with Mr. Hay; he gave me a letter, stating that he had used it very roughly; sometimes it was in the water, at other times it was kicking about in the dockyard, exposed in the sun, dragged across the stones, and subjected to very rough treatment; notwithstanding which the india-rubber has got quite hard, and seems almost as good as it was the first day.

The following is a copy of Mr. Hay's letter:

Chemical Department, H.M. Dockyard,
Portsmouth, May 25, 1859.

DEAR SIR,

HEREWITH I forward you a small piece of the india-rubber covered wire which formed a portion of that which was laid down experimentally under my superintendance (in accordance with directions I received from Admiral Superintendent Sir Hyde Parker, to give every assistance), between H.M. ships *Pique* and *Blake*, and subsequently across the harbour, in 1846. The insulation was at that time considered by all who witnessed the experiments to be most satisfactory. Since that period I have used it as occasion required, and the insulation is now quite perfect, although my usage has been very rough, such as exposure to the sun, and frequent coiling, and straining on rough stones after use in the water. With respect to the reports you refer to, I must beg of you to make your application to the Lords Commissioners of the Admiralty, who will, no doubt, if they are in existence, cause you to have copies of them.

I remain, dear Sir,
Your most obedient Servant,
WILLIAM JOHN HAY.

C. West, Esq.

After that period I discontinued the use of cotton with the shellac, and covered the wire directly with india-rubber. Here is a portion of the very same india-rubber cable that is now laid across from Hurst Castle down to Key Haven, where it has been in constant use from the period of its being laid to the present (*produwing the sample*).

2113. (*Mr. Saward.*) When was it laid down?—It was made in 1852, and laid down in 1853.

2114. (*Professor Wheatstone.*) To what do you attribute the softening of india-rubber when it is in contact with copper wire?—I do not think the copper wire has the slightest effect upon india-rubber; I have made miles and miles, and I never found it run unless it is a mixture of the india-rubber of East India with the Para. The sample I have produced has been made for many years.

2115. The original specimen does not seem to have been affected at all?—No; what I laid through Box Tunnel has not been the slightest affected.

2116. Do you attribute any such effect to its being an inferior material?—Bad material; I can say this, because I have used india-rubber that has never been put on to copper; that was not exposed to the sun, and was in a shady place, still that has gone wrong without the slightest exposure to the atmosphere, or without being applied to surround any metallic conductor; therefore I speak positively that it is not from any chemical effect that the copper can have upon the india-rubber.

2117. Why does it most generally occur when it is in contact with the copper, and that the other parts further removed from the copper seem to be the most solid?—I find when it does go in this way, it is generally to a certain extent at the end, where the wire is exposed to the atmosphere; probably it may be from the metallic heat; but if you cut it in two, you will find it does not go through, it only goes when it is exposed to the atmosphere in connexion with the copper.

2118. (*Mr. Varley.*) Does that which is not near sunlight or near metallic influence decay from the interior or the exterior?—From the sides.

2119. What shape was it in?—I cut it in strips for the purpose of winding it round the copper, and I found when I wanted to take some down with me for the joints in the Coast lines I am now laying, I put my hand upon this specimen, which shows the softening adverted to.

2120. (*Mr. Saward.*) Have you used preparations of oil or grease at any time?—No.

2121. Will not oil or grease decompose india-rubber?—So it is said; boiled linseed oil, I know, will affect india-rubber; but here is a piece with plaited rope over the india-rubber, dressed with a composition of a very oleaginous character. It is what is used by Peacock and Buchan to put on the bottoms of vessels; all the Peninsular and Oriental ships are coated with it.

2122. Is it not like yellow paint?—Yes; the object is to keep the marine animalculæ and insects, and also the green slimy stuff, from adhering to the ship's bottom; and I thought if the copper were preserved by the use of that material, it would also preserve the hemp itself from being touched by those insects.

2123. (*Professor Wheatstone.*) Do you produce those specimens to show that oily substances do not affect india-rubber?—I presume they do not; that was used in a very wet state.

2124. (*Mr. Varley.*) We have had a piece of wire, covered with something of that kind, hung up in our office; Mr. Clark cut it open at various places, and it is decayed throughout the whole length?—That may be.

2125. Therefore it would appear that the copper has acted upon it, and as though the copper does determine the action?—I never found copper act upon it; it must have been coated with inferior india-rubber.

2126. Is not the common sort of manufactured india-rubber liable to decompose, the same as ordinary india-rubber? Will that we use for rubbing-out purposes?—That is the East India rubber. I never found the South American decompose in any one single case. I have given attention to this matter for upwards of 14 years.

2127. You never use the East India rubber now? No, never; the East India rubber is chiefly used for vulcanizing purposes, not for insulation. Here is a piece of the Hurst Castle cable I was speaking of. I wanted to see what effect heat would have upon it; it has been made for many years. I cut this piece off, and I had it boiled for two hours, with the lid on the utensil in which it was boiled, in order to see whether the heat would have the effect of injuring it or not.

2128. (*Professor Wheatstone.*) Can you give the result of any experiments made, in order to ascertain the comparative insulating properties of india-rubber and gutta-percha?—I can only state as to india-rubber. I have not taken gutta-percha with the view of testing how far india-rubber is superior to it. I have entirely devoted my own time to my own gum. I never made any comparative experiments; it has been subjected to test, to the extent of 500 elements, by the Electric Telegraph Company.

2129. (*Mr. Saward.*) Have you the result of Mr. Whitehouse's testing of wires, manufactured under Messrs. Silver's process?—I have not; Mr. Whitehouse has stated himself that he subjected the wire

not only to test for insulation, but he subjected it to a very severe test by hydraulic pressure. He stated that he had got as much as 1,000 atmospheres, and when the hydraulic machine burst, the wire remained perfect.

2130. (*Mr. Saward.*) Are your cables covered with iron wire?—Yes, by iron wire; that is put on plaited, not wound spirally round.

2131. (*Chairman.*) Do you object to the mode of winding it spirally round?—I should think for deep water that it would be better to have no iron at all, if you can get sufficient specific gravity to sink the cable. Of course, in shallow water, or where there is likely to be any surface interference, the external covering must have some protection.

2132. Have you had any experience in paying out cables?—I have paid out cables as a sailor, having originally been in the service; but I have not had much experience in paying out telegraphic cables, save and except a short distance of my own; but in that case I used a very different method, avoiding, as I think, all this complicated machinery, which is not well understood by seamen in paying out a cable. I laid it the same as if I was going to let go the anchor.

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2133. Laying it round the windlass?—No, laying it in fakes; and the fake is run out, merely adding an ordinary stopper such as sailors would have. You may get anything that will merely compress it, because the specific gravity would be in proportion to the depth, so as to enable it just to sink quietly, and hence there would not be that heavy strain continually upon the cable that there would be if it was made of or coated with a metallic material. If what we are told is true, that the motion of the ocean is only superficial, and does not extend beyond a certain number of fathoms, below it is quiet and calm, there can be nothing whatever that can interfere with the cable when once it gets to its position at the bottom. I think there has been two miles or some quantity sent for experiment to the works, and I would take the liberty of suggesting that those two miles should be put to these tests:—for insulation under any temperature; for retention of charge under any temperature; for insulation under any temperature with hydraulic pressure, and for retention of charge under any temperature with hydraulic pressure. If you put those two miles to that ordeal, I think you will arrive at something like what india-rubber is able to do.

Mr. Thomas Barnabas Daft examined.

Mr. T. B. Daft.

2134. (*Chairman.*) I believe you have produced a very good insulating material?—I think so.

2135. What is the peculiar process that you use?—It is the perfect adhesion of the vulcanized india-rubber to the metal. I may say cohesion.

2136. Will it adhere to any metal?—No.

2137. To what metal will it adhere?—To brass, a composition of copper and spelter. I put copper where it is necessary, or I give a greater sectional area of brass, to which it will perfectly adhere, so that you may use any amount of tension without separating it. If you cut it with a knife, so as to reach the metal, it will not separate. No amount of pressure will cause water to permeate the line horizontally so as to follow the wire.

2138. The wire is in intimate mechanical connexion with the material?—Not mechanical, chemical connexion. It is far removed from any mechanical connexion, because you can strip any mechanical connexion. I have three wires in this specimen, which I propose for the Gibraltar cable; one for the Board of Trade, and another for the public. They are entirely distinct and separate.

2139. (*Mr. Varley.*) Are not you aware that in a telegraphic point of view in that shape they will not do?—No.

2140. You do not know that by the inductive action of one wire upon the other, whilst the first wire was working, the others would be entirely stopped?—I thought that when each wire was perfectly insulated and disposed in a band like that, each wire being a considerable distance apart, that there would be no practical difficulty from induction.

2141. (*Mr. Saward.*) In what manner is this applied to the metal?—The sulphurized india-rubber is placed upon the metal, the metal being perfectly free from oxidation, and then a considerable amount of pressure is brought to bear upon it.

2142. It is not passed through a die?—It is not; it is submitted to the action of steam at a pressure 40 lbs. on the inch, to produce vulcanization, and then the adhesion occurs.

2143. Are you aware whether this substance is liable to decomposition in the salt of the sea?—I am pretty sure it is not. I have put india-rubber to metal and found the metal destroy the india-rubber: it will do this unless there be perfect adhesion or cohesion. I am pretty well prepared to prove that to you. Ordinarily india-rubber in contact with metal is decomposed by it, but this specimen of perfection adhesion was made about 10 years ago, and was put into the Exhibition of 1851; it is a piece of a wheel tire, and the rubber is just so good as it was the first day it was made.

2144. (*Mr. Varley.*) What metal is it?—Brass; had they been placed together without adhesion the wire would certainly have destroyed the india-rubber whether it were copper or brass. It was said by some electricians, the difficulty will be to make the joints. We can make the joints, and you may roll it up in any way, but you will not injure it in the smallest degree; there is no danger of a kink, it cannot occur, and I think, when properly made, nothing is likely to break it. The joint is made by vulcanizing one piece of india-rubber over another, properly preparing the surfaces; it can be done on board a ship in about an hour and a half, not merely a mechanical, but a perfectly chemical joint can be made, presuming that there is steam power on board.

2145. (*Chairman.*) You believe that this material is perfectly durable in salt water?—I do; I do not say that it would last for 50 years, but I verily believe it would last 10 years or 12 years, and in deep water that it would last much longer, probably even over 50 years.

2146. Would you propose to cover it with anything?—I leave that to those who are better able to judge of the necessity.

2147. Have you tested its tensile strength at all?—No; I should add to it by increasing the size of the wire.

2148. (*Mr. Varley.*) Do you think you can ensure vulcanizing the india-rubber in this state throughout any length you may require?—Decidedly; you may have a length of 1,000 miles, if you could stow it in one unbroken length. I am sure you might stow 50 miles without joints, because you can put it where you please.

2149. Have you submitted it to any tests for insulation, or anything of that sort?—Yes; and they have been favourable.

2150. (*Mr. Saward.*) Do you think that material could be put on by a die?—No, I believe it could not. I place it longitudinally along the wire, and pass it through a machine. I can put two, three, or any quantity of coatings you thought necessary.

2151. How many coverings has this specimen upon it?—That is one sheet, rolled up; it becomes chemically compact in the process of vulcanization, it becomes as solid as the india-rubber itself. I think a cable like that would last for a great many years, would sink easily and be paid out easily.

Mr. W. Hancock. 15 Dec. 1859.

MR. WALTER HANCOCK examined.

2152. (*Chairman.*) You have paid considerable attention to the manufacture of gutta-percha?—Yes, my father was the first patentee of gutta-percha, and my attention has been exclusively devoted to it for the last 15 years.

2153. Have you turned your attention also to its application as an insulator for submarine cables?—Yes; my father was also the inventor of the machinery and processes by which all the gutta-percha wires have been insulated that have yet been made.

2154. Did you make many experiments in gutta-percha when you first took out a patent for covering cables?—We have been incessantly experimenting upon it from its introduction into this country up to the present time, and during the last 11 or 12 years, especially with reference to insulating.

2155. Have you made experiments upon its permeability by water?—We have tried it by simple immersion in water for long seasons.

2156. You have not submitted it to pressure?—We have not submitted it to any great pressure, only slight pressure.

2157. By immersion in water do you find that it imbibes water?—No; we have not found it do so; we have found that the surface gets slightly affected, but we have not found it to penetrate or apparently to penetrate more than the thickness of a piece of paper; it appears more than anything else a mere filling up of the slight roughnesses upon the surface of the gutta-percha; the molecules of the gutta-percha itself.

2158. Filled with water?—Yes; just with the moisture; so much so that it affects the surface of the gutta-percha, and makes it lighter in colour than it was previously, but it darkens again, when it becomes dry.

2159. Do you think it affects its chemical condition?—No, not at all.

2160. To what depth does the water penetrate gutta-percha?—Scarcely the thickness of a piece of paper; it appears to be a mere surface action.

2161. Even if there was no covering to the gutta-percha when it was immersed in water you do not imagine that it would be injured by water?—Decidedly not; gutta-percha under water is in the most favourable circumstances; gutta-percha is more durable under water than in any other situation, and any situation that approaches to water (as, for instance, a cold damp cellar), is the next best place to keep it in.

2162. Does light injure it?—Light and atmosphere together appear to injure it; there is a sort of oxidation goes on on the surface when exposed to the light.

2163. Do you not attribute the deterioration which it undergoes when exposed to the air chiefly to its being deprived of its moisture?—Yes, it undergoes a drying process on exposure to the air, but that drying process is much more rapid when it is exposed to light as well as air. We have found that by hanging up sheets of gutta-percha outside our office wall, the side that has been exposed to light has become quite decomposed, while the other side has been comparatively uninjured. In the course of time the back side will get injured in the same way; but that will be many months, sometimes years afterwards.

2164. Have you observed whether salt water produces any effect upon gutta-percha, which is injurious?—No, we have kept it in salt water for a considerable time, but we never noticed any deteriorating effect. There is a piece of the first cable that ever was laid (*producing the same*), that is the Dover cable. When it first came into our possession it was apparently as perfect as when it was laid.

2165. Is that made with a single wire?—Yes, it is a single wire unprotected by any outer covering; the gutta-percha was quite perfect. We have kept that piece in our office for some considerable time, and the surface is getting oxidized in that way.

2166. Have not the processes for the manufacture of gutta-percha very materially improved in the last few years?—Not nothing more than the slight improvements that have occurred in the manufacture of the machinery, and more care in carrying on the operations.

2167. You do not consider that the manipulation of the gutta-percha itself has undergone any improvement?—No; it is precisely the same as is stated in the patents that were taken out at an early period; there have been slight modifications in the machinery, and some few accessories.

2168. Is gutta-percha subject to any deterioration from adulteration, or anything of that sort, as it is imported?—Yes; we find that especially the case, of course when the price is high, and there is more inducement to adulteration; but in working gutta-percha, especially for telegraphic insulation, we select the raw blocks of gutta-percha before we begin to work, throwing out any that appear to be at all deteriorated either by age or exposure to the atmosphere, or from any admixture of a foreign material, carefully reserving those that appear to be pure and sound for insulation, using up the others for commoner purposes.

2169. What are the substances used for adulteration in the market?—The bulk of the adulteration arises from mechanical impurities mixed with the gutta-percha to increase its weight. In some gutta-percha we find other inferior gums, but we have no certain knowledge of what they are. Very likely they are found on similar trees to that from which the gutta-percha is procured. Some of the blocks of raw gutta-percha appear very sound in the first instance, but when they have been exposed for a considerable time, then there is a sort of resinous formation, and in some in a sort of laminated state, owing to the presence of some other gums. From practice, we could, of course on an inspection of 100 blocks of gutta-percha, pick out 60 suitable for insulation, and say at once that such and such blocks are unfit.

2170. Can the supply of gutta-percha be depended upon for constancy?—It would be uncertain if there were any very sudden or excessive demand for gutta-percha. The supply fell off very much about three or four years ago and continued very low for about two years; during the last two years it has been certainly larger than during the previous two or three years. Though the price has advanced from 10*d.* per lb. to an average of 1*s.* 8*d.* or 1*s.* 9*d.*, it has not brought forward the large supply which that enormous increase of price would have led us to expect.

2171. Is it the case that the trees from which gutta-percha is derived are being gradually destroyed near the coast and that consequently greater expense is incurred in bringing it down to the coast?—We have no certain knowledge. From the fact of the price being higher at Singapore and from the information that we have received, we believe that a great deal of the rise of price is caused by the difficulty of conveyance from the trees to the port; of course there are no roads and it has to be conveyed through jungle.

2172. Have any arrangements been made for cultivating the trees artificially?—No, although I believe that gutta-percha offers larger inducements for cultivation than coffee, tea, or many other products.

2173. In what climates will the gutta-percha tree grow?—It must be a tropical climate apparently; gutta-percha is collected in a very small circle, in the Malaccas, Borneo, and the adjacent islands.

2174. The trees grow naturally wild?—Yes. I believe the gutta-percha is the property of any body who takes the trouble to collect it; it is so in many parts.

2175. Have you tried any compounds of gutta-percha as insulating substances?—Yes; we have tried a great many compounds at different times.

2176. With what effect?—As a general rule, the admixture of any other substance with gutta-percha is decidedly prejudicial; but there are some materials that may be mixed with it advantageously, or slightly

so. India-rubber is almost the only material that can be relied upon, because it is almost the only material sufficiently allied in character to the gutta-percha. Cocoa-nut shell in a finely ground or comminuted state may also be advantageously combined with gutta-percha; this material is highly indestructible, and a non-conductor, and being of the same botanical family, its chemical affinities are nearly identical with those of gutta-percha. The inventor is a practical chemist of considerable experience, and he states that there is a volatile oil in gutta-percha, which, as it gradually dries away, leaves the gutta-percha dry and brittle, and that the fixed oil in the prepared cocoa-nut shell supplies the place of the natural volatile oil, and thus arrests and prevents this decay, while at the same time it gives gutta-percha greater power of resisting the effects of friction, and of extreme temperatures. We are covering a mile of wire of this cocoa-nut gutta-percha for the Committee, and we are sure that the testing of it will be satisfactory.

2177. Do you know Chatterton's compound?—Yes, I know a material that goes by the name of Chatterton's compound. It is a material that has assumed the name rather singularly, because it was already covered by a patent of my father's 10 years ago; it is in fact a compound of gutta-percha, resin, and a solvent. There is a patent of my father's in 1848, which is not *ipsissimis verbis*, but nearly in the same language. "To obtain a gutta-percha compound suitable for coating the wires of electric telegraphs, "covering the cells of galvanic batteries, and other purposes, where complete electric insulation is desirable, "or where the isolation of one substance from another "is for any reason required, I take gutta-percha, hot "from the mastinator (preferring, as aforesaid, that "which has been previously boiled in the muriate of "lime bath), roll it between heated cylinders, and "during the rolling sift thereon common resin, in a "pounded state, so as to mix the same intimately with "the gutta-percha; or I dissolve equal portions of "gutta-percha and common resin in coal naphtha, or "any other suitable solvent; or I dissolve the gutta-"percha and the resin separately, and then mix the "two solutions together." The product resulting from "any of these mixtures should be kept hot in steam "kettles, and it may be either applied to the articles to "be protected by a brush or spatula, or the articles may "be dipped in it, or drawn through it, and wound off "upon suitable revolving racks or open cradle reels." It is precisely the same thing as that recently patented by Chatterton for the same purpose; it is gutta-percha, resin, and a solvent for the purpose of giving it a little more fluidity. The solvent that my father names is naphtha, or any other suitable solvent; Chatterton's compound is also gutta-percha, resin, and a solvent. The solvent that he names is not naphtha, but tar, which is very nearly allied to crude naphtha. The object which my father had was precisely the same in any instance; he knew very well the great difficulty that would arise in the mechanical working of gutta-percha from air holes and slight mechanical imperfections,—scarcely large enough to call air holes,—with a sort of frothiness on the gutta-percha. His object was to obtain "*a complete electrical insulation*" by running any of those materials through a varnish of this kind which did not require pressure to put it on to the gutta-percha, but which merely by travelling through would, from the fluidity of the varnish, take a certain coat. Chatterton's patent of 10 years later date is declared to be for the same object, which he proposes to accomplish by the use of the same materials, prepared and used in the very same way.

2178. After this intermediate compound you could work another coat of gutta-percha?—Yes; it is fluid at a much lower temperature than gutta-percha, and it does not require to be forced through, but by passing anything through it would insure a perfect covering, and thus cover over slight imperfections which would not be visible to the eye, and possibly not visible under strong magnifying power.

2179. For the purpose of submarine cables do you consider gutta-percha to be the best insulator that can be adopted?—I believe there is scarcely a possibility of a better practical insulator, though I have a very high opinion of india-rubber.

2180. In what way would you apply india-rubber?—Certainly not next the copper, because of the strong chemical action that is going on between the copper wire and the india-rubber; but what we believe is the best way of applying india-rubber, and by which the best cable can be made, is embodied in a patent of ours of 1852, which is alternate layers of india-rubber and gutta-percha, the first coating next the wire being gutta-percha, then a coating of india-rubber round that, and then one or two exterior coats of gutta-percha; by that means you get all the mechanical strength of the gutta-percha and you get the elasticity and accommodativeness of the india-rubber; in fact you get a very beautiful result.

2181. (*Mr. Varley.*) Can you make these different coatings unite perfectly?—Yes; the mere fact of passing through our patent covering machine gives sufficient heat to unite them perfectly. If the india-rubber is carefully cleaned, and laid on freshly made, there is a sufficient heat to unite them into a perfect tube. There are minor details to be attended to.

2182. (*Chairman.*) You do not put the india-rubber spirally?—Either spirally or longitudinally.

2183. Which do you prefer?—We scarcely have a preference for either; we sometimes prefer two coats putting one longitudinally and the other spirally. If we use two longitudinal coats, one joint would come on the upper side and the other joint on the lower side, so that the joints would be broken; there would be 180° distance.

2184. (*Mr. Varley.*) Can you make perfect unity between india-rubber and gutta-percha?—Yes.

2185. At the present time the great difficulty has been found in uniting the gutta-percha with the india-rubber, in this way, that we never have been able by the ordinary process, which would stand an electrical test in working the line, to join the india-rubber to the gutta-percha, so that *each* failed?—There is some little difficulty in it; it requires some little care to thoroughly join india-rubber to gutta-percha. With a joint made in this way, which would require to be carefully made (*producing a piece*), there would be a chemical effect produced which would assist the joint to a very great extent and fill up any possible interstices between the one layer and the next, or the one edge and the next. We have another plan by which we could put india-rubber on mechanically between two coats of gutta-percha. We are carrying on some experiments; I have mentioned to Mr. Latimer Clark two or three, and I believe he has them under consideration. We put three coats, one gutta-percha, the second india-rubber, and the third gutta-percha.

2186. (*Chairman.*) Have you any specimens of your covered wire?—I have only two little pieces, which I happen to have in my pocket, that we were testing upon to-day. These two pieces we find are exceedingly good; there are in this sample two coats of ordinary gutta-percha without any intermediate compound, either Hancock's or Chatterton's, upon the wire, but the compound is between the gutta-percha, not precisely the same as the original patent of my father or the present patent (Chatterton's); that wire we find tests very well indeed. We have tested it upon the Reid galvanometer, which is the one that was used by the Atlantic Telegraph Company, and also upon our own galvanometer, which is five or six times as sensitive as the Reid instrument. The battery used was a newly mounted one, and although only 250 pairs of plates, it shewed upon testing a greater intensity than a battery of 500 that had been in ordinary use after being set up about three weeks. The test of a half mile length of this No. 4 guage wire was as follows:—On Reid's galvanometer, charge 10°, mean defect 8°, on our more sensitive galvanometer charge 55°, mean defect 22°.

2187. (*Professor Wheatstone.*) Have you made the

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same experiments with ordinary covered gutta-percha wire?—Yes; in this instance I took one mile of copper wire, and covered it to No. 10 gauge in one length. I then put it into the machine again, and run one half mile through in the ordinary way, and the other half mile with the addition of my intermediate compound. With this little alteration the comparative result upon our excessively delicate instrument was as 32° against $2\frac{3}{8}$°.

2188. (*Mr. Varley.*) Were the experiments tried at the same time?—Yes, and after they had been immersed for three weeks in water; this test was made last evening after the batteries had been freshly mounted yesterday, in order to put them to a very severe test. I do not believe that the difference is so great as it appears to be. I think there must have been in that half mile that gave 32° some one or more very slight mechanical imperfections, such mechanical imperfections as do occur in the covering of long lengths of wire; but still it shows very fairly that there is a considerable advantage to be gained by the use of our patent intermediate coat, as well as the double covered wire. We are now covering three miles for Captain Galton, and it is to be covered ultimately to the seven-sixteenths of an inch to a sample supplied by Mr. Latimer Clark,—with four coats of gutta-percha. We are covering this with some chemically prepared gutta-percha, which is a sort of medium substance between india-rubber and gutta-percha; it has far greater flexibility and elasticity than gutta-percha, but considerably less than india-rubber. The object of this chemical treatment of the gutta-percha is to render it more durable; it has also been found to increase its insulating power at the same time. The samples now produced are two first coats of this wire to No. 6 gauge. The testing of these two first coats in the half mile lengths gave upon our sensitive instrument $1\frac{1}{2}$°, $1\frac{1}{3}$°, and $1\frac{1}{8}$° respectively; two half miles, when joined, gave $2\frac{1}{8}$°, and the three half miles together gave $3\frac{1}{4}$°; another length was run through, not at the same time, but precisely of the same material; 1,000 yards gave seven-eighths degree upon our sensitive instrument, and another 1,000 yards gave five-eighths degree; the two lengths in one gave $1\frac{1}{2}$° mean defect. I think that is exceedingly satisfactory, because this is two sizes smaller than No. 4, No. 4 being the smallest size that is generally used for land wire; and this is to have two further coats of gutta-percha. We have tested it merely out of curiosity, and we find that while the half mile of No. 4, before mentioned, gave $2\frac{3}{8}$°; the half miles of this chemically prepared gutta-percha, although two sizes smaller, gave an average of about $1\frac{3}{8}$° only.

2189. (*Mr. Varley.*) You are inclined to think that the specimen that gave 32° was partially defective?—I think it must have been.

2190. You have not compared it against any other portion of ordinary manufactured wire?—We were covering some ten miles at that time, and the other lengths were somewhat better; they were for an order, and we were obliged to send them away.

2191. Have you any comparison between those three half miles and the others?—Only after a short submersion, when they averaged from 10° to 15° instead of 32°.

2192. (*Mr. Saward.*) How does a mixed cable composed of alternate layers of india-rubber and gutta-percha compare in price with other descriptions of cable made wholly of gutta-percha?—It will be higher in price, of course, depending upon the proportion of the two materials; the proportionate cost of india-rubber to the same quantity of gutta-percha will be at present about 25 per cent. higher than an equal quantity of gutta-percha.

2193. Is the process of applying it more expensive, as well as the material?—Yes, the process of applying it is slower, and much more expensive. I have two or three specimens of cable that I should like to submit to the Committee. One of the great defects of the gutta-percha cables is, that in hot climates the copper wire has a tendency from a slight increase of temperature, or from any accidental bending there may be upon it, to protrude through the gutta-percha. My father has a patent for coating the wires afterwards with one, two, or more layers of india-rubber cloth, which we can unite thoroughly to the gutta-percha, and each layer will slightly increase the insulation, with this great additional advantage, particularly in hot climates, that these coatings would in many instances save the cable, because in the event of any accident by which the copper wire protruded through the gutta-percha, it would protrude only as far as this outer coating; the india-rubber cloth is put tightly round the gutta-percha, and though in the event of such an accident the cable would not be so well insulated in that part, it would be still sufficiently insulated to work, instead of being completely destroyed. This coating of india-rubber, or india-rubber cloth, may be put between the coverings of the gutta-percha, if considered advisable, as already mentioned.

2194. (*Chairman.*) Do you use the india-rubber cloth, instead of hemp serving?—You might adopt it either with the hemp serving, or without. The main advantage is, looking at the probability of the supply of gutta-percha, falling short we shall economise the gutta-percha; the cable might be made of thin coats of gutta-percha with one, two, three or more coats of air-proof cloth. I think this specimen is covered on the one side with about eight different layers, so that all the advantages which we derive from uniting a great many different coats of gutta-percha are attained, by putting two or three of those layers upon the gutta-percha; you may have two coats or 20 coats of india-rubber, each very thin; this material is perfectly waterproof; it is called air-proof and water-proof cloth.

2195. (*Mr. Varley.*) Have you tested that cloth electrically?—Yes.

2196. (*Chairman.*) Would not water penetrate through the joints?—Water would penetrate through the first coat, possibly the second, but it would never pass the air-proof cloth.

2197. (*Mr. Varley.*) Not even at the bottom of the Atlantic?—I think not.

2198. (*Mr. Saward.*) Is this cable expensive?—Yes; the process of making air-proof cloth is expensive, but one of the plans named in the patent is giving, perhaps one coat or two thin coats of gutta-percha, then a coat of solid india-rubber, and one or two coatings only of this air cloth; there would be an exterior stouter cloth to give strength; the cost would resolve itself the same as in a gutta-percha cable to the amount of insulation required, and the amount of outlay that was desirable. One of the advantages of air-proof cloth is that you can have it solid, or in six or eight different coats.

2199. (*Chairman.*) What would be the expense of this outer covering compared with other cables?—We have not gone into that question fully yet.

2200. Have you apparatus by which you could cover cables in this way?—We are fitting it up for making small quantities.

2201. Have you tested this specimen for longitudinal strength?—We have not put any breaking strain upon it yet. I made that simply with a view of trying it; the strength would depend entirely on the fabric used. Sail cloth is about the strongest fabric we could use for the purpose.

2202. (*Mr. Varley.*) Would not this semi-conducting compound between the two reduce your speed of transmission?—In any case where insulation is desirable, we do not propose to build it up entirely of this cloth, but to put a coating of rubber, or of gutta-percha, or of both, and a coating of cloth, so that the cloth would give strength to the rubber. As I have already said, another object we have in view is to meet the probability of the supply of gutta-percha falling short in case of an excessive or very sudden demand.

2203. Have you ever noticed in any case that gutta-

percha in contact with india-rubber is very liable to decomposition, to re-act upon the gutta-percha and decompose it?—No.

2204. A great quantity of wire was laid down by the South-Eastern Railway Company, which went bad almost immediately after it was put down; there were a great many wires, and the copper wire was covered with india-rubber, and that again covered with gutta-percha?—It was the copper wire that decomposed the india-rubber, and the india-rubber was no doubt of low quality, and if so, it would have perished of itself.

2205. After an interval of two or three years, the rubber became so fluid, and the gutta-percha so bad, that the wires were left in hollow shells of gutta-percha?—This was done by some parties who never covered any gutta-percha wire before; it was their first attempt, and a very unsuccessful one.

2206. I have seen other compounds of gutta-percha and india-rubber, which have failed in the same way?—The most perfect specimens we have are those compounds of india-rubber and gutta-percha; the fact you have mentioned cannot have happened with good india-rubber by itself, it must have been inferior or damaged.

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2207. Some of the gutta-percha joints that have been covered with india-rubber in a tunnel on the Brighton line have been very recently cut out, and the rubber outside was perfectly good, and the gutta-percha also, but these other wires on the South-Eastern line went bad?—They were placed in tarred boards, if I remember rightly; crude tar is a solvent both of gutta-percha and india-rubber.

2208. Gutta-percha has stood under similar circumstances in boards, treated in the same way without india-rubber?—That is very likely; gutta-percha is not so susceptible of injury as india-rubber is. The wire you mention as used on the South-Eastern line would, from imperfection in its manufacture, no doubt, have failed under more favourable circumstances. I have heard of other small lots of wire covered by the same parties failing in the same manner.

2209. By whom was it covered?—It was covered at a small factory, that had only an ephemeral existence, called the Lambeth Gutta-percha Factory; it has ceased to exist for many years.

Adjourned to To-morrow at Three o'clock.

Friday, 16th December 1859.

PRESENT:

Captain Douglas Galton.	Mr. Saward.
Professor Wheatstone.	Mr. Varley.

Captain DOUGLAS GALTON in the Chair.

Charles Vincent Walker, Esq., F.R.S., F.R.A.S., examined.

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2210. (*Chairman.*) Are you a telegraphic engineer?—Yes, to the South-Eastern Railway Company.

2211. You have been connected with that company for a great many years, I believe?—Since the erection of their telegraph from London to Dover, upwards of 14 years.

2212. Have you had very considerable experience in electric telegraphy?—As far as the South-Eastern Company is concerned, and with respect to land lines.

2213. You have turned your attention to some extent to submarine lines, have you not?—To a certain extent.

2214. Have you had much experience of gutta-percha covered wires?—Yes, underground and in tunnels.

2215. What has been the result of that experience with respect to the durability of the gutta-percha?—Not very satisfactory.

2216. To what do you attribute the failure; to the fact that the gutta-percha was not a good material, or that the positions in which it was placed were such as to cause it to decay?—I have no reason to think that the decay is due to anything else than to the nature of the gutta-percha itself.

2217. You have not a good opinion of gutta-percha as a permanent insulator?—On land, I have not. Take cases in point. I was the first to use gutta-percha in England. I advised Mr. Forster of Streatham to apply it in our very early difficulties in telegraphing. We purchased and we used the first wire covered with gutta-percha, on November 11, 1848; after a certain time it became bad, and failed; and I felt that the failure was due, either to Mr. Forster's then inexperience in selecting it, or to the inexperience of the owners of gutta-percha generally. It was a new thing. That was our early experience; and lately, as a particular case:—you are aware that the South-Eastern Railway passes along a viaduct the first two miles out of London. About five years ago, or rather more, up to the time we repaired it, we placed some 29 of the Gutta-percha Company's best wires underground; they were laid down with perfect success. I myself, or my chief man, witnessed the whole work; and the wires were perfectly good at the end of the work, and stood the test well; but, in the course of three or four years, they began to show very great signs of leakage. We repaired them at the end of five years, and we found in them a new defect that had not presented itself heretofore or elsewhere. The old defects, with which we were too familiar, were a dryness and decay on the outer surface. The case in those viaduct wires was a decomposition, commencing on the copper wire itself. A gummy substance formed round the copper wire itself, and oozed out in small spots continuously along much of the whole length.

2218. Do you attribute that to chemical action going on?—It was apparently a change in the character of the gutta-percha in contact with the wire, and not connected with the action of electricity, because one wire not working behaved in the same bad way.

2219. Were not those wires buried underground?—Yes.

2220. Were they insulated from moisture?—They were in square iron troughs with light shifting lids, made on Reid's patent.

2221. Was the earth round them damp?—The earth was damp; it was not what one would term a wet soil, because it was on a viaduct; the troughs were often full with water—sometimes, in the dryer weather, the troughs got dryish. The outer surface of this gutta-percha presented all the appearance, as it does now, of perfect goodness.

2222. Were its insulating properties injured?—Certainly.

2223. As a covering for subterranean wires, you do not consider gutta-percha to be good?—I should hesitate in laying out large sums on gutta-percha for subterranean purposes. If the decay had confined itself to the surface, one might expect to find means of protecting the surface; but when as in the case

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to which I have referred, the decay commenced internally, the surface remaining perfect, my faith, I must confess, was much shaken. I am now pressing upon the Astronomer-Royal to communicate from the Observatory to our railway, if possible, overland, and not underground; and he has made arrangements for that purpose. His buried wires are very bad, almost *hors de combat*.

2224. Are the wires which you have suspended in your tunnels covered with gutta-percha?—Yes.

2225. Are those equally bad?—Their behaviour is of a different kind; as a rule, the misbehaviour commences from the outer surface cracking.

2226. The tunnels on the South-Eastern Railway are not damp, I suppose?—Some are very damp indeed.

2227. In those tunnels is there equally this cracking of the gutta-percha?—Equally so.

2228. Then the conclusion at which you arrive is, that gutta-percha does not answer where it is alternately subjected to wet and dry?—In either of those cases it does not answer as a perfect insulator for many years.

2229. If it is alternately subjected to moisture and dryness?—I can hardly say alternately; I should infer that some of those places are always wet.

2230. We have seen several yards of submarine cable which have been down a long time, in which the gutta-percha is perfectly good?—Under water gutta-percha has answered admirably. I have a piece of common hempen rope with two wires in Deptford Creek, on the Greenwich Railway; we tested them a very few weeks ago, and they are all perfect at this moment; they have been down since January 6, 1853.

2231. (*Professor Wheatstone.*) What is your opinion of india-rubber as an insulator?—I purchased some india-rubber wire immediately it was for sale, and I placed it in the Martello tunnel at Folkestone, on May 12, 1859; I have not looked at it since it was there.

2232. (*Chairman.*) Is it a damp tunnel?—Very damp. All the specimens of india-rubber wire that I received from Messrs. Silver, and which are in my office, without exception show decay next to the wire, as though a gummy matter was oozing out round the end of the copper wire. That is not the case with a piece of india-rubber covered wire that I possess, and that was sent to me many years ago. It is labelled in my collection, "Sample from Billings "and Co., Oct. 27, 1848."

2233. Is that pure bottle india-rubber?—It has the same outward appearance as Messrs. Silver's rubber-coated wire.

2234. Messrs. Silver's wire is covered with a sort of masticated india-rubber, is it not?—Yes.

2235. (*Professor Wheatstone.*) There are specimens of wire which I have seen covered with pure india-rubber which do not seem to have undergone that decomposition?—I have one specimen at home which I think is pure india-rubber, sent to me from some other quarter lately. It looks well as yet.

2236. (*Chairman.*) Have you made any experiments on the relative insulating properties of gutta-percha and india-rubber?—No, I have not, beyond the ordinary testing.

2237. You only use these gutta-percha covered wires in your tunnels and in your underground work?—With here and there a river or stream. We have had a couple of cables underneath the river Stour, near Sandwich, since August 30, 1853; they are perfectly good; we have a couple of cables under the harbour at Folkestone, which have been down several years, and are perfectly good.

2238. Are those embedded in the mud?—They are embedded in the mud. We have also some wires under the river Rother, near Rye, on a somewhat different arrangement. Long before cables were known, in May 24, 1851, I made a frame of timber, a horizontal baulk for the bed of the river, and two vertical baulks on either side; they were cut open and grooved, and then bolted together, with a wire in each groove. Several wires there, after having been under water for some considerable time, went bad.

2239. What was that from?—It may have been inferior gutta-percha; it was early wire put down before cables were devised. The parts nearer the top of the vertical grooves were chiefly bad.

2240. Do you consider that gutta-percha has improved materially in character since so much attention has been paid to it for submarine cables?—I think what we have had from the Wharf Road has been very perfect in its character. But I ought to mention that I have eight of Mr. Forster's wires at this moment under Folkestone Harbour that have been long out of use, and we have within the last few weeks tested them; they have been down since September 11, 1850, and some of them are in a good condition. They are simply laid roughly in old engine boiler tubes, and in a trench. I am using one or two of them for some new wants; we have one or two leaky and bad, one or two tolerably good, and some three or four very good, but not perfect.

2241. Have you found your subterranean wires subject to injury from atmospheric phenomenon, from lightning?—We almost always expect injury after a storm; but not detecting any special spot where you could see evidence, I should hardly like to say. It is a thing extremely probable. We know in the short piece under the river Rother one case of the wire being perforated during a storm.

2242. Can you give the particulars of that case?—It was a violent thunder-storm which visited the county of Sussex, and a certain wire failed, "found earth" as we call it; and on testing we found where it was, and when the wire was drawn out subsequently from the river Rother, we found the gutta-percha pierced.

2243. Was it injured in this way (*handing to the witness a piece of core injured by lightning*)?—There was no symptom of this fused matter outside; it had more the appearance of being pierced by a brad-awl, as if the water had chilled any tendency to fuse. I have a similar specimen to yours with lumps of melted gutta-percha, taken from Plumstead station after a violent discharge of lightning, inside the office.

2244. Did you take any precautions against that on each side of the river Rother by lightning conductors, or any means of diminishing the chances of accident from that cause?—We have a small lightning conductor in many of our telegraph stations attached to each wire as it enters the instrument.

2245. Is that for the protection of the instrument?—That would protect this piece of wafer, being near the station. At Rye there are these lightning conductors.

2246. If the lightning struck the wire on the opposite side of the station it would still be subject to injury?—Yes.

2247. You have not thought it worth while to take any special precautions?—No.

2248. I was induced to put that question from the fact that we have been told that it was suspected that the Corfu cable has been injured by lightning?—It is not at all improbable, for the facility for escape is so great under water for a flash of lightning.

2249. Have you made any experiments on retardation in underground wires?—I made some on the Atlantic cable while it was lying in part at Keyham yard, and in part on board the "Niagara" and the "Agamemnon."

2250. Was the cable then coiled?—Yes.

2251. Had you made any experiments before on any underground wires?—On different lengths of cable at Greenwich; or, rather, I have witnessed and assisted in experiments making by Mr. Whitehouse.

2252. Were those on the Atlantic cable?—Yes; I visited Mr. Whitehouse when he had the Atlantic cable, and saw some of his experiments. I cannot say that I made them; I went as a visitor to see anything that he had to show, and I possess some of the results.

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2253. What were the experiments that you made at Keyham?—I made some of various kinds; upon the time, for instance, that elapsed between a current's entering the near end of the cable and its appearance at the far end.

2254. Will you describe generally the character of the experiments?—The retardation in 2,300 miles of cable, with 24 cells of the sawdust battery; 1·78 seconds of time in arriving at the further end. Here is another experiment on the same length of cable, but with 288 cells: time, 1·8 second; nearly the same, in fact, as the other.

2255. In fact, you found very nearly the same result whatever the battery power was?—Whether with 24 cells, or a much larger number of cells—that accords with Faraday's results published long previously; I was able to discharge the cable and get a second reverse signal through in six seconds after the appearance of the first; that made about 15 currents per minute through the wire. Here is another comparative series of the same length, 2,300 miles of cable. The induction coils in use, and two cells of a battery for the primary wire, gave 37 currents per minute; four cells of a battery, 44 currents per minute; six cells, 49 currents; and with six cells the maximum obtained was 65 currents per minute. The cells were used to excite the primary wire, in the ordinary way, of the large induction coils that were used.

2256. What size were those induction coils?—I do not know the precise dimensions from memory; they were Mr. Whitehouse's large coils.

2257. What he worked with after the cable was laid?—Yes, a mile and a half of No. 14 wire, in 12 lengths, was the primary coil, I think, according to my notes.

2258. With the induction coils you got a very considerably greater speed than with the mere battery?—Yes; and to a certain extent an increase of speed is obtained with adding a few cells, but the limit is soon apparently reached.

2259. Do you consider that the cable was injured at all by the strong current sent through by means of the induction coils?—I have no reason to think so.

2260. Did you test the cable after it was laid?—No, not afterwards. I was led to test it for leakage as it lay in coils in Keyham yard, and in part on board ship, not under water.

2261. How did you make the test?—I adopted this system: I placed a galvanometer between the battery and the cable, the far end of the cable being on the earth in the first instance, and then the far end of the cable being off the earth in the second instance.

2262. (*Professor Wheatstone.*) The circuit remaining complete?—Yes, in the first instance. Again, in a third arrangement, the galvanometer was taken from the home end to the far end. The values were obtained from reading off the number of oscillations made by the galvanometer needle in a given time.

2263. Can you give the results?—With 2,200 miles of cable, four-fifths of a given current leaked out when the far end was free. Three-fifths reached the instrument at the far end when the instrument was put at the far end for taking signals.

Here is another case with 400 miles of cable; this was a new length that had just arrived, and in very good condition. With this 400 miles of cable, more than nine-tenths of the force reached the far end as signal force. The leakage was less than one-tenth in that piece when the far end was free.

2264. You have stated that you had observed oscillations; what formula did you use, because upon that the accuracy of the result would depend?—The formula will be found in De La Rive's "Treatise on "Electricity," vol. I., p. 187. The galvanometers were what I found at Keyham. I should have prepared a different galvanometer. These are approximate results, and must not be taken as mathematically correct.

Here is one case of 554 miles, of which 400 were the new and good cable mentioned above, and 154 older cable, known not to be so good. When the best piece of cable was at the home end, the signal force at the far end was 0·755; therefore a leakage when the circuit was complete of 0·245; leakage when the far end was free 0·798. The first two figures make the unit of the whole force; that is to say, the total force which enters the cable when the circuit at the far end is completed, by connecting that end direct with the earth. When the new 400 miles of good cable was at the far end, the signal force that reached the far end was only 0·485; leakage, therefore, when the circuit was complete 0·515; but the leakage when the far end was free amounted to 0·937.

2265. (*Mr. Varley.*) In all the cases you have referred to, the unit is the force of current passing when there was no other instrument in circuit, I presume, but the galvanometer and the battery?—And the cable.

2266. (*Chairman.*) Are there any other results that you could give the Committee?—Here is a case of 992 miles, leakage 13 per cent. The entering current, namely, the current that enters the wire when the circuit is complete is the unit to which I refer. [*Vide* Supplementary note A.]

2267. (*Mr. Varley.*) After the battery had been on a considerable period you did not take the rush in for the charge?—These are not experiments upon "rushing in" or upon "charge."

2268. (*Chairman.*) In every case you found that the wire leaked considerably?—Yes; and in all cases coated wire leaks to a certain small extent. For specimens of good insulation, see some of the figures in the last Column of the Table in note B.

2269. To a very considerable extent in one or two of the cases you have mentioned?—The remark I made on this point, in the Report that I sent in to the directors of the Atlantic Telegraph Company, and which Mr. Saward, their secretary, has promised to place before this committee, that it may be printed with this evidence, is: "I was hardly "prepared to find the leakage so high." I have various other results in my note-book not worked out. I have heretofore scrupulously avoided publishing anything in connexion with this cable, from the unpleasant things that were notorious after the cable was laid. I rather preferred not mixing myself up with any of the correspondence that was going on."

2270. What was your general opinion of the cable after those experiments at Keyham?—That the cable was very leaky.

2271. Were your experiments such as to lead you to anticipate that if laid, the cable would not be able to work successfully for any length of time?—If the cable had been in the ocean as it was on shore, there was a sufficient per-centage of current passed through to work it. I have named how much percentage passed through in certain cases. I think my Report, when printed, will furnish materials for an answer to that question.

2272. Did the result at which you arrived lead you to the conclusion that the Atlantic cable was the best form for a submarine cable which could be devised?—No.

2273. What form would you have preferred; for instance, what proportions would you be inclined to select between the conductor and the insulating covering, what size of conductor would you prefer?—I do not think any records of comparative experiments have been given to us between conductors and cores of relative sizes. I use the word "core," which represents the gutta-percha and the wire. I had no opportunity then of comparing any two cores together; I had only the Atlantic cable before me while at Keyham. I have a few comparisons of experiments made elsewhere and subsequently, which advance us the first step in the inquiry; but I had no opportunity nor means at that time of extending them, so as to carry on the inquiry through the further stages, and onwards to the last step.

2274. What were those comparisons?—They are comparisons between various cores. In all the cores, the outer diameters of the gutta-percha were the same

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as the Atlantic cable core, 0·375 in.; they were not covered with iron wire outside, nor with anything; they were simply cores lying in the canal at the Wharf Road.

2275. Are those comparisons between different lengths of the core?—No, they are comparisons between nearly the same length of various cores. I think they are the only experiments that have been made in a series. No. 1 was 1,880 yards in length, and contained three No. 22 copper wires, each distinct and insulated from the others. The charge as it is technically termed, as manifested by the deflection of a galvanometer needle when the current entered, and the far end of the cable was free, was 42°. I will take another one as a contrast. No. 4 on the list: this was what we now call a "Chatterton" wire, containing a compound next to the copper wire, and also a compound between the different layers of gutta-percha. (*Vide* Sup. note B.)

2276. How many layers of gutta-percha had the first wire that you experimented upon?—I believe they were the same in number as in the Atlantic cable core; with which core, as I have said, they all corresponded in external diameter.

2277. (*Mr. Varley.*) And number of coatings?—I think, the Atlantic cable had three.

2278. (*Chairman.*) What sized wire was this No. 4 experiment?—No. 4, the Atlantic strand; the strand was composed of seven No. 22 wires; 0·125 in. was the diameter of the strand.

2279. Giving, therefore, seven times the conducting surface?—Not seven times the surface; four times only. Experiment 1 was one wire in substance; experiment 4 consisted of seven No. 22 wires in a strand, 1,760 yards long; and the charge in that case gave 52°. I will now take one of Mr. Hearder's arrangement, that is, covering the conducting wire with cotton or worsted, and then covering it with gutta-percha.

2280. What were the conducting wires in this case?—The Atlantic cable conductor again. I will take one of the same length, No. 9, 1,760 yards, covered with double worsted before the gutta-percha was put on; the charge in that case was 95°. Those will give three comparisons of the results that are obtained on taking this first step in that inquiry.

2281. The first wire you mentioned was a smaller conducting wire, and the charge was very much less. Was that covered in a different way from the second case?—No.

2282. I thought you said in the second case, two coats of Chatterton's compound were used?—Yes, but I can give you the Atlantic core without Chatterton's compound as the fourth case. No. 5, Atlantic cable, 1,760 yards, charge 55°.

2283. That would compare with the first case that you gave, in which the only difference was an increase in the size of the conducting wire?—An increase in the size of the conducting wire, which implies a greater surface of gutta-percha that is presented to the conducting wire. In the case of Mr. Hearder's wire covered with cotton and worsted, you have the same conducting wire as in the Atlantic cable, but you have a larger inner surface of gutta-percha presented to it, because the worsted is necessarily between the two.

2284. What are the conclusions which you draw from those experiments?—I have said they take us on but a single stage in the inquiry. The experiments were made with great care; and it is evident on the first inspection that the larger the surface of gutta-percha presented to the wire, the greater the charge. The next question is whether a larger wire discharges the surface with greater rapidity or not.

2285. Have you made any experiments upon that point?—I had only a mile of each variety for examination, and a mile is too short a length from which to read time; 1·8 seconds for 2,300 miles would give us no means of reading the minute portion of time occupied by a current, however much retarded, in traversing a mile.

2286. (*Mr. Varley.*) Did you form any opinion as to what would be the result of using a thick wire, compared with the use of a thin wire?—I have said the results here recorded are gathered from advancing only one stage in the experiment; it was impossible here to take the question of time and retardation.

2287. Have you formed any opinion upon that?—That is quite another question. I prefer to publish my opinions in connexion with facts. I have studiously avoided, during the whole progress of the Atlantic cable controversy, putting forth my opinions, especially when facts were few from which conclusions might be deduced, because I did not desire to expose myself to the kind of attacks that had been going on in the public papers so long in connexion with the Atlantic cable. I am neither willing, nor do I approve of discussing scientific questions, as this important question was discussed, in the public papers. I am willing to give results, and I shall be happy to assist the Government with all the information in my power, but I am not ambitious of attacks in the public papers.

2288. Are you aware that Mr. Hearder thinks that the interposition of a semi-non-conducting material between the insulator and the conductor will materially modify the rapidity of charge and discharge, and that he anticipates by such a medium to greatly increase the speed of the working of the cable. He has published that in a pamphlet. Do you think that your experiments justify such conclusions in any way?—I should hardly like to form an opinion without seeing the results to which Mr. Hearder has arrived by actual experiment. As such semi-conductor was not present in my experiments, they can give no evidence one way or the other.

2289. (*Chairman.*) Have you not seen Mr. Hearder's pamphlet?—I scarcely know to which of his papers reference has been made. I have seen, from time to time, various letters of Mr. Hearder's in "The Engineer," and in the "Philosophical Magazine." I have five of Mr. Hearder's wires in this list, with the various charges. The figures are 70°·5, 70°·5, 85°, 95°, 100°.

2290. (*Mr. Varley.*) The Atlantic core covered with these different materials?—Yes; and all, as I have said, within a few yards of a mile in length.

2291. Your experiments would clearly seem to indicate that the inductive absorption is very much larger in these wires than where gutta-percha is used instead of Mr. Hearder's material between the wire and the insulator?—Yes; the larger the wire, the greater the absorption of electricity as it enters, and the larger the surface of gutta-percha presented to the wire.

2292. (*Chairman.*) Where Chatterton's compound was used next to the wire, did not you find the absorption of electricity rather less than in the case where gutta-percha was used next to the wire?—There were a few degrees of difference, but that is mainly due to the better insulation of the two miles in question. The leakage of one was only 2°, the leakage of the other was 6°, which leads to the same result, within one degree.

2293. (*Mr. Varley.*) What was the leakage in No. 1, in which there was a single No. 22 wire?—3°·5.

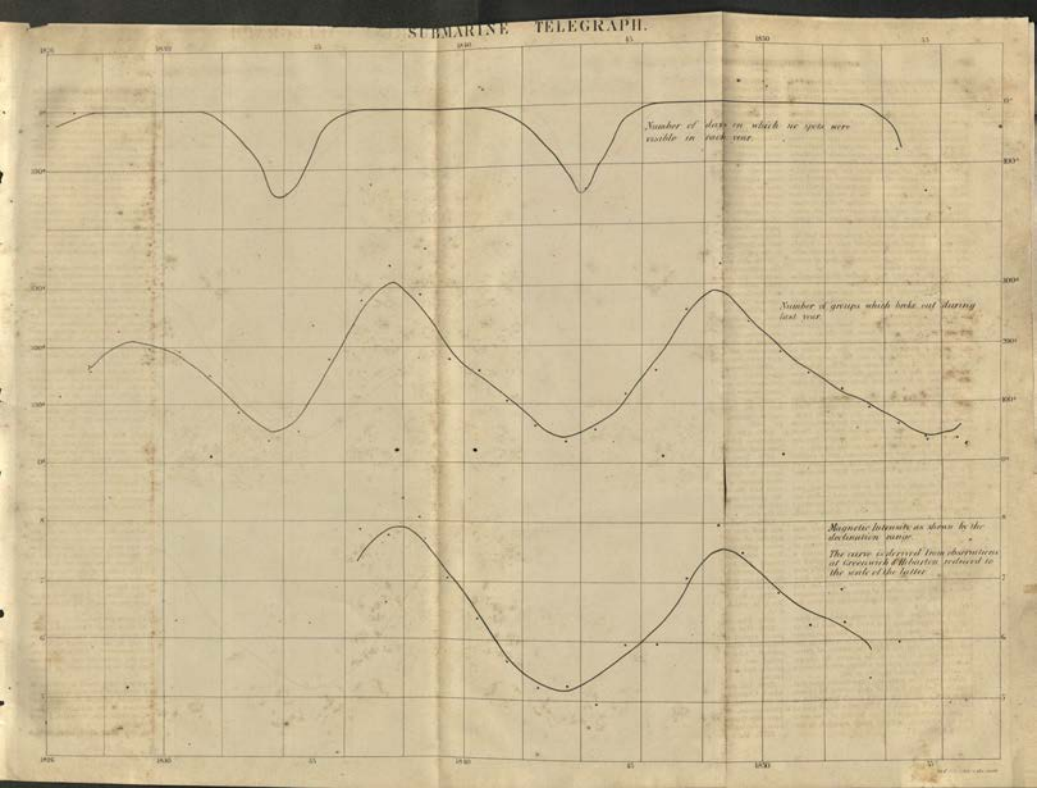
2294. That was all gutta-percha?—Yes.

2295. Had any of these wires Chatterton's compound applied next to the wire, or was Chatterton's compound applied only to the exterior?—No. 4, marked "Chatterton's compound;" Chatterton's compound was between the conductor and the gutta-percha, and between the respective coats of gutta-percha.

2296. Did you try any experiments upon wires to which Chatterton's compound was not applied next to the strand, but applied between the coatings of gutta-percha, or applied to the exterior only?—I have only the one Chatterton wire, to which I have been referring throughout.

2297. Have you any other sized cores besides

SUBMARINE TELEGRAPH.


Number of days on which the spots were visible in each year.
Number of groups which broke out during last year.
Magnetic Intensity as shown by the declination range.
The curve is derived from observations at Greenwich & Hobarton reduced to the scale of the latter.

those you have mentioned, namely, the Atlantic and the seven No. 22 wires?—Mr. Whitehouse's conductor was formed of one of three No. 22 wires, and the other conductors were all, as I have said, the Atlantic strand of seven No. 22.

2298. Did you try any of Mr. Hearder's, in which there was a semi-non-conductor between the two coatings of gutta-percha, but not between the gutta-percha and the wire?—No; I tried cores containing single cotton, single worsted, double cotton, double worsted, and double cotton and worsted close to the wire; not between the layers of gutta-percha.

2299. Did you find with the increase in the thickness of worsted and cotton that the charge increased?—Single cotton 70°·5; double cotton 85°; single worsted 70°·5; double worsted 95°; cotton and worsted 100°.

2300. (*Chairman.*) Was the cotton interposed between the wire and the gutta-percha in one case?—In all these cases it was next the wire, both the cotton and the worsted.

2301. (*Mr. Varley.*) Did you use a Daniell's battery in these experiments, or a sand battery?—504 sands.

2302. Do you think they kept their power up pretty uniformly during the experiments?—I think so.

2303. In your experiments on the Atlantic cable did you try to ascertain at what speed signals could be transmitted through the cable compared with the transmission of simple waves?—I have some records at home of letters and messages that have been sent, but I worked chiefly for waves or currents.

2304. Waves and Morse letters?—Yes, when I tried for letters.

2305. Have you any records as to the relative speeds, and the amount you have to deduct from the speed of a wave to get the speed of transmission of letters?—None.

2306. (*Chairman.*) You have had some specimens of wire, in which india-rubber and gutta-percha were mixed together, laid on the viaduct of the South-Eastern Railway, leading out of London?—We had a little that came to us from Mr. Bunn; it was rather a mistake than otherwise, and it was very unsatisfactory.

2307. What sort of mixture was that; were they alternate layers?—It was a layer of india-rubber next the wire, and then gutta-percha.

2308. Did the india-rubber decompose?—Yes. It was evidently attached by the aid of solution. Therefore I do not think any conclusion of any value could be arrived at from the behaviour of that specimen. It was not a well manufactured article; it was not such as Messrs. Silver would send us, as made by their modern processes.

2309. (*Mr. Varley.*) You do not think that india-rubber and gutta-percha in contact produce any action that is likely to endanger the insulating power of either of them?—There is nothing to lead me to suppose that. (*The witness here referred to a diagram given in his report to the Atlantic Telegraph Company, which shows the correspondence between the curves of sun spots and magnetic storms.*) (*Witness.*) There seems to be little doubt that a large wire will be more affected than a small wire by magnetic storms. We are now at the apex, or nearly so, of the curve, and we shall be passing downwards into a more quiet period.

2310. (*Chairman.*) Where are your experiments being carried on?—In the counties of Kent, Surrey, and Sussex. I am working out the resultant of the current that would be acting upon the magnetometer at the Royal Observatory at the time our telegraphic needles are disturbed. I think I shall be able to lay down, within a very few degrees the direction of the current. There are magnetometers at Greenwich, and I have cases in which they have been deflected in the right direction to correspond with the particular current that I have laid down from various elementary currents.

NOTE A.

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MODE by which the LEAKAGES of the ATLANTIC CABLE were determined.

1. The formula employed in obtaining the comparative values of the leakage and the available force, is—

$$\frac{m'' - m}{m' - m} = \frac{n''^2 - n^2}{n'^2 - n^2},$$

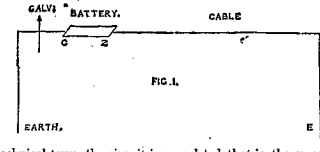
in which m is the action of the earth's magnetism upon the needle; and $m' - m''$, &c., is the total action, made up of the action of the earth's magnetism, and of that of the current circulating in the coils of the galvanometer, so that $m' - m$, $m'' - m$, &c., would represent the respective actions of the currents alone upon the needle. Again, n represents the number of oscillations made by the needle in a given time, under the influence of the earth's magnetism alone; and n', n'', &c., the numbers of oscillations made in the same time under the joint influence of the magnetism of the earth, and of the current circulating in the coil. And $n'^2 - n^2$, $n''^2 - n^2$, &c., give the relative values of the currents themselves that are in motion in the respective experiments. In all the cases quoted in the evidence the unit is taken as the value of the current from a given battery entering the cable, when the far end is on the earth.

2. The mode of obtaining this unit was as follows:—

The needle of a horizontal galvanometer was allowed to take its position in the magnetic meridian. The frame of the instrument was then turned until the galvanometer coil was at right angles to the magnetic meridian. This position was tested, by sending any current through the galvanometer. The position being correct, the current would of course in no way deflect the needle.

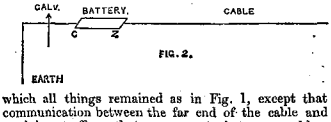
3. The testing current was then taken off. A small magnet was now moved near the needle, so as to set it in motion, and then removed far away, and the number of oscillations made by the needle in a given time was counted. Each experiment was several times repeated. In all cases the smallest arcs of vibration that were compatible with obtaining a sufficient number of oscillations, were taken. By this operation the value n in the formula was obtained.

4. The galvanometer, the battery, and the portion of cable under examination were then connected together and arranged, as in Fig. 1, which explains itself. To use the



technical term, the circuit is completed, that is, the arrangements are complete for each end of the battery to discharge itself. In this case the home end of the cable goes to earth, having the battery and galvanometer between it and the earth; and the far end goes direct to earth. The galvanometer needle is now under the joint influence of the voltaic current and of the earth's magnetism; and when it is disturbed, as already described, by the aid of a small magnet, it oscillates, and finally rests in its normal position, under the combined action of these two forces. The number of oscillations obtained in a given time was then taken and recorded; they give n' of the formula. Now $n'^2 - n^2$ is the unit in each experiment. One minute of time is the interval to which the oscillations in the whole series are reduced. This unit is what I have called, in short, the value of the ENTERING CURRENT; and it is, necessarily, the highest value in each series.

5. The arrangement shown in Fig. 2 was then made, in



which all things remained as in Fig. 1, except that the communication between the far end of the cable and the earth is cut off, so that *no* current whatever would pass if the cable were perfect, and all that did pass would be due to leakage in the cable.

6. Oscillations were read off and recorded as before and reduced to the number per minute, and so the value n'' was obtained. And the unit, as before mentioned, be $n'^2 - n^2$, the relative value of the current in circulating in Fig. 2, is $\frac{n''^2 - n^2}{n'^2 - n^2}$, and this is what I have called in brief the LEAKAGE.

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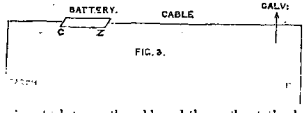
7. Another arrangement of the apparatus is shown in Fig. 3, where the battery is retained, as in the two previous



experiments, between the cable and the earth, at the home end; but the galvanometer is transferred to the far end, and placed there between the cable and the earth. In this case, the current that traverses the galvanometer will be the difference between the unit that enters the cable, and the portion that is now lost by leakage, and will be greater, according as the cable is in better condition; or less, as it may be in worse condition.

8. Oscillations under this new condition were read off and recorded as before, and reduced to the number per minute and represented by n'''. Treating this value in the way before described, we obtained $\frac{n'''^2 - n^2}{n'^2 - n^2}$; which is the relative value of the current that reaches the galvanometer in Fig. 3, and which was called the ARRIVING CURRENT.

9. It must be borne in mind, in these investigations, that the values obtained, by the right use of the formulæ given above, are not absolute quantities. They are relative each to the other in any individual series; but their relation varies with the varying conditions under which each experiment is made. These relations to each other remain constant only while the relations of the parts of the circuit are the same. If the resistance of the galvanometer,—the resistance of the portion of cable between the galvanometer and the leak,—the resistance of the leak itself,—the resistance of the portion of cable between the leak and the far end,—if either or all of these are varied, the total length of cable remaining the same, the same battery power being presented to it, the value of the entering current will change; and the relative or absolute value, either or both, of the forces, collected in the various parts of the circuit here in question will change also.

10. In order to illustrate the effect of these changes, I have calculated a series of Tables; and have assumed, in preparing them, relations tolerably analogous to those which exist in the case of testing the leakage of cables.

The quantities retained constant in calculating these Tables are the electro-motive force of the battery, the resistance of the battery, and the *total* resistance of the cable. They are as follows:—

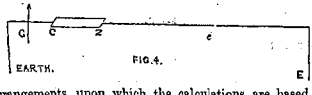
Electro-motive force	$= E$
Battery resistance	$= 0$
Cable resistance	$= 2$

11. The resistance of any length of cable, quoted in the text, is so great in comparison with that of the battery employed, that the latter may be rejected in these inquiries without introducing any sensible errors.

12. The galvanometers that are apt to be used with long cables will often consist of long lengths of very fine wire, presenting great resistance. One of those I found at Keyham was reported to be equal in resistance to 612·5 miles of the cable itself. I have therefore calculated the values for six different galvanometers, the respective resistances of which were—0, 0·25, 0·5, 1, 2, and 4. I have also considered the whole of the leakages to be summed up into one resultant, and represented by a single leak. I have also selected *three* different places in the cable for this leak. The resistance of cable between the galvanometer and the leak has been made respectively 1·5, 1·0, and 0·5.

13. I have further calculated the results for leaks of three different values; their respective resistances being 2, 1·0, and 0·5.

14. The following diagram, Fig. 4, will give the general



arrangements upon which the calculations are based, in which G represents the galvanometer, C Z the battery, Z to E the cable, e the leak.

15. The formulæ for *derived currents* are required for these investigations. The *principal* or *undivided* portion of the circuit is the portion from the home earth to e, which comprises the galvanometer, the battery (whose resistance is rejected), and the portion of cable between the battery and the leak.

16. The leak e is the branch through which one portion of the current is *diverted*.

17. The length e to E is the circuit through which the *undiverted* portion passes.

18. Let the resistance of galvanometer, &c. $= a$
Galvanometer to leak $= b$
Leak $= r'$
Leak to cable end E $= r$

Let the principal or undivided part of the circuit

$$a + b = R$$

19. A length of wire or single resistance, R', that may be substituted for the two resistances r and r', is obtained by dividing their product by their sum; that is

$$\frac{r\,r'}{r + r'} = R'.$$

20. And the following are the formulæ for the several values in question:—

$$\text{I. Entering current} = \frac{1}{R + R'}.$$

$$\text{II. Leaving current} = \frac{1}{R + R'} \times \frac{R'}{r}.$$

$$\text{III. Lost portion} = \frac{1}{R + R'} \times \frac{R'}{r'}.$$

$$\text{IV. Leakage} = \frac{1}{R + r'}.$$

21. The electro-motive force E is taken as unity in the above formulæ.

22. A single illustration will suffice to show how the Tables have been calculated. Take the first line of figures of Table I., in which

$$r' = 2$$
$$R = 1{\cdot}5$$
$$r = 0{\cdot}5$$

$$R' = \frac{r\,r'}{r + r'} = \frac{1}{2{\cdot}5} = 0{\cdot}4$$

$$\frac{R'}{r} = \frac{0{\cdot}4}{0{\cdot}5} = 0{\cdot}8; \text{ and } \frac{R'}{r'} = \frac{0{\cdot}4}{2} = 0{\cdot}2$$

I. Enters $\frac{1}{R + R'} = \frac{1}{1{\cdot}9} = 0{\cdot}526$

II. Leaves $\frac{1}{R + R'} \times \frac{R'}{r} = \frac{1}{1{\cdot}9} \times 0{\cdot}8 = 0{\cdot}421$

III. Lost $\frac{1}{R + R'} \times \frac{R'}{r'} = \frac{1}{1{\cdot}9} \times 0{\cdot}2 = 0{\cdot}105$

IV. Leaks $\frac{1}{R + r'} = \frac{1}{3{\cdot}5} = 0{\cdot}285$

23. And then by taking the entering current as the unit, we have

V. $\frac{0{\cdot}526}{0{\cdot}526} = 1$

VI. $\frac{0{\cdot}421}{0{\cdot}526} = 0{\cdot}8$

VII. $\frac{0{\cdot}105}{0{\cdot}526} = 0{\cdot}2$

VIII. $\frac{0{\cdot}285}{0{\cdot}526} = 0{\cdot}542.$

TABLE I.

24. Resistance of Leak = 2.

		Comparative Forces.				Proportionate Distribution.			
	0.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
	Galv.	Enters.	Leaves.	Lost.	Leaks.	Enters.	Leaves.	Lost.	Leaks.
Resistance to leak = 1·5.	0·	0·526	0·421	0·105	0·285	1·0	0·8	0·2	0·542
	0·25	0·465	0·372	0·093	0·286	..	..	..	0·570
	0·5	0·416	0·333	0·083	0·250	..	..	..	0·600
	1·0	0·344	0·275	0·068	0·232	..	..	..	0·644
	2·0	0·258	0·205	0·051	0·181	..	..	..	0·709
	4·0	0·169	0·135	0·033	0·133	..	..	..	0·786
Resistance to leak = 1·0.	0·	0·600	0·400	0·200	0·333	1·0	0·66	0·33	0·555
	0·25	0·531	0·347	0·173	0·307	..	..	..	0·339
	0·5	0·470	0·313	0·156	0·285	..	..	..	0·615
	1·0	0·375	0·250	0·125	0·250	..	..	..	0·666
	2·0	0·272	0·181	0·090	0·200	..	..	..	0·733
	4·0	0·176	0·118	0·058	0·142	..	..	..	0·809
Resistance to leak = 0·5.	0·	0·735	0·421	0·315	0·400	1·0	0·571	0·428	0·542
	0·25	0·632	0·355	0·266	0·336	..	..	..	0·594
	0·5	0·555	0·307	0·230	0·333	..	..	..	0·619
	1·0	0·424	0·243	0·181	0·285	..	..	..	0·673
	2·0	0·297	0·170	0·127	0·223	..	..	..	0·746
	4·0	0·188	0·106	0·079	0·153	..	..	..	0·824

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TABLE II.

25. Resistance of Leak = 1.

	θ.	Comparative Forces. I. Enters.	II. Leaves.	III. Lost.	IV. Leaks.	Proportionate Distribution. V. Enters.	VI. Leaves.	VII. Lost.	VIII. Leaks.
Resistance to leak = 17.	0°	0·545	0·363	0·181	0·49	1·0	0·66	0·33	0·732
	0·25	0·48	0·32	0·160	0·36	..	..	..	0·787
	0·5	0·428	0·285	0·142	0·33	..	..	..	0·777
	1·0	0·353	0·235	0·117	0·285	..	..	..	0·809
	2·0	0·26	0·173	0·086	0·22	..	..	..	0·831
	4·0	0·171	0·114	0·057	0·153	..	..	..	0·897
Resistance to leak = 18.	0°	0·666	0·333	0·333	0·500	1·0	0·5	0·5	0·750
	0·25	0·571	0·285	0·285	0·444	..	..	..	0·777
	0·5	0·500	0·250	0·250	0·400	..	..	..	0·800
	1·0	0·400	0·200	0·200	0·333	..	..	..	0·833
	2·0	0·285	0·143	0·142	0·250	..	..	..	0·875
	4·0	0·181	0·090	0·090	0·166	..	..	..	0·916
Resistance to leak = 95.	0°	0·909	0·363	0·545	0·666	1·0	0·4	0·6	0·733
	0·25	0·749	0·296	0·444	0·571	..	..	..	0·771
	0·5	0·625	0·250	0·375	0·500	..	..	..	0·800
	1·0	0·476	0·190	0·285	0·400	..	..	..	0·840
	2·0	0·322	0·128	0·193	0·285	..	..	..	0·888
	4·0	0·196	0·078	0·117	0·181	..	..	..	0·927

TABLE III.

26. Resistance of Leak = 0·5.

	θ.	Comparative Forces. I. Enters.	II. Leaves.	III. Lost.	IV. Leaks.	Proportionate Distribution. V. Enters.	VI. Leaves.	VII. Lost.	VIII. Leaks.
Resistance to leak = 15.	0°	0·571	0·285	0·285	0·500	1·0	0·5	0·5	0·875
	0·25	0·500	0·250	0·250	0·444	..	..	..	0·888
	0·5	0·444	0·222	0·222	0·400	..	..	..	0·900
	1·0	0·363	0·181	0·181	0·333	..	..	..	0·916
	2·0	0·266	0·133	0·133	0·250	..	..	..	0·937
	4·0	0·173	0·086	0·086	0·166	..	..	..	0·958
Resistance to leak = 19.	0°	0·750	0·250	0·50	0·666	1·0	0·33	0·66	0·888
	0·25	0·631	0·210	0·421	0·571	..	..	..	0·904
	0·5	0·545	0·181	0·363	0·500	..	..	..	0·916
	1·0	0·428	0·142	0·285	0·400	..	..	..	0·933
	2·0	0·300	0·100	0·200	0·285	..	..	..	0·952
	4·0	0·187	0·062	0·125	0·181	..	..	..	0·969
Resistance to leak = 95.	0°	1·142	0·285	0·857	1·000	1·0	0·25	0·75	0·875
	0·25	0·888	0·222	0·666	0·800	..	..	..	0·900
	0·5	0·727	0·181	0·545	0·666	..	..	..	0·916
	1·0	0·533	0·133	0·399	0·500	..	..	..	0·937
	2·0	0·347	0·086	0·259	0·333	..	..	..	0·958
	4·0	0·205	0·051	0·154	0·200	..	..	..	0·975

27. Columns I., II., III., and IV. in Tables I., II., and III., contain values all comparable one with the other; they are all multiples of the electro-motive force; and they show how the absolute value of the force varies as the several resistances vary. Columns V., VI., and VII. show that the *proportions* between the force that leaves and the force that is lost remain constant so long as the resistance of the leak (r') and the place of the leak remain the same, notwithstanding the changes that are made in the galvanometer; whereas columns I., II., and III. show, as might be supposed, that the absolute values vary with every change that is made in the arrangements, including that of the galvanometers. It is evident also that the relations between the portion of current that arrives at the far end and the portion that is lost at the leak, are as the resistance of the substituted wire (R') divided by that of the length from the leak to the far end, to the same resistance divided by that of the leak itself; or as

$$\frac{R'}{r} : \frac{R'}{r'}.$$

28. It will be noticed also that the *absolute* quantity lost by a leak, when the far end of the cable is free, according as the resistance of the galvanometer is greater, is less; while the *relative* quantity lost under like circumstances is greater.

29. The result of the change of the galvanometer, according as the leak is large or small, and nearer or further off, may be gathered by comparing the Tables one with the other; as may also the effect of varying the place and size of the leak, the galvanometer remaining the same.

30. It will be observed in these Tables, that the galvanometer in every case is taken to be at the home end of the cable, near the battery. This could not be the case in practice, because the value of the arriving current could only be obtained by observation, when the galvanometer is at the far end, as in Fig. 3. Of course, where the resistance of the galvanometer is = 0, or is only a very small fraction of that of the cable, this is immaterial. But for short pieces of cable, or for long lengths, where a galvanometer of great resistance is used, regard must be had to this in calculating the values. I have done this in Table IV. In the lines of figures opposite the word "Far," I have given in columns II. and III., the values calculated for this condition. I have also given in column IV. the amount of force that circulates when the galvanometer is thus transferred to the far end, which, on comparison with column I., will be found in all cases necessarily greater than had circulated when the galvanometer was wholly retained, as in Tables I., II., and III., at the home end.

31. The lines of figures marked "Home" are taken from the previous Tables for the sake of comparison.

32. I have taken for this illustration (Table IV.) a galvanometer of great resistance—half that of the cable itself; and I have applied it with the three leaks, in the three different places,

TABLE IV.

33. Resistance of Galvanometer = 1.

Comparative Results.

Galvanometer at Far End, and at Home End.

			Comparative Forces. I. Enters.	II. Leaves.	III. Lost.	IV. Enters.	V. Leaks.	Proportionate Distribution. VI. Enters.	VII. Leaves.	VIII. Lost.	IX. Enters.	X. Leaks.
Leak = 2 -	Dist. = 1·5	Far	0·344	0·242	0·181	0·423	0·232	1·0	0·704	0·527	1·231	0·644
		Home	„	0·275	0·068	..	„	„	0·800	0·200	..	„
	Dist. = 1·	Far	0·375	0·250	0·250	0·500	0·250	1·0	0·666	0·666	1·333	0·666
		Home	„	0·250	0·125	..	„	„	0·666	0·333	..	„
	Dist. = 0·5	Far	0·424	0·275	0·344	0·619	0·285	1·0	0·650	0·812	1·463	0·673
		Home	„	0·242	0·181	..	„	„	0·571	0·428	..	„
Leak = 1 -	Dist. = 1·5	Far	0·353	0·190	0·285	0·475	0·285	1·0	0·530	0·800	1·346	0·869
		Home	„	0·235	0·117	..	„	„	0·666	0·333	..	„
	Dist. = 1·	Far	0·400	0·200	0·400	0·600	0·333	1·0	0·500	1·000	1·500	0·833
		Home	„	0·200	0·200	..	„	„	0·500	0·500	..	„
	Dist. = 0·5	Far	0·476	0·235	0·588	0·823	0·400	1·0	0·494	1·235	1·729	0·840
		Home	„	0·190	0·285	..	„	„	0·400	0·600	..	„
Leak = 0·5	Dist. = 1·5	Far	0·363	0·133	0·400	0·533	0·333	1·0	0·367	1·101	1·469	0·916
		Home	„	0·181	0·181	..	„	„	0·500	0·500	..	„
	Dist. = 1·	Far	0·428	0·142	0·571	0·714	0·460	1·0	0·333	1·334	1·668	0·833
		Home	„	0·285	0·142	..	„	„	0·666	0·333	..	„
	Dist. = 0·5	Far	0·533	0·181	0·909	1·091	0·500	1·0	0·341	1·706	2·048	0·937
		Home	„	0·133	0·399	..	„	„	0·250	0·750	..	„

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34. Returning now to the observations made at Keyham on the Atlantic cable, the results of which are given in the evidence. The values were obtained by reading oscillations; and they are necessarily amenable to the laws of "derived "currents." So that there is a ready mode of testing their accuracy. I have given five different lengths of cable that were examined. There were other cases. The shortest length given is 400 miles. The galvanometer employed in all the cases in question was the same. It was reported to consist of 9 lb. 13 oz. of No. 20 wire, and presented a resistance of about 1¾ miles of the cable, certainly under two miles. This quantity being so small may be conveniently rejected from the calculations. The battery used in some cases consisted of 144 cells of zinc-copper, with sawdust moistened with dilute sulphuric acid; in other cases of 288 cells of a similar battery. The resistance of either of these batteries is small, compared with that of the cable under examination, and may be rejected in these calculations, which are not very rigid, without sensibly affecting the results, except when the leak is large or near home; and the same exception holds good with regard to the galvanometer.

35. The following Table V. is a summary of the results given in evidence.

TABLE V.

	Miles.	Oscillations per Minute. I. Earth alone.	II. Enters.	III. Leaves.	IV. Leaks.	Proportionate Distribution of Force. V. Enters.	VI. Leaves.	VII. Leaks.
288 Cells	400	20·7	84·	83·	32·	1	0·974	0·059
	2,200	„	55·5	47·05	53·3	1	0·598	0·805
144 Cells	554	16·3	60·	52·77	54·1	1	0·755	0·798
	554	„	67·41	48·38	65·80	1	0·485	0·937
	992	15·8	59·33	53·00	33·64	1	0·87	0·302

36. Without being in possession of the correct resistances of the galvanometer and battery, it would be an unprofitable task to attempt to refine upon these figures; the more so, as the circumstances under which the examinations were made were not favourable to very accurate measurements. The needle was on a pivot, and subject to friction; the table was not over stable, and much iron was about. The variations in the number of vibrations per minute, (as shown in column I.), obtained by the same needle on different days, and under different conditions, is an illustration of the disturbances to which we were exposed; some evidently due to the unstable character of the galvanometer itself.

37. But for the moment, confining our attention to mere galvanometer-force, apart from all calculation, a reference to columns II., III., and IV. will show at a glance where there was the *greatest* action on the galvanometer, and where the *least*; where, for instance, the amount that reached the far station was greater or less than would go off by the leak alone, when the far end was free; and this without even taking the square of the number, which would make it more manifest. For instance, with the good piece of 400 mile cable the available force at the far end was *greater* than that of absolute leakage; with the 2,200 miles it was *less*. With the 554 miles of cable the available force was less than the leakage, whether the good length of 400 miles was nearer or further from home. But it was, comparing the two cases, less when the good length was far away than when nearer home. With the 992 miles the available force was greater than the absolute leakage.

38. On referring to columns V., VI., and VII. for the proportions, we have figures which, from the cause above stated, may require correction. For instance, on testing the values given for the 400 mile length by the laws of "divided currents," I find that the leaving proportion is a little too *high*, and the absolute leakage is not too high.

39. The mode of testing was by assuming an unknown resultant of leakage x, at a given distance a, and applying the formulæ to these values. If the figures in the Table were correct, the value of x would be the same in all the equations; if otherwise, it would not be.

40. Finding it was not the same, I assumed that 0·089 in the Table was the true leakage, and from this calculated what should be the corresponding value of the arriving current, and found it should be 0·932. I substituted this for the original value, and by calculation found it would correspond to a leak 100 miles distant, presenting a resistance equal to 4,100 miles.

41. I applied the formulæ to these resistances as follows:—

		Miles.
Resistance of galvanometer	=	0 }
Galvanometer to leak	=	100 } R
Leak	=	4100 = r'
Leak to cable end E	=	300 = r

We should have—

	Miles.
R =	= 100
$R' = \frac{r\,r'}{r+r'} = \frac{12300}{44}$	= 279·5
$\frac{R'}{r} = \frac{279·5}{300}$	= 0·932
$\frac{R}{r'} = \frac{279·5}{4100}$	= 0·068

Then—

I. Enters $\frac{1}{R+R'} = \frac{1}{379·5} = 0·00263 =$ 1

II. Leaves $0·00263 \times \left(\frac{R'}{r} =\right) 0·932 = 0·00245 = 0·932$

III. Lost $0·00263 \times \left(\frac{R'}{r'} =\right) 0·068 = 0·00018 = 0·068$

IV. Leaks $\frac{1}{R+r'} = \frac{1}{4200} = 0·000236 = 0·089$

42. From this it is evident that the values for the 400 mile cable given in Table V. are very nearly true, the error being in the leaving current having been reckoned a little too high.

43. The amount of error is $0·974 - 0·932 = 0·042$, so that the sum of leakage in that cable was equal to a leak 100 miles from home, presenting a resistance equal to 4,100 miles of cable.

44. By a little painstaking the recorded values for the other cables might be converted into very proximate, if not absolutely true, values.

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Note B.

Charging Capacities of various Cable Cores.

The following table is the result of an examination made at the Gutta Percha Company's works on May 6, 1858, when the temperature of the air was 57°, and that of the water 60°.

The battery employed was 504 cells of the zinc-copper sand battery, charged with diluted sulphuric acid.

The galvanometer was a horizontal, made by Reid, Brothers, for the Atlantic Telegraph Company. It was reported to contain 3½ miles of No. 46 copper wire; which, according to data given by Mr. Henley in his report from Valentia, dated September 30, 1858, would be equal in resistance to 612½ miles of the conductor of the Atlantic cable. It gave a deflection of 62° with one pair of the plates of the above-named battery in short circuit.

Eight distinct cable cores were at my disposal; the outer dimensions of all were the same, namely, that of the Atlantic cable core, 0·375 inches, and the length of each was about a mile. I have given the precise number of yards of each in column 1. They were all suspended in the canal. In the Table, W. indicates Whitehouse core. It contained three distinct No. 22 copper wires, each insulated from the other. C. is Chatterton core, having a layer of his compound between the conductor and the gutta percha, and a layer of the same compound between each two coatings of gutta percha. The conductor was the Atlantic strand, consisting of 7 No. 22 wires, and presenting an outer surface four times that of a single No. 22. A. is the Atlantic cable core.

H., in experiments 6 to 10, are five distinct arrangements of Mr. Hearder's core; the conductor, as before, is the Atlantic strand; and this is covered with cotton or worsted, either or both, as specified in the Table, before the gutta percha coats are applied.

	Charge.	Discharge. Immediate.	Discharge. After 30″.	Leakage.
	°	°	°	°
1. W. 1880, one No. 22	42·0	39·0	10·0	3·5
2. W. 1880, one No. 22	45·0	42·0	9·75	3·5
3. W. 1880, one No. 22	45·0	42·0	9·75	3·5
4. C. 1760	52·0	51·5	33·5	2·0
5. A. 1760	55·0	51·5	10·0	6·0
6. H. 1067, single cotton	70·5	68·0	11·0	7·5
7. H. 1754, single worsted	70·5	70·0	18·5	6·5
8. H. 1747, double cotton	85·0	84·5	23·5	6·0
9. H. 1760, double worsted	90·0	86·0	20·0	9·0
10. H. 1752, cotton and worsted	100·0	95·0	20·0	9·0
11. W. 1880 × 2 wires	65·0	62·0	15·0	4·5
12. W. 1880 × 2 wires	65·0	63·8	15·6	5·0

Atlantic strand in all.

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One end of the battery was permanently in connexion with the earth, the galvanometer intervening. The far end of each conductor was free and insulated. The column headed "charge" gives the extreme deflection of the galvanometer, when the home end of the conductor of the respective cores was touched for a moment with a wire leading from the free end of the battery, the battery itself being well insulated.

By inspecting this column, it is manifest that the smaller the conductor the less the charge; or the larger the inner surface of gutta percha presented to the conductor the greater the charge.

For example, in Exp. 1, in which the charging surface is represented by the surface of a single No. 22 wire, the deflection is 42°. In Exp. 4, where the conductor is the Atlantic strand of seven No. 22 wires, presenting a surface four times that of Exp. 1, the deflection is 52°, and in Exps. 6 to 10, where the charging surface of gutta percha is manifestly larger, from the interposition of the fibrous material between the wires and the gutta percha, the deflection became still higher, varying from 70½° to 100°; the latter being the result, when the interposed fibrous matter was greatest. The deflection in Exp. 1 is full high, as a comparative value, the W. core being longer than the others.

The last column headed "leakage" represents the permanent deflections of the galvanometer needle, when the wire from the free end of the battery was allowed to remain in permanent connexion with the home end of the conductor. It is well to read these figures in connexion with those of the "charge" column; because they help to correct the latter by the quantities due to leakage. Due allowance being made for leakage, the conclusions to which column II. leads are still maintained.

It is manifest that the experiments herein recorded cannot fully solve the problem of the relative value of conductors of various size. They advance us one step in the inquiry. They prove, it is true, that the larger the gutta percha surface the greater the absorption of electricity or charge,—and as charge and retardation are in the relation of cause and effect, the suspicion may arise that large conductors are not the best; but, in order to solve this problem experimentally, it is necessary to examine whether the larger charge, acquired by the larger surface, may not be produced from the larger wire, in the same or in less time than the smaller charge from the smaller wire; and still more,—for this is the greatest element of the retardation,—whether the larger wire may not *discharge* the larger *charge* from the larger surface with as great or greater rapidity than the smaller does the smaller charge. I apprehend that experiment alone can determine this. The lengths being so short it was not possible for me to measure the interval between the current's entering one end and arriving at the other;—it would not have amounted to more than the thousandth part of a second. Exp. 1 was made upon one of the three wires in the Whitehouse core, the other two having their ends free and insulated.

Exp. 2 was made on the same one wire, one of the others being connected with the earth.

Exp. 3 was the same, except that both the spare wires were connected with the earth.

The charge in these two cases was greater. The probable explanation is that the dielectric or gutta percha between the inner and outer coating was thinner; the spare wires in connexion with the earth acting as it were as an outer coating nearer to the inner.

Exp. 11 and 12. The battery wire was applied to two of the three wires of the W. core. In the one case, the two home ends were united, and the current was distributed between the two wires. In the other case the two far ends were united and the current passed along the first wire to the second. The results are identical. The charge in both cases is greater than with the Atlantic conductor Exp. 4, although the total surface presented to the wire is one half less. These two results cause regret that there was no opportunity of pursuing the inquiry further.

The two "discharge" columns were obtained by touching one end of the charged wire with an earth wire, either immediately after the charge was given or after an interval of 30 seconds. They may be useful for reference. The latter column has a general relation to the "leakage" column. The better the insulation the better the retention of charge, the ends of the conductor being free. The figures given in the Table are the results in each case of several consecutive observations. Considerable discretion is necessary in dealing with these figures. They bear no other relation than that any one is greater or less than any other; but how much greater or less each is to another is not apparent, and is not to be determined. The lowest deflection in the charge column is high, and every degree beyond this is of far higher value than the degrees within it.

On comparing Exp. 1, with Exp. 11, that is, comparing the charge of one No. 22 wire with the charge of two No. 22 wires we have the figures 42° and 65°. The coated surfaces here are as 1 : 2; and the deflections are 42° and 65°. It is not at all impossible that the 23° (beyond 42°), shown in the second case, may be the exact equivalent of 42°; in other words that a deflection from 42° to 65° is equal to a deflection from 0° to 42°, and that the full deflections 42° and 65° are in the proportion, each to the other, as 1 : 2.

This is only a suggestion in the present state of our knowledge. It might have been determined clearly had we had two No. 22 wires in *distinct* cores.

If it were confirmed by an experiment of that kind, other inferences would follow; as, for instance, on comparing Exp. 11 with Exp. 4 where the surface ratios are as 1 : 2, we have deflections as 65° to 52°, or in an inverse ratio to the surface, the lesser surface giving the larger charge, and *vice versâ*. In this case, while the surface ratios are as 1 : 2 the conducting ratios are as 1 : 3·5. The question then presents itself in this form:—Surface and conduction in the proportion of 1 : 1, compared with surface and conduction in the proportion of 1 : 1·75, giving values represented by 65° and 52° respectively, will this be accepted as evidence that *toties quoties* less proportionate charge is to be expected with large conductors than with small. This is an important question, and one which has evidently occupied greatly the attention of those engaged in these inquiries, and it is not the whole question, for the case may be in which the conducting power of a smaller wire may be equal to the work required. All beyond this is expense and loss, and if attended with increase of charge and retardation, although not a proportionate increase, it would yet entail a real evil for a questionable good.

C. V. W.

Fernside Villa, Red Hill, Reigate,
April 27, 1858.

1.—*Mission to Keyham.*

DEAR SIR,

ON the receipt of your letter of March 22, I made arrangements to proceed, with the least possible delay, to Keyham Ford, in order to investigate the conditions under which the retardation of signals takes place in the Atlantic cable, and to furnish the directors with any suggestion that might occur to me whether on this or on any other point connected with the cable.

2.—*Facilities afforded.*

I arrived at Devonport on Saturday March 27, and commenced my examination on the morning of the following Monday in association with Mr. Whitehouse, the Company's electrician, to whom I am greatly indebted for the courteous and cordial manner in which he co-operated with me throughout, and for the assistance he rendered me, and which, in my comparative inexperience, I so much needed. I found also the same ready help cheerfully rendered by all the officers in the department.

I left Devonport on the morning of Wednesday, April 14.

3.—*Induction.*

The retardation is due, as we have long been aware, to *induction*. The word has become so popular in connexion with cables that its significance is apt to be overlooked by any one who uses it.

4, 5.—*Polarisation.—Loss during Polarisation.*

Electric force on being presented *polarises* the particles of copper wire end on, as it were, and in advance of itself as in fact is the case with our aerial wire; not only so, it also polarises in all directions the cylinderical plate of gutta-percha, with which the wire is surrounded. The polarisation of the plate of gutta-percha is accomplished at the expense of the electric force presented to the cable, so that as it passes on it, leaves part of *itself* on the gutta-percha, and, (though nothing should be lost by leakage) the initial force, as it reaches the *far* end of the cable, is not equal in value to that presented at the *home* end.

6.—*Charge.*

The successive portions of the current in passing add to the effect, and the gutta-percha plate becomes what is termed charged, a condition precisely analagous to that of a plate of glass coated on each side with tinfoil, one side being presented to a source of force, the conductor of an electrical machine, while the other side communicates with the earth.

7.—*Time for Charging.*

The operation of *charging* requires *time;* hence the interval of about two seconds between the time of electric force being presented at the home end, and its first appearance at the far end of the 2,000 miles of cable. But this

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delay would be of little moment if the whole force that passes could at once come out from the far end, and leave the cable in its normal state as it found it on entering, because other currents under such circumstance would be sent rapidly in without waiting for the first to make its exit.

8.—*More Time for Discharging.*

But the plate of gutta-percha retains a charge, and parts with it slowly, if allowed to discharge itself many minutes will elapse before the whole charge will disappear, and during this time a continuous current capable of making one long continued signal will flow out from the far end. It is this second source of delay which is the main ingredient in preventing the rapid transmission of signals through the Atlantic cable. Finding no record of experiments for discovering the *precise character* of the static *polarisation*, as produced by the various conditions under which electric force could be presented to the cable, we availed ourselves of the kindness of Sir W. Snow Harris, (to whom our thanks are due in this and other matters), who lent us the apparatus necessary for this enquiry, and cheerfully placed any of his other apparatus at our disposal; by this means we acquired the information of which I was in quest, and to which I shall refer hereafter.

9.—*Depolarising at home end.*

I made myself well acquainted with the method adopted by Mr. Whitehouse to depolarise the gutta-percha, or clear the cable of its charge. It is briefly this: he presents a current to the home end of the cable, which travels on, polarising as it goes until it reaches the far end of the cable and *passes to earth* through the relay, where it makes one signal, which will be expended in length and time until the cable is fully discharged. He now, at the right interval, presents a second *current* of the *reverse character* to the home end of the cable, which travels onward depolarising the cable as it goes, and polarising it in the reverse direction; so that while the charge is running out from the far end of the cable it is being neutralised at the home end of the cable, and thus a second signal can be sent into the cable with less delay. The depolarisation at the home end of the cable by means of a reverse current is more rapid and efficacious than the mere *running out to the earth* at the far end.

10.—*Depolarising at far end.*

I was, therefore, induced to consult with Mr. Whitehouse as to the possibility of depolarising by a reverse current at the *far* end, as well as at the *home* end, but this operation is beset with difficulties, and would seem only to complicate the matter; we devised no method, by which the process could be put into practise.

11.—*Pre-charging Wire.*

We had already discovered the absolute character of the polarisation produced by a current under given circumstances, and I felt it right to examine whether by antecedently producing similar polarisation by extraneous means, it were possible to arrange that the current itself should not be delayed in its course; in order to discharge this essential function we produced this antecedent polarisation of like or of unlike character, and we also put the cable into a rheo-electro static state, by opposed force; but, as far as our experiments went, retardation and not acceleration was the result.

12.—*Velocity.*

With voltaic currents no increase in velocity is obtained by increasing the number of elements, this is not quite the case with the induction currents employed by Mr. Whitehouse. A slight increase in velocity is obtained by adding to the elements, but the increase is not in proportion to the increase of elements, and it cannot be pressed to excess.

13, 14.—*Secondary Coils.—Large Gutta Surface.*

The secondary coils employed by Mr. Whitehouse for generating the current he requires, are constructed with wire of unusually large size (No. 20), very different from the wire No. 36 or No. 40, that is commonly employed for such coils as well as for the coils of magneto-electric machines; this necessity seems to arise from the large size of the conducting wire of the cable, which gives a corresponding large extent to the inner surface of the gutta-percha plate; this larger surface retains a larger quantity of the passing force for the act of polarisation; and it appears that if the large force is abstracted from a current that is derived from a smaller secondary wire, so little will remain to reach the far end that no working result will be obtained.

15.—*Magnetic Machine.*

I had not the opportunity of seeing the result derived from Mr. Henley's large magnetic machine with fine wire coils; the coils were out of order when it arrived at Keyham, and the repairs were not quite completed when my time for leaving had arrived. From the above considerations I should be prepared to expect that the magneto-current which might arrive at the far end, would be inadequate to its work.

16.—*Platinized Graphite.*

The large wire of the primary coil, and the equally large plates of the battery cells, are necessary for getting out the full effect from the secondary coils in use. I had already mentioned to Mr. Whitehouse my employment of platinized graphite instead of silver for battery plates, its advantages and its comparatively little cost, I think he is prudent in being about to substitute it for silver, he will obtain from it larger supplies of electricity, and the company will realise no small sum for the rejected silver plates when returned into store. There are some anomalies connected with the behaviour of the secondary coils in use; and it occurred to me with discussing my notes to suggest whether any experiments have been made with coils not having iron cores, and to what extent and with what results.

17.—*Coils without Iron Wires.*

If currents adequate to the work can be readily generated from such coils, the absence of iron cores would be an advantage in favour of the instantaneity of generation and cessation. It now takes time, appreciable time, to magnetize and demagnetize, and this time in the absence of iron would be avoided.

18.—*Cable when laid.*

For the above observations I have confined my attention to the cable as it now exists, to the difficulties with which as a cable it is surrounded, and with a view to mitigate them. My impression is that they will not be increased when the cable is laid; the lowness of the temperature and the uncoiling of the cable will so far be in its favour.

19.—*Suggestions.*

The shortness of my stay at Keyham, together with the unavoidable occupations at times, or interruptions of the cable necessarily prevented my exhausting the questions which most pressed themselves upon my mind:—1st. Whether any means can be devised by sending in reverse currents at the far end to conspire with those at the home end for shortening if not the dots and dashes at least the blanks. 2nd. Whether by any other means than those we were able to try of sending a constant current through the cable and keeping it charged, a working current would be sent through the *charged* cable, which should not be delayed or attenuated from the act of charging. This is a purely experimental question I had proposed trying the effect of Henley's large machine in conjunction with the coils, but it was not ready. 3rd. Whether the absence of iron from the centre of the secondary coils would be an advantage or otherwise, in respect to the amount of force raised, and its prompt generation and cessation.

20.—*Instruments.*

I speak more of the cable than of the apparatus, because the essential properties of the cable will have their influence, and evidently with little modification, whatever the instruments may be; and time, especially that for the discharge, is one element in the problem that can only be dealt with in modes analogous to those which have been already adopted, and possibly modified by some such operation as those I have just set forth.

21–25—*Magnetic Storms—their Periodicity—Effects neutralized.—Return Wire.*

Should the cable happily be laid in its integrity, there are certain cosmical conditions to which it will be exposed that promise to be of serious inconvenience; I mean the variation in terrestrial magnetism, and especially those variations known as magnetic storms, and which are now increasing in number and force, and will doubtless follow the recognised cycle, and steadily increase during the coming two years or so. This subject had excited so little attention that, for the instruction of civil engineers, last summer, when Mr. Window's paper was being discussed, I prepared the corresponding curves of sun spots and magnetic storms, of which I enclose a copy, with a view to directing attention

to this probable source of interruption. The time for closing the discussion arrived without my having an opportunity to bring them forward. Doubts were afterwards expressed to me whether this influence would be felt on submerged wires. These doubts, however, have been removed by the results of the observations made at Valencia on the 380 miles of cable lost there. The effects of these disturbances are felt by me in their fullest developement on the line of telegraph from Ashford to Margate, places occupying somewhat the same relative positions as Valencia and Newfoundland. Coincident with the earthquake at Naples in December last, we had our needles so violently deflected as to become horizontal at one time. The disturbance continued with greater or less violence, and with continual variations, for 36 hours. During my absence at Keyham on April 9th, we felt the effects of a still greater storm than the above. I have it noted as the most violent on record. During some part of the time we could not send messages on the instruments in their then condition. In 1848-9, when these disturbances were least at their height, we met the minor difficulties by a mechanical arrangement, but in the heavier cases we condemned one of our needles and used its wire as a return wire for the one remaining needle, in place of the earth. By this device the disturbing action of one wire was neutralized, as far as the telegraphic instrument was concerned, by the reverse action of the other wire, while both led to earth. With earth wires removed, the line wires were free from disturbing influences.

I have long considered that your only protection against these disturbances will be to have two wires instead of one; to have, that is, a complete metallic circuit returning by a second wire, instead of being discharged by the earth.

26.—*Other Advantages.*

There appears to me, as far as my experiments extended, other and considerable advantages connected with an entire metallic circuit. The polarization of the gutta-percha plate, and the attendant retardation, is less under such circumstances, and I believe it will be found that, in proportion as the batteries, the apparatus, and the wire, are insulated from the earth, and the earth itself is more perfectly excluded from all participation in the matter, the greater will be the progress. These, of course, are points to be settled by careful research before another cable is devised.

27.—*Strand too large.*

I am of opinion also that the conducting strand of the present cable is unnecessarily large. Its largeness requires that the gutta-percha surface which is around it and contiguous to it, shall be large also, and hence there is greater abstraction made of the current as it passes, and a greater amount of depolarization or discharge to be accomplished after the current has made its signal at the far end.

28.—*Small Wire.*

The only essential condition of decreasing the size of this wire is that its resistance is increased, but this is a matter, compared with other things, of small importance. The resistance of the cable is the least of all the difficulties to be overcome. A little extra power will readily overcome a large resistance. The advantages are, on the other hand, great; to have a less surface to be charged, and to gain all the extra power of insulation that follows from having a small instead of a large wire, in a given bulk of gutta-percha. The increased resistance of the returned wire mentioned above may be met with equal readiness. The examination of a small wire, in relation to a large one, is a question that can readily be tested by experiment.

29.—*Other Advantages.*

Whatever may be the result of experiments upon this point there are other great advantages to be expected from a smaller conducting wire, which is, that the batteries, the primary and secondary wires may be all in like proportion smaller and therefore more manageable every way, and of course less costly. Should this be so the advantage will re-act favourably in respect to the size and construction of the commutators also, the wire being smaller, *ceteris paribus*, will take less space from the gutta-percha and allow the cylinder to be just so far thicker, and the thicker the plate is the more is its inductive power reduced, and the less charge will it take.

30.—*Remedy.*

As far as the present cable is concerned, the question is rather to find a remedy for an existing disease, or the most we can expect is to modify or slightly mitigate it. Since I was first addressed on this subject, my own thoughts have been directed to a prevention rather than a cure. It is well known that if the same kind of electricity is presented to both sides of a coated pane, as the cylindrical plate of gutta-percha, of which the insulating material of the cable is composed, we cannot charge it, if, for instance, we know, as by experiment we do know, the character of polarisation that will be acquired by the inner side of the gutta-percha plate, by the passage of an electric current, under known conditions, through the conducting wire, we may reasonably expect to prevent such a current from producing such a charge, by at the same time presenting to the outer side of the gutta-percha plate a current that shall produce a similar character of polarisation. My impressions are confirmed by some elementary experiments on this point, made with the assistance of Sir W. Snow Harris, with his apparatus; my opinion is this, that if the thin conducting wire were duly insulated with its several coats of gutta-percha, and the gutta-percha then coated with a metal, of which the well known lead-protected gutta-percha wire is a type, and this again were insulated with gutta-percha, that the effect of the current in the inner wire might be neutralized by the equal effect in the reverse direction of the outer metal, and the charge might be prevented. This remark is equally applicable, with necessary modifications, to a two-wire cable; the reaction of the returned wire would modify the charge on the gutta-percha plate directly between it and the main conducting wire, and I believe some of Mr. Whitehouse's early experiments show that the retardation, though not annihilated, was comparatively less under such circumstances. It is not easy to arrive theoretically at the full comprehension of such an arrangement, because so many unexpected results come from the cable, under new conditions, but the test of experiment can be very easily applied.

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The *specific inductive* capacity of all bodies is not the same. Should we succeed in laying hands on a tolerably insulating body, such, for instance, as Trinidad bitumen (as proposed by the Earl of Dundonald), and find its specific inductive capacity comparatively low, advantage might be derived from placing a layer of it first and next to the wire. I scarcely expect to find such a body that would not charge at all, but it is just possible to find one that would charge very little. It is worth looking after.

31.—*Leakage.*

During my sojourn at Keyham I made several measurements of the proportion of the current that was lost by leakage, and that passed through the cable under different circumstances. The instruments at my command had not been constructed for this special purpose, and, although the results arrived at are open, on this account, to a certain correction, due to instrumental error, yet they are not very far from being, on the whole, tolerably accurate representations of the conditions.

32.—*Great Leakage.*

I was hardly prepared to find the leakage so high, but, notwithstanding this, a current at the far end equal to the work is readily obtained. I found, for instance, with 2,200 miles of the cable, that four-fifths of a given current were lost in leakage when the far end of the cable was free or clear of earth, but that only *two*-fifths were lost when the far end was put into condition to receive signals, so that in practice *three*-fifths of the whole force reached the far end available for signals. With *400 miles of cable* more than nine-tenths of the whole force reached the far end of the cable as signal force. If the far end of this length of cable was free, only one-twelfth of the whole force escaped by defective insulation.

We obtained similar measurements of 554 miles of cable, of which 154 miles consisted of the least perfect cable, and the remaining 400 was the length newly manufactured of standard wire. The results were very different according as the standard length, which is well insulated, was nearest the battery power or farthest from it. The following are the reduced details of the two cases:—

1st. Standard wire at *home* end,—

Entering current	1·000
Signal force (far end)	0·755
Leakage (circuit complete)	0·245
Leakage (far end free)	0·798

2nd. Standard wire at *far* end,—

Entering current	1·000
Signal force (far end)	0·485
Leakage (circuit complete)	0·515
Leakage (far end free)	0·937

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33.—*Place for Good Lengths.*

These measurements show that the greatest advantage is gained out of the best specimens of cable by having them nearest to the source of electricity, or at the shore end. The advantage gained in the above two modes of arrangement is as 755 to 485, or as $1\frac{1}{2}$ to 1.

The good specimens of 400 miles is at the bottom of Niagara coil, and comes out last. Should her portion of the cable become so exhausted that this will become the first length from Newfoundland, it will be in the best possible position.

Should the first lengths from either shore consist of the worst specimens, they will be in the least advantageous position.

Had time permitted, these measurements would have been extended and modified to all the sub-divisions of the cable.

The nine questions, therefore, which have presented themselves to my mind in respect to any subsequent operations are—

34.—*Suggestions.*

1st. That variation in the earth's magnetism will interfere with the signals, and that this difficulty will be entirely overcome by having a return wire instead of an earth discharge.

2nd. That there is reason to expect that the charge and its attendant retardation will be favourably modified by the use of a second wire.

3rd. That the use of a smaller wire promises favourably to modify the charge and retardation; and it can be better insulated under equal conditions.

4th. That a charge may possibly be altogether prevented by having an insulated outer conductor, either as tube or woven wire.

5th. That charge might be kept down by the intervention of a body of bad specific inductive capacity, if such can be found.

6th. That the best parts of a cable should be nearest to their respective coasts.

35.—*Subsequent Questions.*

In laying this brief summary of the results of my short visit to Keyham before the directors, I do not dismiss the subject. I contine to discuss my notes; and should any question arise tending to throw light on this complex problem, and which may require experimental solution, I shall not hesitate to forward it to Mr. Whitehouse, who may probably be able to answer it of his own knowledge, from his past experiments; and if not I am sure he will most gladly put it to the *prompt test* of experiment.

Although it is out of the usual course to address a board of directors in connection with experiments made by one of themselves, yet I am unable to conclude without expressing my surprise and admiration at the remarkable results obtained by Professor Thomson.

36, 37.—*Gauss and Weber.—Professor Thomson.*

I am aware that 24 years ago Gauss and Weber described a telegraph which gave signals by the deflection of a magnet, that the magnet carried a mirror in which a scale was reflected, and that the signals consisted of the greater or less deflections of the magnet in one or other direction; but I was not prepared to see any such system realised. The present Atlantic system consists of dots and dashes, and has *time* for its basis. Professor Thomson's consists of degrees of deflection, and has *force* for its basis. He uses derived currents of various values, in order to produce the amount of deflection he requires to correspond with the signal in request; and he discharges the wire by compensation currents of calculated values. Although the individual signals may not travel more rapidly than other kinds, and though the discharge of the wire *requires*, as in other cases, time, yet since each letter is represented by one act instead of by several acts combined, more letters can be read off in a given time. The index is a spot of light reflected from a mirror and traversing a scale. Should this system come into practical use it will be a most remarkable realization of theoretical deductions. I think I should place on record the following:—The first message sent through 2,640 miles of cable by this system, on the evening of Saturday April 10th; it was the result of a rough arrangement and before much experience had been gained; and, under the circumstances, is remarkably accurate. The first column shows the values *given* to the respective letters; the second, the *values obtained*; and the third the letters themselves that constitute the message:—

— 66	— 65	P
— 10	— 17	l
+ 72½	+ 60	e
+ 14	+ 13	a
— 96	— 90	s
+ 72½	+ 65	e
— 96	— 90	S
+ 72½	+ 64	e
— 36	— 37	n
+ 57	+ 53	d
— 110	— 105	T
+ 123	+ 102	h
+ 65	+ 73	e
— 10	— 12	L
+ 14	+ 13	a
— 96	— 93	s
— 110	— 110	t
— 36	— 42	N
+ 72½	+ 58	e
— 125	— 120	w
— 125	— 120	
— 96	— 110	s
+ 88	+ 77	F
— 81	— 80	r
— 50	— 59	o
— 25	— 30	m

There was an index error of 7° at the end to be allowed for, the first five words of the message "please send the last news from," were sent in 11½ minutes. The message was read off by means of the alphabet with which we had been previously furnished.

The above are the chief points that pressed themselves on my attention during my stay at Keyham.

In conclusion, I would direct attention very forcibly to the extreme importance of taking no hasty measures in respect to any future cables, for the safe laying of the present will assuredly call for companion cables. While every advantage is being taken on this occasion of having so unusual a length of cable for experiments, future movements should be conducted with deliberation, and careful experiments made, and this not in haste, in order to determine as nearly as possible which of the above, or of any other models, may produce a cable free, or in great measure free, from the properties that so militate against rapid signalling.

(Signed) CHARLES V. WALKER, F.R.S., F.R.A.S.

G. Saward, Esq., Secretary,
Atlantic Telegraph Company.

Mr. W. T. Henley.

Mr. WILLIAM THOMAS HENLEY examined.

2311. (*Chairman.*) You are a telegraphic engineer?—Yes.

2312. You have been connected for several years with devising and manufacturing instruments for receiving and recording telegraphic signals, have you not?—For 22 years.

2313. (*Professor Wheatstone.*) Have you also manufactured submarine cables?—I have manufactured submarine cables, and laid many miles of underground wires.

2314. Can you state what cables you have made, and where they have been laid?—I made the Tasmanian cable, 240 miles in length, which is laid between Victoria and Tasmania. I made a piece of about 30 miles for Dr. O'Shaughnessy in India; I do not know the exact spot where that was laid; I think it was cut up into lengths to lay across rivers. I also made a piece of about 10 miles to lay in the Mediterranean.

2315. (*Chairman.*) Where was that laid?—It was intended for repairing the Malta and Cagliari line, but on failing to do that the end was landed at Marsala, connecting that place with Malta.

2316. Did you go out with the expedition that went out to repair that cable?—No, I did not.

2317. Was not the expedition unsuccessful?—Yes;

they got into deep water and the cable broke in lifting; they were in about 360 fathoms, and in grappling for it, every time they got hold of the bight of the cable it broke in raising.

2318. Are there any other cables you have made?—Only those three that I have mentioned. I have laid 5,000 miles of underground gutta-percha wire.

2319. Were not you concerned in the first introduction of the magnetic instrument as a generator of electricity for telegraphic purposes?—Yes.

2320. After the injury of the Atlantic cable, were you employed by the directors to test and examine that electrically?—Yes.

2321. Will you describe the condition of the cable, and relate any facts of importance with respect to it which are not specially alluded to in your report?—I think I explained in my report all the results that I arrived at. I found a very serious fault, which gave what we call dead earth, and by testing with the resistance coils it appeared to be a fault equal to 270 miles.

2322. Was that after it was laid?—That was after it was laid and had failed.

2323. Are you quite satisfied with that result?—I was quite satisfied; it gave a resistance equal to 270 miles, but how much was made up of the fault, and how much of the wire intervening, I could not exactly say.

2324. Had you ever tested the Atlantic cable before it was laid?—I never tested for leakage. I worked through it with a small magnet when it was lying at Keyham, through the 2,500 miles.

2325. (*Mr. Varley.*) In testing for a fault, do you think it possible to ascertain the amount of resistance peculiar to the fault itself, so as to be able to ascertain with any precision the exact locality of the fault, whether the fault was close in shore, giving a resistance of 280 miles, or whether it was 280 miles out?—We could only judge by testing whether the fault got better or worse after the current had been passing through for some considerable time.

2326. Can you give any accurate idea of the amount of resistance properly due to the fault itself?—I judged it to be a bad fault, as it did not mend by passing positive currents through it; if it was a small fault, we supposed the copper would become oxidized to mend the fault.

2327. You think it is not certain that the fault is at a distance of 280 miles, it may be nearer?—It may be nearer. I think it is some considerable distance off; more than 100 miles.

2328. (*Chairman.*) When did you make these experiments?—The experiments connected with working through the cable were made during the time it was at Keyham; but those for testing for insulation at Valentia in September, after its failure.

2329. Since then have you not published a document showing the relative testings of the cable?—I sent in a report to the Atlantic Company, from Valentia, as to the state of the cable. That report was sent by Mr. Saward to the papers for publication.

2330. Will you state your impression as to the present state of the cable?—It appears to be in about the same state, as near as we can judge, except that a serious fault has shown itself at the Newfoundland end also.

2331. (*Mr. Saward.*) Have you formed any opinion as to what kind of fault it is?—I think it must have been a fault in the manufacture. I do not think anything has gone wrong since.

2332. Have you had any means of testing to form an opinion as to whether it is a bad piece of wire?—I think it consists of several imperfections in the gutta-percha, rather than one large fault; perhaps injuries that occurred in putting on the external covering, and also straining in laying it; it might have been injured in some way of that sort.

2333. Did you make any examination of the cable at Keyham?—I did not test it for insulation, as the cable was in Mr. Whitehouse's hands. I did not consider the test of much use, as the cable was not placed under water. I passed currents through it several times from a battery, and also from a small magnetic machine.

2334. (*Chairman.*) Were those experiments for the purpose of testing the resistance?—Testing the speed and retardation.

2335. What results did you arrive at?—The results were that a current from a small magneto-electro machine, wound with small wires or from a small plate battery, would pass more rapidly than the currents from larger wires, or a battery consisting of larger plates. That was quite contrary to Mr. Whitehouse's opinion. He told me that it was no use for me to bring down the magneto machine wound with fine wire; it must be wound with very large wire. I brought two machines, one wound with very fine wire and the other large wire, and I found that the one wound with fine wire answered much the best.

2336. (*Mr. Varley.*) What was the size of the wires?—The large wire was No. 22, and the fine wire No. 32. With the fine wire instrument I sent a current through and printed with the printing telegraph words at the rate of about, I think, two and three-quarter words per minute. I brought the slip up and gave it to Mr. Saward; it was rather faster than could be printed with the large coils and batteries that they had down there.

2337. (*Chairman.*) What was the length of the fine wire in the coil?—The length was about four miles; the other wire was about a mile and three-quarters.

2338. (*Mr. Varley.*) What was about the strength of the magnet or its weight?—The one with the large wire was three-quarters of a cwt., and the one with the fine wire was somewhere about two-thirds of the size.

2339. About 50 lbs. weight?—50 or 60.

2340. You say that you got two and three-quarters words per minute?—Yes. I should like the slip to be examined, because the time is given on the slip with an apparatus that Mr. Whitehouse had.

2341. Mr. Whitehouse says that he got from two and three-quarters to four and a half words per minute?—He tried for some time with his large induction coils; I think that was before he put on the last length of wire, making it up to 3,000 miles; that made a great difference, decreasing the speed more than one-half.

2342. When you tried batteries, you say that the small plates gave a quicker signal than the large plates?—Yes.

2343. Did you compare the speed of the battery current against the induction coil current?—We did compare it, and I found it was not so rapid as the induction coil; the signal did not show so soon, it was about one-third longer to what it was with the ordinary battery.

2344. Did you transmit any words with the battery?—No; I saw them transmitted with the ordinary reversing key.

2345. Mr. Whitehouse states that the difference of speed was as 1 to 4, when batteries were used and when the induction coil was used. Did you see anything to make you fancy that such a difference as that existed?—I did not; we tried with 40 cells, and then, by putting on 200, the speed was not increased; it was just the same.

2346. (*Professor Wheatstone.*) Was not a very large magnet sent out to Newfoundland with the hope of resuscitating the cable?—Yes.

2347. Will you state any fact that came to your knowledge arising from the attempt to use it?—I tried at the Valencia end the first day it was worked, and they received stronger reversals at Newfoundland than they had received for some time; that is the statement I received from Mr. De Sauty, but no intelligible signals came; sometimes they thought they saw words, but it was a mistake.

2348. (*Mr. Saward.*) Did any scientific facts come out of that experiment?—No, not being at the other end we could not tell what was taking place.

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2349. (*Professor Wheatstone.*) Are you of opinion that any permanent damage occurred to the cable from employing the very strong induction coils, as some persons think?—No. I tried, when at Valentia, the effect of the induction coils, as compared with that from the 400 cells of Daniell's battery. I found, if a small fault existed, such as the puncture from a needle, the current from the induction coil would enlarge to about the twentieth of an inch in diameter, it would act no longer, but on connecting the battery it would enlarge to about five times the size.

2350. What was the greatest amount of force employed in any of your experiments upon the cable?—The greatest amount of force I saw employed was the current from the large induction coils, and that from the Daniell battery combined.

2351. How many cells?—About 500 cells of Daniell's connected with a key, one manipulator working that, and another the key of the induction coils, taking care that their movements should be simultaneous. The battery exciting the induction coils was about twenty graphites of a very large size.

2352. (*Mr. Varley.*) You have stated that at Keyham you tried experiments with a 200 twelve-plated battery, that would be 2,400 cells?—It must have been a mistake, and could have been 200 cells only, but I know there was no difference in speed of working between a given battery and 20 times as much.

2353. (*Professor Wheatstone.*) You do not think that the fault that has been observed in the Atlantic cable arose from the use of that power?—No; I have stated that I tried a few experiments with the induction coil and with the battery, as regards injury to the gutta-percha from powerful currents. I put a piece of gutta-percha wire in a vessel of sea water, and connected it with the induction coils. I made a small artificial fault with a needle, and on connecting one terminal of the coils with one end of the wire, and the other terminal with the water, the fault became enlarged to one-twentieth of an inch, flame being emitted from the gutta-percha under water; but when it was enlarged to that size the burning ceased, as the fault was then large enough for all the current to pass to the water. Being no longer any resistance to the current, there was no heating effect. I then substituted the 400 Daniell's for the induction coils. The burning effect then commenced again, and went on till the hole was one-eighth of an inch diameter, when it ceased. Introducing a resistance equal to four or five hundred miles of wire into the circuit prevented the battery current acting in this way, as the resistance of the wire then became equal to the resistance of the fault: consequently no burning or enlargement could take place.

2354. (*Mr. Varley.*) Do you think Daniell's battery would enlarge a simple needle hole of itself without first applying the induction coil current?—I found it did not with a small needle hole. The 400 cells had no perceptible effect on the gutta-percha, although sufficient current passed to deflect the galvanometer needle to 90°.

2355. After the hole had been enlarged by the induction coil current it would be still further enlarged by the battery current?—Yes; after making a small hole with the fine point of a needle the battery current did not appear to pass through. I think so far it made no enlargement.

2356. After the hole was once enlarged, the battery would still further enlarge it?—Yes, after the coil had done its worst, the battery would make the hole four times as large.

2357. The battery alone would not enlarge a hole so small as that?—No.

2358. In working a cable, do you think it would be safer to use a battery of 400 cells of Daniel's than induction coils, as were used on that occasion?—It is not my opinion that the induction coils injured the cable at all; neither batteries nor induction coils would injure the cable if it was in a sound state.

2359. Assuming that a small defect existed, then which do you think it would be safer to work with, a battery or an induction coil?—If a small fault existed near the shore, it is my opinion that the battery would be safer than the coils; but if it existed in the centre of the Atlantic, I do not think either the induction coils or the battery would injure it; it would make no difference which was used.

2360. (*Professor Wheatstone.*) In your opinion, what would be the best and the safest form of electric power to work through a long submarine line, a voltaic battery, an induction coil, a magnetic machine, or what?—A moderate battery would be perfectly safe, and could not injure the cable, provided the gutta-percha was in a sound state; neither could it injure the gutta-percha by burning, but it would make existing defects worse in another way, namely, by the electrotype action of the current. The copper is dissolved, and the salts of copper are deposited in the fault, and appears to saturate the gutta-percha for some distance round the hole, and as these salts are good conductors, it allows more of the current to pass. I have had opportunities of noticing much of this in underground lines, where batteries have been used. In many cases the copper has been worn completely through, and I have known instruments to have worked for weeks after the copper has been separated for a quarter of a inch, the points being as sharp as needles. The current was conducted from one point to the other by the decomposed copper, which seemed to have completely saturated the gutta-percha for an inch or more. Great part of the current must have escaped to the earth, but enough passed through the wires to give feeble signals. I have never seen this effect where magneto-electricity was used, it not being intense enough to burn the gutta-percha, neither is it sufficient in quantity to decompose the copper.

2361. (*Chairman.*) What do you call a moderate battery?—200 or 300 cells of the sand battery is a moderate one for a long line. 400 cells of a Daniell's battery would have more effect in injuring a cable than the same number of sand battery. The quantity would be larger, and the intensity somewhat more.

2362. (*Mr. Varley.*) You are aware, of course, that the quantity conducted by the cable is in proportion to the resistance and speed?—Yes; I am speaking where the fault is not near. Suppose there are a few faults in the cable near the shore, and another further off, of course there is more escape in those near the shore than in the one further off; and also other faults take away the resistance of the circuit, because it passes through those faults to the water, instead of going through the cable.

2363. In order that more electricity shall flow from a battery of low tension through those faults, is it not necessary that those faults themselves should not offer great resistance, otherwise the current will be limited by that resistance; would not that be the case?—Certainly.

2364. If you use the induction coil, or any other source of electricity that gives electricity of a much higher tension, will not more electricity pass through a fault from such a source of electricity than where you use a battery of low tension?—Yes; it might be passed through without injuring the gutta-percha.

2365. A larger quantity of electricity from the magneto machine might pass through without injuring the gutta-percha?—Yes, it would pass through small holes without making them larger. At the same time, much of the current would keep to the wire, from its superior conductability.

2366. Cannot you pass more electricity at one time than another, without producing corresponding resistance?—I am not aware of it. A current from the magneto machine is more constant than from the induction coils; also, it is not of the same burning character.

2367. To what do you attribute that peculiarity?—A sufficient quantity will pass through a small conductor with less resistance.

2368. I have been just speaking of a case in which more electricity passed through in consequence of the higher tension of the source of the electricity, the fault

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being a small one like a needle hole, the chief limit to the electricity that passed through being the resistance in the circuit; such being the case you admitted that when the tension was increased more electricity would pass through that fault; you say that electricity generated by a magneto machine has not that heating effect that electricity has generated by an induction coil or battery?—No; the current to heat must meet with great resistance, it must have a road to pass through, which is too small for it, and it has to force its way through. No heating effect takes place till there is a resistance to the passage of the current.

2369. We have been just taking a case, if you fully understand what I wished to express, that is where a fault is near the shore, and that fault is a minute one and offers so much more resistance than anything else, that being the chief resistance and the chief limit to the passage of the electricity?—It is the chief resistance in the passage of the current to the water, but not through the cable, of course.

2370. We will assume that the end of the cable is disconnected; that being the case, if you use a source of electricity which gives electricity at a high tension, more electricity will pass in that case than where you use a source of electricity of low tension, because the electricity which goes through is in proportion to the resistance in the circuit multiplied by the tension; such being the case you state that the electricity from the magneto machine has not that burning effect?—I have always found that to be the case.

2371. To what do you attribute the limit?—As I said before, to the quantity being so small. We cannot gild or silver with the magneto current without winding the coils with very large wire, thereby increasing the quantity and diminishing the intensity. We can vary the character of the current from the magneto machine at pleasure, by altering the size of the wire in the coils, decreasing the intensity and increasing the quantity, as the size of the wire is increased. With fine wire the current is similar to that from a great number of extremely small plates of a battery arranged for intensity, and for telegraph purposes small plates do just as well, or perhaps better, than large, but the zinc plates are obliged to be of considerable size, to keep up the supply of zinc constantly used. In practice I have just as good an effect by immersing one corner of the plate about a quarter of an inch square as upon immersing the whole plate.

2372. (*Professor Wheatstone.*) Will you state your opinion upon the form of cable suitable to deep water. I believe you have paid some attention to that subject?—I have tried some experiments for very deep water, for above 1,500 fathoms; I should recommend a mixture of hemp and wire, instead of all iron.

2373. (*Mr. Saward.*) Have you a specimen of the plan that you suggest with you?—No, I left a specimen with Mr. Gisborne of a cable covered with strands; the strands are formed of wire and string combined; that is to say, I take four strings and three wires, and twist them into a strand first, and then there are eighteen of those strands round the core. I sent a piece to the Adelphi to be tested, and it bore a strain of three tons two cwts.; but at the same time, although it is my own patent, I do not think it is so good as this No. 9, to speak candidly, because it will not bear so great a strain; it is rather cheaper.

2374. (*Professor Wheatstone.*) What do you consider to be the most perfect insulator?—Vulcanite is the most perfect, but it is not capable of being laid on wire very nicely.

2375. Practically, what should you prefer above all the things that you know?—I should prefer pure gutta-percha, in my own opinion.

2376. What is your opinion of india-rubber as an insulator?—Many years ago I made some india-rubber covered wire; that was about 1843, I think.

2377. What advantages or disadvantages did you find in it?—I found it was so difficult to make the india-rubber adhere; it was made with strips of ordinary india-rubber run round the wire, then passed through naphtha, and compressed by being passed under hot pressure.

2378. Does not Messrs. Silver's process do away with a great deal of that difficulty?—That is formed of strips of india-rubber, cemented together by a peculiar method. I also found that india-rubber, when exposed to the air, shrivelled very much, and opened in several parts.

2379. (*Mr. Varley.*) Did you try any further experiments with regard to speed through the Tasmanian cable?—I tried a Morse through it, and Professor Hughes also tried his instrument. The effect was just what I supposed it would be, viz., that with 240 miles of cable coiled in a tank there was very little difference in the speed of working than on short circuit. On the Morse 20 words per minute was obtained with a dexterous manipulator.

2380. Did you ascertain anything as to the speed of the wave?—No, I did not try anything as to that.

2381. (*Chairman.*) Have you made any experiments upon Professor Hughes's semi-fluid?—No, I have not; I have seen it experimented upon.

2382. (*Mr. Saward.*) I believe you have seen a good deal of Professor Hughes' semi-fluid?—Yes.

2383. (*Chairman.*) What is your opinion of it?—It appears to promise very fairly, if the inclosed semi-fluid substance will remain in the same state for any length of time as it is when the cable is made, and it does not injure the gutta-percha.

2384. (*Mr. Varley.*) Do you think there would be any danger in the use of Professor Hughes' semi-fluid, that during the manufacture of a cable any serious injuries might be healed, so as to escape detection during manufacture, and subsequently become bad when the cable was submerged?—I should not fear anything of the kind. If you wish for a statement respecting decay of gutta-percha I shall be glad to give it, having taken up a considerable quantity of decayed wire.

2385. (*Chairman.*) To what do you attribute that decay?—In many instances to the want of moisture; when the air can be excluded, and it can be kept wet, I have always found it good, and where it has been laid in a light gravelly soil, very pervious to air, it has generally gone bad and cracked.

2386. Did you lay a number of wires between London and Manchester some years ago?—I laid 10 wires between London and Manchester.

2387. Do you know anything of the state of those wires?—They were laid in 1853; a great deal has been taken up since, and overground lines put in place of them. Some of them were as good as the first day they were laid. I should say that nineteen-twentieths of the wire is quite as good as when it was first laid down; owing to some places decaying here and there the wire was pulled up.

2388. (*Mr. Saward.*) Is not there an underground line between London and Dover perfect?—Yes. About three years ago the line used by the Submarine Company between London and Dover had become so bad that they could not use it at all some days, and on others only for an hour or two. I undertook without stopping the working to take up all the wire, to bring it to London for thorough test, taking out all the bad portions, replacing with new, covering the same with tarred yarn and coating of marine glue. Since this work was done the line has continued to work perfectly.

2389. Is not that different from anything of the kind that has been done before?—Yes.

2390. (*Chairman.*) Is the marine glue applied hot?—Yes.

2391. Has not that a tendency to injure the gutta-percha?—No; it passed through a trough at a certain speed.

2392. What speed?—Two feet per second.

2393. (*Mr. Varley.*) What temperature is the marine glue when it is applied?—Melted; the temperature of boiling marine glue.

2394. Supposing the machine should stop for a

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second or two, would not the gutta-percha wire be injured in that case?—Then, I have a man in attendance who immediately lifts it out. I applied the marine glue to a mile and three-quarters last evening to lay through London streets.

2395. (*Chairman.*) Have you had any experience of the durability of gutta-percha treated in that way?—So far as the Dover line goes it has worked all right since; it is, about three years ago since it was laid down. It has been examined in different parts, and it is quite as perfect as when it was re-laid; the greater part of it was old wire. I found, on taking up the wires on the Dover line, where they laid in a low locality, which was generally wet, there the appearance of the wires was quite good; and on the top of Shooter's Hill and other places where there was light gravelly soil, the gutta-percha cracked very much, as it did anywhere where it was exposed to the sunlight; but a very short distance under ground, say, six or seven inches under ground, I found it in many cases very much cracked.

2396. (*Mr. Varley.*) From your experience in the thousand miles you have put down, do you think you could now put gutta-percha under ground for any considerable length that would remain perfect for, say, 20 years?—Yes, having laid 5,000 miles, I would undertake to lay a line and guarantee it for 20 years, supposing I was allowed to lay it as I pleased.

2397. What depth would you place it under ground?—Not less than two feet; from two to three feet.

2398. Should you cover the gutta-percha with anything?—The wires I have laid lately in London for the London District Company were No. 18, copper wire, covered to No. 7 size, then laid up spirally into a rope, then covered with a tube of gutta-percha, with as much tar as the tube would hold besides the wires; and then I covered it over with two coatings of tarred yarn. I then passed it through marine glue and sand, to prevent it sticking, otherwise it would stick together on the drum. I have laid 160 miles in London this year for the London District Company, every wire is perfect, and does not give a degree with 24 12-cells battery.

2399. Should you use that process in laying down long lines?—I should use stouter gutta-percha.

2400. Should you cover a series of wires with double gutta-percha in that way if you were putting down circuits to work through a distance of 200 miles?—Yes.

2401. You see no objection likely to arise from induction?—No.

2402. Would not that induction stop the working altogether?—I am certain it would not in 200 or 300 miles.

2403. Have you tried any experiments to ascertain that point?—I tried experiments on the line between London and Manchester. On that line the wires were all wrapped together in a double serving of tarred yarn, not in a tube of gutta-percha as used by me latterly. The gutta-percha tubing on the outside is not a perfect insulator, having perhaps small holes and flaws in it, but it is sufficient to keep the tar in its place; and I find where the tarred yarn remains perfect, the gutta-percha in every case keeps quite good. In places that are alternately wet and dry the tar washes out of the yarn, the latter rots, and the gutta-percha soon decays. I took up some wires near Calais this year that were laid in 1851 for the Submarine Company; they were laid in the earth without any trough, and without being covered with yarn at all, except that they had been bound together in a bundle with a tarred string passed spirally along at intervals to keep them together. I found every foot of the gutta-percha decayed, except where the tarred string had touched them, and the effect of that was to form a sort of spiral of gutta-percha in a sound state round the bundle the whole distance.

2404. To what do you attribute that partial action in the gutta-percha?—Chiefly to the tar. It will dry where the air has got to it; but I feel confident that tar will preserve gutta-percha, because in taking up the Dover line in a trough there were placed six naked wires and six small wires laid up spirally, covered with tar; I believe these six wires had to be pulled up in a year or two after they had been laid. I found a very great deal of decay in the naked wires, but not one single inch of decay on the 40 miles of six wires that had been covered with tarred tape. That was half-way down the line; the other half, instead of drawing six wires covered with yarn in the way the old wires had been drawn, there were six new wires not covered with yarn or tape, and they have decayed as much as the others that have been down the whole of the time. I think that is a convincing proof that tarred yarn or tarred tape is a great preservative to gutta-percha.

2405. Did you come across any instances of fungi, which have been observed by Mr. Highton to have affected gutta-percha?—I came across several instances where a white fungus had been formed round the wire; at every place where it appeared the fungus was as white as a piece of paper, and the gutta-percha cracked on bending.

2406. Did you find that in any instances where tar was present?—No, I found that after we came to the naked wires, not wires laid in with tar.

2407. You never found the fungus where tar existed?—Never in the 40 miles where the tar was present.

2408. Do you think that tar is a preservative against fungus?—I cannot say that it is; the soil might have been more favourable to the growth of fungus in that part than in others.

2409. Did you observe the effect of the oxidation of the iron on the gutta-percha?—In the iron pipes through London the oxide of iron formed on the gutta-percha is very small, not $\frac{1}{10}$th of an inch, and it has not injured the gutta-percha in the slightest degree.

2410. Have you had any experiments made upon gutta-percha covered wires enclosed in lead?—No.

2411. Nor india-rubber?—No; I have seen it enclosed in wooden troughs, in iron pipes, and laid in the earth without any protection.

2412. (*Chairman.*) In the wires that you laid between London and Manchester, and Liverpool, did you make any experiments upon the induction or retardation?—Yes, I found very great retardation in the wires laid between London and Manchester. That being the first line, of course it had not been brought to my notice before; but I had made machines purposely for working through this line, which would work through 300 or 400 miles, then lying at the Gutta-percha Works, with the greatest ease. Of course I thought I should be able to work to Manchester, but when I completed the line I found that the machine that would work through 300 or 400 miles at the Gutta-percha Works, was of no use at all in working through 195 miles to Manchester; that led me to experiment, and I found that the retardation was very great, even through that 195 miles.

2413. Was the gutta-percha wire that you experimented upon, under water or not?—Both in water and out. I tried 1,500 miles of No. 4 with the current from a magneto-machine. When the wire was coiled in a warehouse there was no difficulty in working through the whole length with a very small machine, and with no apparent interval of time between the signals at the extreme ends, but on placing the coils or wire in the water there was a remarkable difference. With a large machine I could only get feeble signals, and the retardation was very great, a quarter of a second elapsing between the signals at the termination of the wire. The Gutta-percha Company then had a large stock of wire on hand, as, besides myself, the European and American Telegraph Company were then laying wires to Manchester.

2414. To what do you attribute the extremely slow transmission through the 2,000 miles of the Atlantic cable, which was only one-third longer?—The re-

tardation on 1,500 miles of Atlantic cable when laying at Keyham was certainly very much less than through the 1,500 miles of wire tested at the Gutta-percha Works. This, in my opinion, was due to two causes, viz., from the large conductor and from the leakages from faulty insulation, as it is well known that a perfectfully insulated wire retards the current more than one that is just faulty enough to allow the charge to return to earth. When the wire was coiled in the warehouse it could not receive the charge as when in the water, because there was no outside conductor, consequently there was no retardation in working through; but a cable becomes charged whether in the water or out, on account of the metal covering on the outside. When to the 1,500 miles of Atlantic cable another 1,000 miles were added, it appeared to increase the difficulty of working immediately. It did not appear to me that what is understood as retardation (that is the delay in passage of current) was so much increased, but the current was rendered so feeble as to be scarcely sufficient to record the signals. This effect might be due to defective insulation, as although a slight fault may do no harm, several may do much mischief, as the current would leak through and return without reaching the other end.

2415. When you tried the experiments through the 1,500 miles of wire at the Gutta-percha Works, how was your apparatus connected?—Just in the usual way: one instrument at one end of the wire, and one at the other.

2416. Did you make any use of the earth, or connect them in one line?—I tried at first with an earth-plate at one end only, and a wire led into the water at the other.

2417. At which end was that; was it at the magneto end?—I had an instrument at each end connected with the gutta-percha wire, the other terminal for one instant to an earth plate, the other a piece of wire only led into the water. I did this to try the effect of large and small earth plates.

2418. (*Mr. Varley.*) Had you two distinct plates in the water?—Yes. I also tried with two earth plates, and also with wire instead of water.

2419. With the wire across?—Yes.

2420. And you only observed in 1,500 miles of wire a retardation of a quarter of a second?—As near as I could measure it.

2421. (*Chairman.*) With that retardation, at what rate could you send signals through the wire?—I could send signals at the rate of 10 or 12 words per minute, and from that to 14.

2422. (*Mr. Varley.*) Did you?—Yes; the signals were very feeble; of course it required a practised eye to read them.

2423. That result being so different to what was obtained with the Atlantic cable and the Red Sea cable, do not you think there was some mistake made as to the connexion of the wire to the sand? Did you have the whole in circuit?—I had the whole in circuit; I did not measure the time by a watch.

2424. (*Chairman.*) It was a sort of assumed time?—Yes; I can judge pretty correctly as to intervals of time. I ought perhaps to have taken down the exact time, but it did not then appear to me of very great importance. I have tried many other experiments since, but my time is chiefly occupied in practical operations.

2425. Did you get the same result from the Atlantic cable, when you tested it?—The current was very much reduced in strength in passing; at the same time it is very curious how feeble a current would pass through the Atlantic cable. I tried a shilling and a piece of zinc, with a piece of paper between, just moistened, and the current produced a deflection of 25° upon a delicate galvanometer through the 2,500 miles.

2426. (*Mr. Varley.*) Is not that what you would have expected?—Yes; but I do not believe you would get it, if the cable was laid out in one straight line.

2427. Why not?—From many experiments I have tried through underground or through cables.

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2428. Do you think that when a cable is coiled it is a better conductor?—The inductive effect of one coil upon another has a tendency to increase the current. I have been experimenting upon the Tasmanian cable of 240 miles, and I used one 12-cell battery with a short circuit; I could get no spark. I connected it with the Tasmanian cable, and I got a spark.

2429. (*Chairman.*) Was that laid in coil?—Yes.

2430. (*Mr. Varley.*) When the cable was stretched out, did you observe a different result?—I have not had an opportunity of trying it. I cannot suppose that I should get any such result as that.

2431. Do not you think that the spark was simply the result of accumulated electricity by induction?—I think it was.

2432. How would that affect the induction through the whole coil, when compared with its being stretched out in a straight line? Have you tried any experiment in that way?—We know, Faraday tried experiments on copper ribbon several yards long twenty years ago. When the ribbon was stretched out in a straight line no spark was visible in passing current through; but when coiled up a bright spark with noise was produced in breaking contact with the battery. Perhaps I may be allowed to add to my evidence respecting decay in gutta-percha wire, that I have found many different kinds of decay besides the dry state. In taking up the Dover line I found several wires in which the gutta-percha had become very similar to leather, and would absorb wet. This was the worst kind of decay, as when it occurred it would run for miles, whereas in the other kind of decay there would be found alternately good and bad places. Slight warmth is very detrimental to the wire. On first laying down wires in the streets of London I did not divert the line away from bakers' ovens, but experience has proved to me that a continuance of 90 degrees of heat would in three or four years dry up the gutta-percha, and render it quite useless. In passing ovens now I always carry the wires into the carriage way. Judging from this I am afraid gutta-percha cannot be used for land work in hot climates. I may also mention that I have taken up wire on other lines which appeared quite sound on the outside, with the exception of a slight black speck here and there, but upon opening at these specks I found that each conducted to a large cavity in the gutta-percha, the same having decayed next the copper first and working outwards.

2433. (*Mr. Saward.*) Do you recollect the fact, that in laying the line, wherever there was drainage from oak trees, or even oak posts stuck into the ground, just at that point there would be a piece gone?—Yes; there was a short line that I laid in 1854, from Liverpool to Preston. I covered it with two contings of tarred yarn, and that I believe is quite perfect; it has never been meddled with since; they have never had a spade in the ground. The line from Dublin to Belfast is in tolerably good condition, but it is decayed in places; the water passes through wooden pipes, and washes the tar out of the yarn; the yarn rots, and then the wire soon decays.

2434. (*Chairman.*) Have you paid out any cable?—I was at the paying out of the first iron cable from Dover to Calais.

2435. Have you seen any other cable paid out?—No.

2436. Have you any evidence to give the Committee upon the subject of paying out cables?—I have sent out a small machine for laying a line from Ceuta to Algesiras, which was employed in laying a piece of the Mediterranean cable.

2437. What is the principle upon which that machine is constructed?—It consists of a drum with a ring revolving with it. This ring is by rollers caused to form an incline, and shifts the cable across the drum to prevent its overlapping. The old machines are something on the same principle, but there

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the incline or knife is fixed, and if a broken wire catches it the cable is injured, and perhaps broken.

2438. Has the cable been laid between Ceuta and Algesiras?—No, there was only one week from the time of receiving the order to when when we shipped it; when it got out there, the Spanish Government had not made up their minds as to where they would have it laid.

2439. In what depths will it be laid?—About 700 fathoms.

2440. Is there any depth of that sort between Algesiras and Ceuta?—It is so in the chart, and going a little further out, it is 900 fathoms. In the Mediterranean, on trying to repair the Malta cable, where they expected 30 fathoms, they found 300.

2441. (*Mr. Saward.*) Were they in the proper course?—Yes; they found no bank such as was laid down in the chart.

2442. (*Chairman.*) Are you manufacturing any other cable?—I have 400 miles to make and lay by next June, which I shall go out with; that is to connect the Balearic islands with Barcelona.

2443. For the Spanish Government?—Yes.

2444. What construction is that?—The cable from Barcelona will be one-wire conductor, 14 strand, covering 16 Nos. $12\frac{1}{2}$. Between the island and to Cape St. Martin's the cable will consist of two wires, each conductor same as used in Hanover cable, and covered with 18 No. $11\frac{1}{2}$.

2445. (*Mr. Varley.*) What will be the greatest length on circuit?—183 miles. The greatest depth will be about a mile and a half. I have to make it, and lay it at my own risk.

2446. (*Mr. Saward.*) What did you cover the core with?—Iron wire. We are supposed to get fine weather in June, and, with proper machinery, I think the iron wire can be laid at a depth of a mile and a half, or between 1,400 and 1,500 fathoms.

Adjourned to To-morrow, at One o'clock.

Saturday, 17th December 1859.

PRESENT:

Captain DOUGLAS GALTON.
Professor WHEATSTONE.
Mr. SAWARD.
Mr. VARLEY.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

W. Thomson, Esq., LL.D., F.R.S.
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WILLIAM THOMSON, Esq., LL.D., F.R.S., examined.

2447. (*Chairman.*) You are professor of natural philosophy in the University of Glasgow?—Yes.

2448. You have devoted, I believe, considerable attention to the science of electricity for some years?—Yes, for some years.

2449. I think you became a director of the Atlantic Telegraph Company in 1857?—Either the end of 1856 or the beginning of 1857.

2450. Previously to that, had you given a good deal of attention to the questions affecting conduction through long circuits, and the laws of induction and retardation?—Yes, I had made a communication upon the subject to the Royal Society, which was published in the "Proceedings" for the 24th of May 1855, consisting chiefly of two letters written to Professor Stokes in October and November 1854.

2451. You arrived at conclusions which, I believe, at that time indicated to you the proper dimensions of conductor and insulator respectively to each other for long cables?—I arrived at the law connecting the rapidity of signalling with the proportion between conductor and insulator, but I did not lay down decidedly the best form, because considerations of strength, bulk, and expense could not be taken into account with the data which I had.

2452. Will you be good enough to state briefly the law that you laid down?—The conducting power of the conductor is, as is well known, in simple proportion to the area of the section, and in inverse proportion to the length, when the quality of the metal is constant; that law having been laid down by Ohm many years before. The capacity of the conductor for charge, or the electrostatic capacity as it is technically called, which influences most seriously the rate of signalling through it, I shewed to depend really on the ratio of the diameter of the gutta-percha to the diameter of the copper, and to be independent of the absolute diameter of either. The mathematical expression which I have found for the electrostatic capacity per unit of length is the specific inductive capacity of the gutta-percha divided by twice the Napierian logarithm of the ratio of the diameters. I concluded also that the rate of charging and discharging the cable must be proportioned to the square of the length, other things being the same, because for double or triple length, for example, the capacity is double or triple, and again, the freedom of entering and of getting out is reduced to half, or the double or triple quantity is to be charged with one-half or one-third of the facility of conduction, and therefore four times or nine times the time will be occupied; such in general terms is an illustration of what has been called the law of squares, which I laid down in the published communication already mentioned. Now, it appears that the rate of signalling depends ultimately on the rapidity with which charge and discharge can be effected. I say, "depends ultimately;" but before we reach this limit, there are many other considerations as regards the sluggishness of the instruments, the system of more or less convenience for manipulation, and the susceptibility of the system for accuracy, all of which are, to some extent, uncertain. When these various circumstances are met in the most advantageous possible way, we come to a rate of speed in a line of 200 or 300 miles, which far exceeds the ordinary working rate. In fact, not only theoretically, but practically, a rate of working in 200 or 300 miles, exceeding very much any ordinary rate, is attainable. A machine could be got which would be worked with certainty through 200 or 300 miles at a very much greater rate than has ever been attained yet by any instrument in practical use. In estimating the speed of working through a long line, we must not compare a hitherto practically attained speed through a short line with the attainable speed through a long line, according to the law of squares, but we must consider the mere mechanical difficulties of very rapid action to be so perfectly overcome, as to give an extremely high speed in short lines; and starting from such a foundation as that we may proceed to estimate, according to the law of squares, the rate of signalling through any length. Now, the fact is, that it is easy to approach something like an ultimate limit in a very long line, because in doing so we are not tried with mechanical difficulties which depend merely upon extreme rapidity. If, then, we can get three words a minute through 2,000 miles for example; through 200 miles there would be 300 words a minute possible, and I say that 300 words a minute are possible; but the excessive speed in every department of the work required for producing 300 words a minute, makes it economically impracticable to transmit at so great a rate through telegraphic line, however short.

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2453. Because we have no instruments which will register with sufficient rapidity?—Not so much that as the difficulty of the manipulation required for *sending* at a very high speed. It will be understood that I refer only to systems which have been actually carried into use in telegraph offices. I am aware of improved instruments in which the mechanical difficulties have been got over to a great extent, such as the instrument of Professor Wheatstone to get some such speed, so far as I can judge, as I have mentioned. I merely mention those numbers by way of illustration, and to point out how it is that the law of squares might be misunderstood. I guarded sufficiently, as regards the science of the question, from any misunderstanding in my first communication, but some illustration, perhaps, is necessary to make it quite clear; suppose we take such a rate as is practically attainable and frequently attained, through, say, 200 miles 10 words a minute, 15 words a minute, 20 words a minute, which are frequently worked speeds.

2454. (*Mr. Varley.*) The speed is frequently carried up to 28 words, and sometimes to 33 for a short time of transmission, through 200 miles of underground 16 gauge wire, yet the limit of speed is not dependent upon the apparatus, but the manipulation of the clerk?—Exactly; let us suppose that a speed of 40 words a minute is attained through 200 miles. According to the law of squares, if such were the limit depending on inductory embarrassment, we should only have a 100th part through 2,000 miles, that would be 0·4 of a word per minute. Now, we need not experience any such limit, the fact being that when we come to great lengths, the mere mechanical difficulties of keying disappear comparatively, and allow us to touch the inductive difficulties, while at the short lengths the mechanical difficulties affecting the speed arising from the keying are almost all we meet, and we scarcely notice the inductive obstruction except in residual phenomena.

2455. In the case of the Atlantic telegraph had you not half as much again weight of copper per mile as is usually the case in wires that are laid for 200 miles circuit, and a much greater thickness of insulating medium than is generally used in 200 miles circuits, which would account to a great extent for the greater rapidity through 2,000 miles than you anticipated?—Certainly. I had pointed out that the way of trying to diminish the inductive difficulties for a great length was to increase the mass of the conductor, or to increase or maintain undiminished the ratio of the diameter of insulator to the diameter of conductor. This was published before-hand. I will not say what influence the publication had on the determination of the dimensions of the cable chosen for the Atlantic, but it was only when I saw a specimen of cable that had been agreed upon, and saw that it fulfilled the conditions which I pointed out as required to diminish the inductive difficulties that I gave my approval to the first proposal which led to the formation of the Atlantic Telegraph Company. The weight of copper was 110 grains per foot.

2456. (*Chairman.*) I believe a discussion arose upon the subject between yourself and Dr. Whitehouse, who subsequently became the electrician of the Atlantic Telegraph Company?—Yes.

2457. Did the result of this discussion produce any modification of your original views upon the subject?—None whatever. Mr. Whitehouse's objections to my theory were brought forward before a meeting of the British Association, at Cheltenham, during my absence in Germany. On my return I found a notice of his communication in the "Athenæum," describing experiments by which the author had, as he supposed, overthrown the law of squares. I wrote a reply, which was published in the "Athenæum," on the 1st of November 1856, in which I pointed out that the law of squares remained altogether untouched by the experiments that Mr. Whitehouse had made. In that communication I pointed out certain practical conclusions that I had arrived at as to the speed of working attainable by an Atlantic cable of dimensions such as previously made submarine cables had been. I showed that a considerable augmentation of speed would be attained by adopting a conductor and an insulator of the dimensions which I mentioned; and I said that I believed three words a minute could be obtained through 2,400 miles of such a cable. The dimensions that I mentioned in that communication are, so far as I can now recollect, very closely the same as those which I found a few weeks later adopted in a specimen of cable proposed for the Atlantic Telegraph.

2458. Soon after you became a director of the Atlantic Telegraph Company, was your attention directed to the conductivity of copper?—Yes.

2459. You instituted a series of experiments, did you not, to determine the variation of this quality in different samples of copper?—A number of samples of copper were, at my request, put into my hands for the purpose of measuring their conductivity in consequence of my having accidentally noticed differences greater than I expected in the conducting power of one or two samples which I had had previously.

2460. Will you be good enough to state the general results at which you ultimately arrived, and your modes of experimenting?—My modes of experimenting did not differ materially from the methods which had been followed by certain other experimenters, especially in Germany, and were in reality all based on Professor Wheatstone's invention of a beautiful method for comparing resistances, to which I have frequently referred as Professor Wheatstone's electric balance.

2461. What were the results at which you arrived?—That different specimens chosen at random from the stock supplied for manufacture differed immensely in conducting power.

2462. Although nominally the same quality of copper?—Yes, although nominally the same quality of copper. All those specimens of wire were supposed to be of the very best quality, the only copper supposed to be good being that which admitted of being drawn into wire suitably for the purpose. A good mechanical quality was necessary to prevent frequent fractures in the wire-drawing, and to understand that I should say that hanks in unbroken lengths amounting to a large mass were always required, the worse metal being found to break before it could be drawn into a hank of a certain size. The mechanical qualities seem to have been satisfactory, but no suspicion whatever was entertained that there were also large differences in electric conducting power. W. Weber had many years before pointed out considerable differences in different specimens of copper wire which he had tested. I found differences much exceeding those, and I did not, as I expected, find any approximation to a uniform average among the different specimens tested; some specimens I found nearly double in their conducting power, compared with others, reckoned according to the weight and length, allowing for the variations of gauge. Calling the best specimen which I had in the summer of 1857, 100, I found many specimens standing at 60 in specific conductivity, many standing at 50, many standing at 80, a few above 80, and so far as I can recollect, the average of a large number of specimens that I then examined may have stood between 60 and 70, but I consider the statement of such an average to be of no value, it is so much a matter of chance. If I had received a dozen specimens of a low quality below the average, or if I had chanced to receive a dozen specimens of a higher quality, the average would have been so much the lower or the higher. I never had an opportunity of measuring the conductivity of 200 or 300 miles of submarine cable; such alone would have given me exact information as to the average for that portion of cable. I may mention that a month or two later, still in the summer of 1857, I received specimens of wire which were in stock for submarine telegraphs, for some of the Mediterranean telegraphs, I believe, which stood as low as 43 on that scale; and, lastly, I may mention

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that I have since met with specimens standing two or three per cent. above the 100, and an artificial alloy, which I had prepared, stood, so far as I can estimate, as high as 113.

2463. What was that alloy?—The alloy consisted, so far as I can recollect, of copper and a quarter per cent. of lead. I have made experiments upon a series of alloys, in all about 43 or 44, and have recently repeated the examination so as to arrive at accuracy, within certain limits; and I expect, immediately, to be able to communicate to the Royal Society, for publication, the results. A few months ago I sent a provisional list of the specimens, shewing the relative conductivity of those alloys, but, possibly, requiring correction as to the absolute conductivity stated. That list was communicated to Mr. Latimer Clarke, and, I believe, a copy of it was laid before the Committee.

2464. (*Professor Wheatstone.*) Were you quite certain that you employed pure copper in your experiments?—I could not be quite certain.

2465. The copper might be alloyed with other things than metals; is it not very probable that it might contain some sub-oxide, and that the mixing of lead afterwards with it might have reduced the sub-oxide, and therefore have given it a higher conducting power on that account?—That is possible. I cannot say that I am at all satisfied that the experiments which I have made point out distinctly the relation between the ascertained chemical combination, and conductivity. I may mention that one of my alloys was made with a sub-oxide melted with the copper; but the uncertainty of the process of melting the sub-oxide and the uncertainty as to how much of the oxidation may have disappeared in the melting, prevented me from attributing much weight to the experiment.

2466. (*Chairman.*) What was the result with that alloy; was it a low result, or a high result?—A moderate result; not a low result.

2467. But not a high one?—A somewhat high result; but I may mention that in one series the highest conductivity was found with a mixture of lead and iron; fractions of a per cent. of lead, and fractions of a per cent. of iron mixed with pure copper gave a higher conductivity than a nominally pure copper, with which the alloys were prepared. I must mention further, that in two series the alloys, both prepared by Messrs. Matthie and Johnson, and as I have been assured with equal care, gave results presenting considerable discrepancies; the conductivity of the pure copper in the first stood high, nearly agreeing with the 100 of my first scale, the pure copper of the second series fell considerably below that limit. On this account it appears, that even pure copper carefully prepared by the electrotype process does not always give us results which shew perfectly in point of conductivity; but to make such experiments in a satisfactory manner, it would be necessary to have a thorough chemical investigation, both synthetical and analytical of the metals used; such a thorough investigation I have not been able to carry out, in consequence of the large expense which it would entail. I may mention that Mr. Mathiessen has gone through a series of experiments on alloys, of which the chemical composition has been ascertained with all possible accuracy, and has, I believe, arrived at highly important results relative to electrical conductivity. I have been in communication with him, and have supplied him with a specimen of one of my standards. He mentions to me that he has obtained specimens conducting better to a considerable extent than the 100 of my first scale. In that respect he has confirmed what I have myself ascertained, having myself found specimens as high as 113 on that scale. A number of alloys of definite chemical composition, prepared with great care by Mr. Calvert of Manchester, and already tested by him for thermal conductivity, and for mechanical properties, have been put into my hands, in order that I may measure their electric conductivities. I hope soon to be able to obtain and publish results for this series of alloys.

2468. I believe that the last 400 miles of the strand of the Atlantic cable were made of wire selected in accordance with the principles recommended by you?—In the autumn of 1857 a system of testing the wire brought to the gutta-percha works for manufacture into submarine cables was commenced, and much of the wire that was brought was rejected by that test.

2469. Will you describe the system which was pursued?—The system was one in which Professor Wheatstone's electrical balance was used. It was not exactly in all particulars arranged as I would have done it myself, but was, I believe, arranged to give to a considerable extent useful results. My absence from London prevented me from carrying it out exactly in the way that I considered best. Mr. Whitehouse, according to the explanations I gave him, established a system, which was carried on as long as the construction of the Atlantic cable was in hand. I am not aware what the improvement that was effected in consequence of that process was, because I have had no opportunity of measuring the conducting power of any considerable length of cable, but I know that now copper of high conductivity can be had from the Gutta-percha Works.

2470. Have the Gutta-percha Company continued that system of testing?—I believe so, I do not know exactly how it is conducted, but I know that the company obligingly sent me a quantity of copper when I wrote for it at any time, and I found it to stand almost always very high in conducting power. I judge, therefore, that they still select it, and probably have even increased the rigour of their test. This is only inferential, and it would be desirable to have information as to their own results.

2471. (*Mr. Saward.*) Are you aware that a thermometer is also employed at the Gutta-percha Works to measure the conductivity by the heat evolved?—I am not aware that it has, although from Mr. Joule's early investigation of the law of the generation of heat by electric currents, I know that a thermometer can be so used. I may mention that in the last specimen that I had from the Gutta-percha Works, a few weeks ago, of copper, selected for high conductivity, stood as high as 103; a quantity of this having been sent to me for the purpose of being drawn into fine wire for making electrical instruments.

2472. (*Chairman.*) Can you say if the whole of the strand of the Atlantic cable had been selected according to your recommendation, whether the speed of transmitting signals would have been materially increased or not?—Certainly very much increased:—increased by at least thirty per cent. I believe that the last 400 miles were considerably better, possibly 10 or 20 per cent than the first part of the cable. Mr. Whitehouse estimated the gain in conductivity, owing to the adoption of my suggestion, to have been 25 per cent., I believe, but I am not aware of the experiments upon which he founded his estimate; but of this I am sure, that by selecting the specimens rigorously, and only choosing the best quality, 25 per cent. above the whole average speed of the Atlantic cable would have been secured.

2473. To what extent would the strength of the signals received at the end of the cable distant from their transmission have been improved by selecting copper according to your recommendation?—With the insulation perfect, the strength of the signals would have been improved by improved conductivity, in the ratio of 70 to 100; the proportionate improvement in the strength of the signals will be in reality greater, however, in consequence of the loss by defective insulation being nearly the same in absolute amount of leakage from the wire of low, and from the wire of high conductivity.

2474. Did not you continue to take an active interest in all the scientific operations of the Atlantic Telegraph Company from an early period after you became a director?—Yes.

2475. Did you make any test of the cable in 1857 before it sailed?—I had no opportunity.

2476. In the spring of the year 1858, I believe you acceded to the request of the directors that you would

give to what extent you were able a somewhat more active supervision over the electrical affairs of the company than you had hitherto done?—Yes.

2477. In compliance with their request, did not you investigate, from time to time, the state of the cable and apparatus for working through it, for the information of your colleagues?—I had comparatively few opportunities of investigating the state of the cable; indeed, I may say, I had no satisfactory investigation of the state of the cable, because it was actually on board ship before I was able to be on the spot, and joined up in such a manner that the testing of the different parts was not practicable. The experiments which I made in Devonport in the May of that year were confined to experiments through the whole length, and as I had only fourteen days before the ships were cleared out of the harbour, they were necessarily of a very limited character; they were entirely directed to ascertaining the speed of working that would be attainable through the cable; they were not made at all with the view of testing the insulation of the cable, the tests for insulation having been regularly conducted by Mr. Whitehouse as the cable was paid out of the tanks in which it lay during the winter, into the ships.

2478. Will you be good enough to state the result of your observations up to the time of the sailing of the earliest expedition in 1858?—In the course of experiments on signalling through the cable, I occasionally made experiments of the strength of the current entering the cable and the current leaving it. When the battery was applied to one end, and the other was put to earth, these experiments, and others slightly varied in detail, shewed the insulation to be much better at one end of the cable than at the other. Although I had not undertaken the testing of the cable, still that circumstance having come to my attention, I thought it right to mention it to Mr. Whitehouse, and he assured me it was just as he expected, and just as he believed it must be, because one end of the cable contained a quantity of newly manufactured cable, which he had found by all his tests insulated much better than the old cable. Another reason that prevented me from having much opportunity of judging as to the real insulating condition of the cable was, the circumstance that the different parts of it were at very different temperatures, and I was aware from experiments that I had made myself, as well as from the testing of the cable in the spring of 1857, that even a slight variation in the temperature of gutta-percha makes a very large variation in the insulating power. This circumstance being taken into account, and the statement of Mr. Whitehouse that the better insulation at the one end of the cable than the other was just as he expected, prevented me from drawing any serious inference as to the considerable difference which I observed.

2479. I believe you accompanied the first expedition which sailed in 1858, at the earnest request of your colleagues, for the purpose of ascertaining the exact electrical condition of the cable during the period of paying it out?—Yes.

2480. Will you be good enough to state the events which happened to the cable from that period to the return of the ships to Queenstown in the second week in July 1858?—There was first an experimental cruise in the Bay of Biscay.

2481. Will you give the Committee an account of the experimental cruise?—The ships proceeded to sea about the end of May, and went to a point selected in the Bay of Biscay. During the voyage out I made several experiments upon the portion of the cable on board the "Agamemnon," and tested both the insulation and the rate of signalling through it. The results were, on the whole, very constant, and showed very nearly an equality, but not a perfect equality, between the insulation of the two ends. The system of testing which it was intended should be followed in the laying down of the cable was attempted during the operations of the experimental trip. In those operations a portion of the cable was carried from the one ship to the other, and a splice made, precisely as it was intended should be done during the laying of the cable, and the splice was lowered in very deep water, 2,600 fathoms, I believe, according to the soundings taken at the time by the "Gorgon." The system of testing had been arranged entirely in my absence, before I came to Devonport; and the operators had been practised to a certain extent in working it. It consisted in applying batteries to the two ends opposite to one another, and observing in each ship the indications of a common detector; and, also, of Mr. Whitehouse's instrument, which he calls a magnetometer. The kind of signal which it was proposed should be made was to put either end to earth, instead of to battery. Arrangements were made for conducting this process at the two ends, and the operators waited for some time, but could not tell whether there was any current through or not. I went several times from the one ship to the other, and met Mr. Field in the "Niagara." I assured him that I would show him a signal coming through. I came along with him to the "Agamemnon." I there directed that the battery should be thrown off altogether, and the detector retained: and, also, my marine galvanometer, which I brought out for my own trials, thrown into circuit. I may observe, that before that I had mentioned my intention to keep my marine galvanometer in circuit, although the instruments upon which reliance was supposed to be placed for testing, were instruments for which I was not in any way responsible. But before going to sea I made it a condition that permission should be given to have my marine galvanometer in circuit, because by it I expected to have more definite information than I should be able to have by watching the tests that were prepared. I gave directions that at a certain time mentioned the battery should be applied for a certain time, and then reversed for a certain time, and then applied direct for a certain time, and after a few minutes of such operations, the end of the cable should be put to earth; that was to be done in the "Agamemnon." I then returned with Mr. Field to the "Niagara," and I pointed out to him the needle of the detector, and asked him to look at his watch for the appointed time. At the appointed time the needle of the detector moved clearly over, remained a certain time, and then turned over to the other side, giving perfectly clear indication of a signal through, and the same signals were observed by my assistant Mr. Macfarlane on my marine galvanometer, in the "Niagara," and were recorded. No other indication of a current through the whole cable, so far as I am aware, than that, was obtained during the experimental trip. I attributed no importance to the result myself, because it only tested the insulation of the piece of the cable which was submerged between the two ships; that was a piece of cable supposed to be bad, and if the test had failed, I should only have attributed it to the badness of the portion that was submerged. I felt that no such test could alter my own conviction as to the certainty with which the current would be transmitted through the submerged cable, but for those who had not the same conviction, no doubt the mere fact of transmitting a signal through a cable any portion of which was submerged in such deep water was considered of some importance.

2482. What was the depth?—2,600 fathoms. The operations of the experimental trip were chiefly conducted for the purpose of ascertaining what could be done not only in the way of laying the cable in very deep water, but also in the way of lifting it after it was laid. The engineering operations have been described in the printed reports of the company, and I need, therefore, not say anything more of them at present, with the exception of this remark, that they seemed to indicate a necessity to change one part of the arrangement of the wheel work, namely, the proportion of the diameter of the grooves of the paying out pulleys; and Sir Charles Bright and myself both urging very strongly upon some other

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members of the engineering department the absolute necessity of doing so, led to this change in the wheels being made at Keyham on the return of the experimental squadron.

2483. Was the piece of cable which had been submerged brought up again?—The splice was brought up from 2,600 fathoms.

2484. Did you see the splice after it was brought up?—I cannot recollect for certain whether I saw the splice. I am certain that it was brought up from 2,600 fathoms. I may mention that no further electrical experiments than those which I have described were performed through the whole of the cable when connected; but the connecting piece which was used in the subsequent operations, was used for the sake of communication between the two ships, and messages were passed by clerks with the ordinary single needle instrument regularly through, and they confirmed the transmissibility of signals through even a very defective cable submerged in great depths.

2485. You subsequently proceeded with the expedition which was unsuccessful in laying the cable?—Yes.

2486. Were there any events upon that expedition which you think would be interesting to the committee?—The description of the gale which lasted for about ten days has been published, and I need say nothing of that. As to the way in which the electrical instruments were affected; I may mention that the electrical cabin was flooded. The instruments, however, did not suffer, it having been thought advisable previously to remove them. As soon as the gale abated, the electrical cabin was altered somewhat in its arrangements, and the instruments were put into a position in which they could be screened from water in case of the recurrence of any such circumstance.

2487. Were there any interesting circumstances attending the paying out of the cable on that expedition?—There was the first accidental fracture on board the "Niagara" after a few miles had been paid out; this was indicated by the instantaneous stoppage of signals. We were still in sight of the "Niagara," and perceived her immediately after putting about. As soon as we met a communication was interchanged, and explanation given, and we started again. In the evening of the second attempt to pay out, the signals ceased, and the indication was of a very thorough failure of insulation. The first indication which I had of the failure was at the time when the "Agamemnon" signals were being sent by an arrangement that I had made in the meantime as to testing, and the character of those signals shewed that there was a sudden loss of the insulation, and immediately after came the period at which the "Niagara" should have sent to us, but nothing came. The testing continued to show very defective insulation, but there being 1,500 miles of cable between the point at which the battery and galvanometer could be applied and the portion paid out, it was difficult to make quite sure what the character and locality of the fault might be; I accordingly begged the engineer, if possible, to give me an opportunity of testing a shorter length. He arranged to do so on a portion not very many miles from the paid-out part, but while making their arrangements for doing so, in which it was necessary to diminish the speed of the ship, and to hold the outgoing part of the cable fast, the cable parted close under the stern of the "Agamemnon." The failure had been so complete, that I could not doubt but that there was nothing whatever remaining for us but to cut away the cable, and return, even before the cable had decided the matter for us, by parting under the stern of the "Agamemnon." We returned with the "Agamemnon," and met the "Niagara," and were surprised to find that they were unable to explain the cause. They had found precisely similar indications of failure of signals and of defective insulation; and, if I remember rightly, after remaining a certain time, they cut away the cable from the ship, and returned, according to a pre-arranged plan. We joined, started again, and everything went well; the signals were regularly interchanged, until the evening of the second day, if I remember rightly. When the whole of the upper deck coil on board the "Agamemnon" had been paid out, and the speed of the ship was diminished with a view to beginning paying out from one of the lower coils, without the slightest warning of any danger the cable parted below the stern of the "Agamemnon." But I now believe that the risk which, on that occasion, proved for the time fatal, may be very much diminished, or altogether done away with, by some slight modifications in the paying out machinery.

2488. (*Mr. Varley.*) Did you in the experimental trip observe any electrical variation of the cable as to its state of insulation, or the transmission of the currents through it, owing to the effect of the tossing of the sea?—No; they remained very constant, we did not find any difference.

2489. (*Chairman.*) You sailed with the final expedition in the "Agamemnon" on the 17th July, I believe, did you not?—Yes.

2490. Will you state what arrangements were made for testing the cable on board the "Agamemnon?"—On the return of the experimental expedition, I had found it necessary to take the arrangements for testing entirely under my own direction, and to adopt the plan of transmitting certain pre-arranged signals from one ship, and receiving in the other ship alternately; during definite intervals of time; the single application of which plan, in the Bay of Biscay had given the only successful signals through the whole length in the experimental trip. The time was so limited that it was quite impossible to arrange such a system as I should have thought satisfactory, but I arranged what seemed to me at the time the best that circumstances admitted. There had been a strong desire expressed on the part of various officers of the electrical department to have no communications whatever, except mere signals; to have no interchange of words, and to have nothing but testing currents; I acceded to this desire, and accordingly during the first attempt to lay the cable the system was merely this, that during the first ten minutes of each Greenwich hour, a series of signals were made; first a reversal every minute for five minutes, then a constant current in one direction for five minutes were sent from one ship. During the next ten minutes a similar series of signals were sent from the other ship, and this system of giving alternate series of reversals, each series from each ship lasting ten minutes, was continued during all the operations of the second expedition.

2491. You do not consider that the best system that could have been adopted?—It was not so complete as it might have been; the instruments used were my own marine galvanometer and one of the ordinary detectors.

2492. Will you be good enough to describe to the Committee the construction of your marine galvanometer?—The marine galvanometer consists of a very light steel magnet cemented to the back of a very light mirror, the weight of the whole being from a grain to a grain and a half. This was attached to a platinum wire in the first two instruments, which were used in the "Agamemnon" and the "Niagara," but I prefer now suspension by means of a stout bundle of silk fibre. In this first instrument the needle was directed by the elastic force of the platinum wire and deflected from its position of equilibrium by the magnetic force of a current from the cable circulating in a coil of fine wire round the magnet. The peculiarity of the arrangement consists in the needle and mirror being balanced by their centre of gravity, and therefore unaffected either by gravity or by the inertia called into play by the pitching and rolling of the ship, and further, the very high directive force imparted by the elasticity of the platinum wire, which prevented the influence of terrestrial magnetism from producing any considerable effect on the place of the needle. This needle took its zero position uninfluenced by the pitching of the ship or terrestrial magnetism, or only in a slight degree influenced by terrestrial magnetism; and when a current passed through

the coil surrounding it, it took the position of equilibrium under the action of the elastic force of the wire and deflecting force of electro-magnetism. I may add that I prefer the directing force of strong steel magnets to the directing force of elasticity, and in the last instrument that I have made I have substituted such. The angular deviations of the needle were determined by observing the image of a lamp reflected from the attached mirror, and thrown on a scale rigidly connected with the galvanometer frame.

2493. In what manner was the marine galvanometer applied to the testing of the cable?—It was kept with its coil constantly in circuit with the cable. The marine galvanometer at the sending end was kept between the battery and the cable; when receiving, the marine galvanometer was kept in circuit between the cable and the earth. In receiving, one of the detectors was also kept in circuit, so that the current came through both its coil and that of the marine galvanometer, in series between the cable and the earth; but in sending, the detectors were thrown out of circuit, in consequence of the tendency their needles had to become demagnetized or reversed by the sudden shocks experienced on applying the battery to the cable. Thus, the only test applied to the cable on board the ship which was sending, during any period, was the marine galvanometer; while in receiving, there was both the marine galvanometer and the detector, each available as a test, so that the one might be used to check the other, if necessary.

2494. With what description of battery and of what power of battery was this instrument made use of?—The common sand, zinc, and copper battery, with saw-dust substituted for sand; was the battery which was used; saw-dust having been introduced in place of sand in 1857 by the Electrical Department at Valentia, in consequence of the sand there not being available, from the quantity of calcareous matter which it contained.

2495. (*Mr. Varley.*) Was not the sawdust impregnated with weak diluted sulphuric acid?—Yes.

2496. In testing the cable after the current that had rushed into the cable was observed, did you leave the cable disconnected for a while after the battery contact had ceased, before connecting it with the marine galvanometer, or did you take the discharge immediately from the cable?—The discharge always went through the marine galvanometer.

2497. Was it taken immediately after the battery contact had ceased, or after a given time?—After a given time. The mode of observation with the marine galvanometer was:—One operator applied the battery, in the two directions alternately, with an interval of six seconds, during which he put his end of the cable to earth. He counted time by the second hand of a watch which he held in his hand, and exactly three seconds after he had touched the key, to throw the end of the cable from battery to earth, he called out "now;" the observer noted the exact position at that instant of the moving spot of light reflected from the marine galvanometer, and recorded the indication. Ten seconds before applying the battery key the operator always called out "look out" as a warning to the observer.

2498. Then the discharge was noticed immediately after the battery contact had ceased?—Yes. The principle of the operation was as follows:—The needle of the marine galvanometer was thrown violently against the stops which limited its range by the first rush into or out of the cable. As soon as the current came to be not so strong as to keep the needle pressed against the stops, the needle gradually returned either to zero in case of the end of the conductor having been put to earth, or to its permanent indication in case of a battery having been applied and kept applied to one end of the galvanometer, of which the other was always kept connected with the cable. If the battery power used was not strong enough to throw the needle against its stops, the needle was set into violent vibration, and an observation became impossible. But the ordinary battery power used; sufficed, to give an indication in the manner which has been described. During the greater part of the process of laying the cable, 20 twelves of the battery described, before was the power used, I was not all satisfied with this battery, it varied so continually in intensity, and also in resistance. The strength of the current arising from it was very much increased on every occasion when the battery was refreshed by having a little more sulphuric acid and water poured into its cells. The battery which I used in all my own experiments on the cable, was a Daniel's battery, and I had a battery of about 10 elements of this class, constructed on a plan devised by myself for marine purposes, constantly available for special tests.

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2499. (*Chairman.*) Did you use a galvanometer of another description in circuit at the same time as the marine galvanometer, so that each would form a check upon the readings of the other?—During the receiving in either ship there was a "detector" in circuit along with the marine galvanometer; during the sending the marine galvanometer was used alone. With reference to the system of signals adopted, I may add, that on the return after the failure to lay the cable, I had a system of signalling, by which the quantity of cable paid out from each ship was made known every ten miles. This was the only addition to the system of testing which I have previously described.

2500. There was a peculiar arrangement for sending?—Yes. With the knowledge which I now have I should not hesitate with even such instruments and arrangements, imperfect and incomplete as they were, to adopt a regular system of speaking by the Morse alphabet, which can be read with great ease, although not recorded by the marine galvanometer, during the whole time of laying the cable, but subject to certain very precise conditions to prevent the possibility of interfering with testing, and further I should consider it absolutely necessary in any reasonably arranged system of testing, that from each end a signal should frequently be given to the other end to indicate the strength of the received current, because it is of the greatest importance in judging as to any defect, to be able to have the results of the testing from each end. Therefore as long as the cable is in such a condition that it is possible to speak through it at all, I think it an essential part of thoroughly good testing in the laying of a submarine cable, that in a certain perfectly arranged system of signals there should be one short period of time allotted to the interchange of information as to the results of testing.

2501. In other respects do you think the system of testing was satisfactory?—The system of testing was perfectly satisfactory, so far as letting us feel quite sure of the transmission of a current through the cable during the whole time, except during certain periods when the current failed, and then the system of testing enabled us to detect, as far as the instruments on board admitted, the nature and quality of the defect; but I consider that there was a very great omission in the apparatus on board in the want of standard resistance coils. I had urged on the electrician of the company, as early as the month of May 1857, the very high importance of having a set of resistance coils properly made, giving a resistance at least equal to the resistance of the whole cable, and admitting of variations to the smallest measurable quantity. I urged this strongly, but the electrician of the company had his own system of testing which he considered satisfactory. The great want in our system of testing was a good constant battery and a set of resistance coils. The constant battery I supplied, as far as I could, from the resources which I, for another reason, had provided. A sufficient set of resistance coils could not at the time be extemporized, and, accordingly, much of the testing was necessarily mere guess work. I supplied the deficiency so far as I could at the time, by making up into properly insulated coils of measured length, a quantity of covered brass wire, which I had with me as material for some of my telegraphic instruments. This I did on the outward

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voyage of the successful trip, and I put two of these resistance coils on board the "Niagara" at the rendezvous, which, with a number retained for my own use on board the "Agamemnon," constituted the only resistance coils available for any of the testing operations either at sea or on shore for the first fortnight after the ends were landed.

2502. Then you would adopt a similar system of testing, in any future case, with the addition that you have been describing?—Exactly so. I may mention that I drew up a proposed plan of testing, with reference to the laying of the Red Sea cable, and submitted it to the contractors, Messrs. Newall and Gordon through my friend Professor Gordon.

2503. Is that the system which has been adopted?—I am not aware; Mr. Siemens took the testing in his own hands. I am not aware to what extent the suggestion may have been adopted during the operations hitherto performed. In fact the suggestions were only sent in writing last May, when probably it was too late to carry them into practice in any of the operations that had to be executed.

2504. (*Mr. Saward.*) Would not the object of resistance coils be to establish a standard of resistance for the whole length of the cable?—Exactly so; and also a standard for measuring the degree of insulation for the whole or for any part of the cable.

2505. (*Mr. Varley.*) In the event of any fault occurring to aid in the discovery of the probable locality of that fault by comparison with that standard?—Yes.

2506. (*Chairman.*) Were these signals transmitted from the commencement to the end of the paying out operations through the entire length of cable on both ships?—Yes, with the exception of a comparatively small part of the cable on board the "Niagara." After a certain number of days, according to the arrangement, the whole was thrown into circuit. We made the signals through the whole length with that exception, during the whole time, except such intervals as were spent in special testing, in consequence of the sudden appearance of indications of defects.

2507. (*Mr. Saward.*) Are you aware whether the signals were also made through the whole of the cable on board the "Niagara"?—Yes, with the exceptions I have mentioned.

2508. (*Chairman.*) From your experience in the continuous passing of currents through the cable, and other sources of observation while on board the "Agamemnon," did any misgiving arise towards the completion of the work, that the cable was, to some extent, in a faulty condition, and must have been so before its submergence?—I had the very strongest misgivings as to the condition of the cable from the Monday forenoon of the laying. I may mention, in the first place, that we had a loss of continuity on the night of the commencement, accompanied with certain very singular circumstances; the whole circumstances I have described in a short paper which I wrote on board the "Agamemnon," at the request of Captain Preedy (Appendix No. II.), and which has recently been published in the "Civil Engineers' and Architects' Journal," in a report of a discussion which took place at the recent meeting of the British Association.

2509. What was the general result of that statement?—The signals suddenly ceased altogether at a certain instant, the testing shows improved, rather than deficient insulation, at the very time the operations were going on in the hold with a view to protecting the portion of the outside sheath (which had got damaged) from the risk of fracture in going out. It seemed most natural to suppose that the breach of continuity in the interior conductor, which these tests demonstrated, was in the damaged portion of cable actually in hands in the hold, since this portion had been bent about so much as to displace the outer strands of iron wire, in consequence of the rough weather of the previous trip. Accordingly the cable was pricked into very close to the outgoing part, with a view to testing whether the breach of continuity was on board the ship or not on board the ship. The result was, that there was almost perfect insulation exhibited when the battery was applied to the conductor, at the part where it was pricked into. It was perfectly certain then that aportion of the wire at fault was broken inside the gutta-percha, between the part pricked into and the "Niagara's" end of the cable; and from the very great perfection of insulation which I found, it was quite certain that the distance of the fractured point could not be great. It was in the paid-out part, or else on board the "Niagara," and I endeavoured to ascertain whether it was on board the "Niagara," or in the paid-out part; but the imperfection of the apparatus which I had at command, prevented me from making more than a guess. I made a rough estimate, by comparing the result of an experiment I had made the day before, with a view to the possibility of such a contingency, by means of one of the common detectors. I found that the discharge after charging by the battery, and then discharging through the galvanometer, was such as was consistent with the supposition that something more than 50 miles, and less than 150 miles, if I remember rightly, might be the locality of the fracture.

2510. How much of the cable had been paid out at that time?—80 miles.

2511. Then the fault might have been on board the "Niagara"?—I could not make certain whether the fault was on board the "Niagara," or in the sea between. When it had been ascertained that the defect was not on board the "Agamemnon," we had nothing to do but to mend the place where the cable had been pricked as quickly as possible, finish the mechanical operation of joining, and get it overboard. It had been necessary to cut the cable in the course of this operation for the electrical test, and the joint was made and put right with great energy and ability by the engineers, and the cable was paid out freely, with the mended part made quite secure. It seemed now as if we had nothing to do but to wait for a certain time, and then cut the cable, because no indication whatever of signals was perceived. There was very excellent insulation, but no vestige of current through. After about an hour and a half from the moment of failure, during a period when we were sending, the current from our battery suddenly became much stronger instead of much weaker than that observed when the whole cable was in circuit. Three minutes later came our regular period for receiving; we put our end to earth as usual; and signals came from the "Niagara." I could only account for what we had observed by two possible hypotheses at the time; one of them was, that the wire had broken under water, and the two parts had come together again by the elasticity of the sheath, when the cable was relieved from stress on or near the bottom; and the other hypothesis was that there had been a breach of continuity on board the "Niagara." According to the second hypothesis, the cause of the great sudden rush of current from our battery must have been owing to their cutting the cable on board the "Niagara," or else to a fracture of the cable by accident, at the stern of the "Niagara;" but when the signals came it could no longer be apprehended that the cable had been broken at the stern of the "Niagara." It might still have been that they had cut on board the "Niagara," and put the outgoing part of the conductor to earth during the period of three minutes, before we had their signals, and that they continued to work with us through this part of the cable, while they looked for and cut out the breach of continuity in the remaining portion of the cable on board their own ship; but this appeared to me less probable than the other hypothesis, because, judging from the order of events, I thought it the more probable that the breach of continuity had been overboard. The event proved so, because in the "Niagara" precisely similar difficulties occurred as to failure of signals. The tests gave similar results, and a similar uncertainty prevailed

until the continuity was re-established. The strong current which I found was therefore proved to be as I anticipated, owing to the fact that on board the "Niagara" they had cut and tested, and were testing at the time when the wire came together again. Thus it was on short circuit, so far as the "Niagara" was concerned, that we had the very strong current from our battery when the wire came together again. About an hour later, instead of our strong outgoing current, we had again the current of previous strength, which convinced me that after having cut to test, and after the fault had come right of itself, those on board the "Niagara" had joined in the whole cable, and when they had joined in the whole cable again, our current became, as before, of moderate strength.

2512. (*Mr. Saward.*) During what period were you without signals at all?—About an hour and a half.

2513. (*Mr. Varley.*) Was not the cessation from 10 to 11.30 P.M. ?—From Greenwich time.

2514. (*Chairman.*) Did you meet with any other indication on the voyage towards the coast of Ireland of want of insulation?—About midnight, local time, on the Sunday night I had retired to rest for a short time, but I was informed almost immediately that there was something wrong. I went instantly, and found the signals coming, but excessively weak. I looked at the diary for a few minutes before and found that in our outward current there had been an increase of strength, shewing that there was some great fault of insulation. The signals became very weak, then they altogether failed for a time, then they came again very weak, failed altogether several times, and on the Monday forenoon about 11 or 12 o'clock, I could get signals only by observing very slight flickers of the spot of light reflected from the marine galvanometer; I could only do so by throwing it out of circuit and throwing it in again several times during each roll of the ship. The rolling of the ship at that time was such as to make the spot of light move over several divisions of the scale, as the mirror was not so well balanced as I have them made now. The consequence was that the signal current, which gave only a fraction of a division of deflection could only be detected by throwing the instrument out of circuit and in again every moment. I ascertained by close inspection that the spot of light moved but a small fraction of its own breadth, each time the circuit was opened and closed, and by judging in this way I read some of the signals indicating the number of miles paid out. In this way I could assure myself that the continuity still existed at this time. On Monday afternoon the signals received became much better, and the other tests indicated an improved but still very defective condition of the cable. In the last part of the voyage, for a day or so perhaps, the insulation became somewhat improved, which I attributed to the outgoing part becoming cooler, but it was still very bad; very much worse than it had been previous to Sunday night. When we landed the indications of insulation were extremely bad, and showed a very faulty condition. Until the end of the voyage we had regular indications of the numbers of miles paid out from the ship, and correct replies to our signals of the same kind. Several short messages were correctly read on the marine galvanometer, on board the "Niagara." I sent those messages according to a preconcerted alphabet, it having been arranged that on approaching the shore, the strict rule that there were no words to be used might be deviated from. During the time the "Agamemnon" lay in harbour in Valentia, before the end was taken out we received signals from Newfoundland indicating the length paid out, and also that they were about to land.

2515. At what distance from the shore was it that the second defect of insulation was perceived?—Only the second fault was defective insulation, the other was defective continuity; the defective insulation appeared first when we were about 420 nautical miles from shore. The whole length of cable laid after that time from the "Agamemnon" was 490 nautical miles.

*2516. To what causes do you attribute those faults?—I have very little doubt but that the first sudden appearance of defective insulation occurred from a bad fault going overboard; by a fault, I mean either a crack or an abrasion of the gutta-percha allowing the copper wire to become wet as soon as it entered the water. There was much confusion in testing, produced of necessity by the attempts made by those on board the "Niagara" to discover the state of the cable. I have been since informed, that they cut the cable and put their end to the earth for some time, during which they did not send a signal at all, and that certain operations of joining and cutting again were gone through at intervals during a great many hours, occasioning the total cessations of signals which we experienced. At the times the signals actually came, the indications were as small as I have mentioned. The ordinary signal current would be about fifteen divisions of the scale, and at certain times a small fraction of a division was all the indication. Even during those times I read messages, and after that there came a considerable improvement. The signals came indeed again as strong as ever, but it appeared to me very probable, that the great increase of strength, which occurred after the very bad times seemed to have passed away, might have been because higher battery power was applied, and events prove that it was so. We got the signal current about as strong as before, but the battery power on board the "Niagara" had been considerably increased.

2517. (*Mr. Saward.*) When the bad fault which you spoke of just now as a crack appeared, what distance from Valentia do you believe the cable had been paid out?—I cannot account for what I perceived during the voyage, except on the supposition that a fault went over board at the time when the bad symptoms commenced. At that time 520 nautical miles had been paid out from the "Agamemnon," and 490 more were paid out before reaching Valentia.

2518. Was not the fault at that point a want of continuity?—The first appearance of a failure of insulation was when we were about that distance from shore.

2519. (*Chairman.*) Do you consider that these faults were due to a defective construction of the cable, or improper use of it before it was laid, or to the fact of its being laid in deep water?—Certainly not to the fact of its being laid in deep water; most probably to injury in the manufacture, either from mechanical injury, or from overheating.

2520. Or from the repeated coilings and uncoilings?—I have no idea that repeated coilings and uncoilings can do any harm whatever to a piece of sound cable; but repeated coilings and uncoilings may finish the development of a fault if there has been a portion of the gutta-percha defective previously; for instance, if the wire is very nearly through the gutta-percha, or very eccentric in any part, the coiling and uncoiling may make it go through, or if there has been a ragged wire from bad joints, the coiling and uncoiling might make it protrude; but no amount of coiling or uncoiling, and no amount of mechanical strain in the way of kinks that a cable ever experiences in being paid out or afterwards in laying itself on the bottom as it does very much in kinks I believe, can possibly do any harm whatever, in my opinion, to a piece of sound gutta-percha core.

2521. (*Mr. Varley.*) Was the cable submitted do your think to any temperature which might be dangerous previously to the laying?—On the experimental trip there was an alarm one evening that some warm water from some part of the engine had got into the hold in the neighbourhood of the cable, and it was feared that it might reach the cable and do damage. I necessarily made close tests, first from one end of the cable and then the other, and I could not convince myself that I ascertained for certain any variation whatever at the time. The cause of that accident was quickly done away with, and I feel perfectly convinced that the cable suffered no damage what-

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ever on the occasion. I continually tested both ends from that time forward of the coil on board the "Agamemnon," and never found any indication whatever of the cable having suffered damage. On the contrary, if I remember rightly, the lower end generally insulated a little better than the upper end in all the tests that I made on the homeward voyage of the first attempt, and on the outward voyage of the successful attempt. I cannot speak for the damage that the cable may have possibly experienced in being coiled out of the store at Keyham into the ships, but I do not think it at all probable that it experienced any: the only way in which it could have experienced any damage on those occasions was by possible exposure to the sun; but there was no sun of sufficient intensity to allow damage to occur, with such precautions as were taken: a large part of the cable was injured by exposure to the sun in one of the manufacturing yards a year before, but I believe there was no damage of that kind experienced afterwards.

2522. (*Mr. Saward.*) Do you consider that sufficient skill and attention were used in testing the cable during the time of its manufacture, and if not, whether that may not account for the faults being allowed to remain in the cable, although they were such faults as might have been discoverable by a better system of testing?—I believe so; at the same time, I do not wish to say anything against the system of testing that was applied, because I am aware that there was not previously to that time much good testing of any cable; but certainly, at one period of the testing, there was a very faulty system followed, from a want of skill in the use of the instruments. The deflection of the galvanometer was so great, that it really gave no satisfactory indication as to even considerable changes of insulation. I was consulted at the time by the electrician of the company, and pointed out how this difficulty, rendering his tests nugatory, should be remedied.

2523. (*Chairman.*) What system of testing should you have recommended?—I should have pursued a totally different system of testing in many respects. As to the make of the particular instrument, it was not at all a well devised instrument, but as good as the instruments generally used at the time. To make use of that instrument in that case, a particular method for bringing the indications of the needle within range for accurate determinations was required.

2524. What system of testing would you prefer for a submarine line?—Testing entirely by comparison with absolute standards of resistance. I am not aware that this system of testing by absolute standards of resistance was ever brought into practice by any practical electrician, except Mr. Varley, at that time, but the principles were shown long before by Professor Wheatstone. For testing, I would never use any other than a Daniell's battery, and a proper galvanometer of almost any kind, even any common galvanometer, with the power used will give satisfactory results, provided absolute standards of resistance variable to any amount, such as the coils that Mr. Varley has had constructed for telegraph purposes, are used. On the other hand, even without the resistance coils, the tangent galvanometer, such as German and British experimenters have used very much, and as some German practical electricians have used, give satisfactory results, and so do the reflecting instruments that I have used. The reflecting instrument may be adopted to give definite results where resistance coils are not available. I would never think any testing apparatus at all satisfactory or complete, without a very well arranged set of coils for standards of resistance.

2525. (*Mr. Varley.*) Do you not think the system very much pursued of using vertical needles, in which gravity is used to bring the needle to an upright position, is very objectionable, inasmuch as any variation of magnetism will cause a variation of the result. When the needle is placed in a horizontal position and brought to zero by the use of magnetism, every variation is the result entirely of a change in the electro-magnetic force of the current to be measured?—Perfectly so; the vertical galvanometers are extremely uncertain and variable in their indication on this account, that the signals may be made to appear stronger by the magnetising of the needle. The horizontal needle galvanometer is not open to that defect, consequently a horizontal needle galvanometer was adopted by the late electrician to the company in his tests on shore; in that respect the galvanometer which he used was an improvement on the vertical instrument or detector, but the tangent galvanometer would have been a very great improvement upon that instrument; and whether a tangent galvanometer or that instrument was used, it would have been absolutely necessary to take a system of relieving (such as I pointed out to the electrician of the company) the coil from too strong a current, in order that its indications may give real tangible results when a very strong current is to be measured

2526. (*Professor Wheatstone.*) What do you think of the method of oscillation employed by Fechner in verification of Ohm's law?—Not so satisfactory as the measurement by deflection; in the hands of a careful experimenter no doubt it is susceptible of high accuracy.

2527. (*Chairman.*) On the landing of the cable at Valentia, you at first gave your aid to Mr. Whitehouse, the company's electrician, and subsequently, at the pressing request of the board, you consented to take the responsibility of conducting the electrical department for a limited time?—Yes.

2528. There was a delay of several days, was there not, after the landing of the cable before readable signals were exchanged between Newfoundland and Ireland?—Yes.

2529. Will you be good enough to state to what cause you think this delay was to be attributed?—I have no doubt it was owing to defective insulation becoming worse and worse; from the landing of the cable on the forenoon of Thursday, the 5th August, till Monday night, near midnight, we never received anything that we could be quite sure was a signal current. On a few occasions during that period something in the indication of the instruments looked like reversals. I repeatedly pointed out to Mr. Whitehouse that either there must be something very much wrong in the cable indeed, or that they must have got into difficulties at the remote end, and that the only hopeful explanation as regarded the condition of the cable and the want of signals received from them, was, that they may have been forced to land in some locality in which they could not bring up their batteries. It has been since ascertained by a direct communication from the parties at Newfoundland, that they were, during the greater part of that time, sending us regularly signals on the ship system; and the fact that we did not receive those signals, shews that the cable must have been in an excessively defective condition during those days. I made several tests, especially on Monday the 9th, and found the whole resistance opposed to a current from the battery end of the cable, not more than equal to from 500 to 600 miles of the cable, so far as I could judge, and very much varying. When the negative pole of the battery was applied, the insulation became worse, that is, the resistance to the current leaving the battery, became less and less. When the positive pole was applied, the outward current fell in strength very much, shewing that the insulation was improved. This was so very defective a condition of insulation, that I thought it scarcely possible that anything could be done with the cable unless the fault could be reached and mended; but on the Monday night we were agreeably surprised by the appearance of very distinct reversals, seen not only on the mirror galvanometer but on the common detector. It became obvious from the rate at which those were sent, that the sending apparatus and the induction coils had been applied at the remote end of the cable; and almost immediately after, there came signals, which we

readily read as words. The first words read were, "Please repeat slower," these were read on a little single needle instrument of Mr. Henley's, and simultaneously on my mirror galvanometer.

2530. Which was a much more delicate instrument?—Yes, a much more delicate instrument, not confined by stops, and giving freedom to the needle to show the signals through a wide range instead of simply between two stops.

2531. Then you attribute the failure of the current immediately after landing to the defects of insulation, which were overcome by the use of more powerful batteries, or more improved means of transmitting the signals, either by induction coils or batteries afterwards?—Yes, and still more by improved means of receiving.

2532. (*Mr. Varley.*) Do you not think that the fault underwent some considerable change; do you not think it was possible, supposing the wire was projecting through the gutta percha, making a very considerable amount of earth, as positive currents were applied they gradually dissolved that wire away, and so rendered the fault capable of offering more resistance in the neighbourhood?—I do not think it at all probable that all the resistance was in the wire. I think the fault certainly varied very much, owing to some such cause as Mr. Varley has just mentioned. The general condition of the cable must have been much worse on the Monday forenoon than it was on the Thursday morning before we landed, because the common detector showed freely their landing signal on the Thursday forenoon, and neither the detector nor my mirror instrument as then adjusted showed any of their battery signals during the four days from the 5th of August to the Monday night, with the exception of the short inversals to which I refer, when doubtful indications or reversals were observed.

2533. (*Chairman.*) After the cable was at the bottom, you think it improved in insulation?—It improved during some days of the voyage.

2534. For how long after you received the first readable signals did you continue to receive signals?—From the midnight of the 10th of August to the 1st of September.

2535. Were the signals equally strong during the whole time, or were they gradually failing?—Gradually failing, but with variations; failing altogether for a time, getting weaker and weaker, and then suddenly showing very satisfactory indications for a time, and then failing again, but on the whole gradually becoming weaker.

2536. Can you give a general opinion as to the causes?—I have no doubt the causes must be looked for in such variations of the fault as Mr. Varley has spoken of. Probably it may be explained, in a general way, by oxidation of the metal taking place at certain times, and by a certain white deposit which I have observed taking place from sea water when a negative current is applied. I believe that the oxidation produced by the positive current is, to a certain extent, an insulating protection, while the white deposit which occurs when the negative current is applied, does not, in general at least, appear to contribute in any way to insulation.

2537. Is that white deposit from the copper?—The white deposit is from the sea water. On the other hand, the cleaning of the metallic surface which the negative current produces probably, for a time, makes the insulation worse. At the same time I consider, if a cable is to be preserved with a bad flaw, it is to be done by the use of negative currents only, of limited strengths, and by using instruments of higher sensibility.

2538. After the fault has been oxidized by the positive currents?—It must be cleaned by negative currents and taken at its worst, if the cable is to be preserved at all. If you keep on the positive current the infallible result is that the wire is eaten through. I have tried it in many experiments with very mild currents, and I find that the current eats through infallibly in days, weeks, or months. I have had wires under the influence of mild positive and mild negative battery applications in plain water, and various compositions of salt water and sea water, and I have found invariably, after a few months, the wires eaten through, or perhaps, through except one of a strand, and that half eaten through, in the cases of positive battery application. In the cases of negative battery application the wire remained perfectly uninjured; in neither did the gutta-percha seem to vary very much, and there did not seem to be ultimately any worse insulation in connexion with the fault that had been kept negatively electrified, than those that had been positively electrified; so that it appears to me that the improvement of insulation by positive currents has only a temporary effect. I make this statement, however, with little confidence, because it would require a great deal more experimenting upon than I have been able to give to the subject to form any opinion upon sufficient grounds as to the ultimate effect on the gutta-percha of a continued application of negative currents, as compared with the ultimate effect of the continued application of positive currents especially with regard to insulation; there can be no doubt of the certainty of the wire being eaten away after greater or less time, as the case may be, if the positive currents are applied.

2539. When it was first discovered that signals were failing, I believe it was thought that the fault was in that part of the cable laid in the Harbour of Valentia or near it?—It was never thought so by myself. I thought it was impossible that any failure in the Harbour of Valentia could diminish the signals from Newfoundland to a considerable or even to an appreciable degree, on the instruments by which they were received (which had unusually small resistance in their coils), without also giving rise to quite a different condition of testing from what we have found.

2540. The cable was, however, under-run to the mouth of the harbour, I believe, with a view to the discovery of the fault supposed to have existed there?—In opposition to the orders of the Board of Directors, and against my strongly expressed advice, and during my temporary absence.

2541. At what distance in your judgment did the fault exist, or does it now exist?—About 300 miles along the cable from the shore end.

2542. Is that in deep water?—My only doubt has been whether it might not be just on the confines, possibly in shallow water, but more probably in deep water. I say about 300 miles, my first estimate was about 300 miles, which I made at the time the cable began to fail, and communicated to the board. When Mr. Varley came a few days after with his graduated resistance coils, I was able to make much more accurate experiments, my previous experiments having been to a considerable extent guess-work rather than accurate measurement. I had strongly urged before upon the directors, that whatever the distance might be, it was something certainly exceeding 200 miles, and therefore no fault need be looked for in the harbour, and further, that there was no use in going to the expense in laying thick shore ends till we found whether or not this fault could be reached and mended. When the cable completely failed at the beginning of September such was the state of opinion upon the subject; 300 miles was the estimate I then made, and Mr. Varley confirmed that estimate very decidedly by his resistance coils; but at the same time there was an uncertainty affecting all our results, arising from the want of knowledge of the conductivity of the cable; the most accurate estimate I was able to make with the decided information subsequently as to the length of the cable in some of the coils I have experimented upon was, that 270 statute miles was the most probable distance. I assigned as the probable limits, that they could not be less than 245, and could not be more than 280 miles, and this estimate was communicated in a letter written to the secretary of the company in the month of October following.

2543. (*Mr. Varley.*) Mr. Henley has stated, and published, that it is impossible to ascertain the resist-

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ance of a fault in any such cable; and in his opinion, although the fault cannot be more than 300 miles distant, that it may be much nearer, say 100 miles. Do you think the means adopted at Valentia were sufficient to indicate the approximate distance of the fault itself?—Quite sufficient; it is impossible, from the mere testings alone, to suppose that the fault can be at a distance so small as 100 miles; the testing alone is sufficient to make it certain that that cannot be the case. My first reason for believing the fault to be at so great a distance as two or three hundred miles (at least before I knew how to discover the resistance of a fault,) my first means of judging of the distance was simply this, that inasmuch as the messages were received on an instrument which presented a resistance only equal to from five to ten miles of the cable, any fault in the harbour which presented a resistance equal to two, or three, or four, or five hundred miles of cable, would only make a very small deduction from the current that would pass through that instrument. As the deduction from the current actually passing through the instrument, as compared with the current sent out from Newfoundland, must have been immense to make the signals as weak as they were. I contended that the fault must be really as far from Valentia as a length equal to the total measured resistance. That was my conclusion, which virtually was founded upon a combination of tests at both ends, that is to say, founded upon a combination of tests as to the amount of sent current from Valentia, and the amount of received current from the batteries, the strength of which I could form some estimate of, at Newfoundland. In subsequently experimenting upon the cable, I found certainly variations in the resistance by which I was led to infer, independently, the distance of the fault; but I found Mr. Varley had been in the habit of prosecuting this manner of testing completely. I had been driven to it, and I found the only way of interpreting what I saw, was to suppose that the fault must be at some such distance as to present variations of a few miles, 10, 15, or 20 miles, in itself. During Mr. Varley's stay at Valentia, I found that this which I was just beginning to see, or formed some distant idea of as a means of testing was perfectly familiar to him, and one of his practical methods of working both during his stay at Valentia, and previously. I made a great number of experiments on faults in water, which convinced me that that was a real practical way of forming an estimate as to the minimum resistance of a fault when the copper is cleaned as much as possible by negative currents.

2544. (*Professor Wheatstone.*) How do you distinguish the existence of a single fault from a number of small faults at unequal distances?—It is not always possible to distinguish perfectly, unless we have recourse to a combination of a considerable variety of effects. But of this we may be sure, that the nearest fault must be a very bad one, and that there is tolerably good insulation up to it if certain phenomena regarding the variation of faults under the action of positive and negative currents are observed. When those phenomena are observed, all that we can say is, that the cable is perfect up to a given estimated distance, and there is a bad fault there; we cannot say how many bad faults there may be beyond.

2545. You are not certain that the defect consists only of a single fault?—The defect which chiefly stopped our operations consisted of a single fault, or I should say there is a sufficient fault to almost, if not entirely, stop our operations at the distance stated, but it is far from improbable that the total failure was contributed to also, as Mr. Varley in his report has conjectured, by a more distant fault.

2546. (*Mr. Saward.*) Are you aware what percentage of slack was paid out during the last 500 miles; I have been told that it was a larger proportion than during the previous part of the voyage?—During the last 500 miles there was a very small per-centage of slack paid out.

2547. (*Chairman.*) By what description or descriptions of apparatus were the messages sent through the cable received and recorded?—The first words were received, not recorded, on the two instruments which I have mentioned; immediately thereafter one of the relays constructed by the late electrician for the company was put into circuit, and some of the signals were recorded by it; but before I left, so far as I am aware, nothing was read on the relay. I left Valentia within a few hours. However after the first words were received, and before the adjustment of the relay was completed, during my absence, which lasted for eleven days, there were, as I have been informed, some indications of legible signals recorded by the relay, but not, so far as I can learn, any one complete sentence. I have had all the slips, which were preserved by the Board, to examine, but these contain very little of the work of the first four days, the slips corresponding to those days having been all abstracted from the possession of the company. I do not find them among the bundle which was put into my hands. Of the few that remained there were two or three which showed the action of the relay, and among those I do not make out any one single word completely and correct, and no one sentence that could be even read at all from the indication of the relay. I find altogether two or three words and a few more letters that were legible, but the longest word which I find correctly given is the word "be."

2548. At what maximum speed were actual messages transmitted?—The maximum speed seems to have been two and a half words a minute; I believe that speed was rarely attained. I have a number of slips which I might lay before the committee to be used for illustration or any other purpose, which contain distinct evidence as to the speed attained. I confess myself surprised by the great speed that those slips demonstrate, as I find decisive demonstration that in the course of the messages which came perfectly distinct on the Morse system, there were dots at the rate of rather more than forty per minute or eighty reversals of the current per minute. The messages were all received on the mirror instrument, and it could not possibly, even with the cable in perfect condition, have marked the dots and dashes distinctly on the relay, and could not possibly have given legible letters on the relay, because, from inductory embarrassment, dots would have been converted into dashes, in some places altogether omitted, and in others marked in such a manner as to make complete confusion out of the indications from the best possible relay. The mirror instrument, not being confined by stops, can mark signals on either side of its scale, without ever returning to zero, or after having made a number of signals on one side, may gradually come through zero to the other side, under the influence of a varying earth current, or of the great ebbs and flows owing to inductive charge, and through all these charges will show every dot and dash with perfect distinctness: whereas an instrument with stops would, when thrown to one side, make a long score, a large dark mark or line, and on the other side a long blank. The maximum rate of signalling which I have myself verified in actual messages is two words a minute. Specimens of the slips will be laid before the Committee.

2549. Did you notice whether any material difference existed between the rate of speed of signals sent by means of the induction coils and those for which batteries were used?—I had no opportunity of comparison, because all the signals came to Valentia from the induction coils at the other end. Until the month of October the whole of the Valentia signals were sent by batteries after the first three or four days; after the first three or four days Mr. Whitehouse himself gave up the use of his induction coils because he found that by trying them alternately with the battery their signals were not read, while the battery signals were read. My own opinion, however, is, that if the receiving instruments at Newfoundland had been put on the same footing as they were immediately at

Valentia, there would not have been the same difficulty in reading at Newfoundland, and probably the coils might have been continued to be used at each end. The signals, however, were received at the other end, (after attempting to receive them on the relay,) for some days chiefly on the detector, and necessarily at an extremely slow rate. It was not until the 22nd of August that the mirror instruments were regularly put into requisition at the other end, and they were then put into requisition in consequence of a message that I had transmitted giving an order to that effect. They had been several times put into circuit previously for a certain time, and then with ease readable indications were obtained. They were again thrown out of circuit, the system appearing to be that as soon as something was read distinctly with the mirror instrument at Newfoundland it was thrown out of circuit, and attempts were made to read by the detector or relay, the director of the staff not considering himself justified in introducing a new patent instrument without positive orders, and he therefore very properly, as soon as the mirror instrument gave indications that something really was coming, attempted to read it on his instruments; those attempts were sometimes successful and sometimes involved great delay; the great delays in the transmission of the Queen's message were accounted for in that way over and above the stoppage during the nugatory operations in the Harbour of Valentia.

2550. How long did the Queen's message take in transmitting?—It was stopped in the middle, and a great many parts were repeated over and over again, therefore, I cannot say what was the time; but it was 16 hours from the time it was begun till the time it was finished, and perhaps even more than that.

2551. Is it your opinion that the electrical state of the cable was made worse by the use of induction coil power or battery power, or either of them?—Most probably it became worse more rapidly than it would have done if the fault had been altogether undisturbed.

2552. By the use of induction coils?—Certainly, either one or the other. My opinion is that the use of induction coils is very dangerous in a case in which the gutta-percha is very nearly pricked through, but I do not think that induction coils could do very much damage, or be worse at all events than battery power at a great distance. I do not think that the induction coils would produce a worse deteriorating effect on a fault already formed than battery power at 300 miles distance, for instance.

2553. What would you use to send messages through cables, if both battery power and induction coils are injurious?—I would have the receiving instruments made as sensitive as possible, and I would use the lowest battery power that could be got to move them if any doubt existed as to the condition of the cable.

2554. (*Mr. Saward.*) Would you use magnetic electricity?—Nothing but direct battery. As far as I can see at present, the use of direct battery can be made much safer and milder in its effects upon the cable than any system of induction coils. At the same time the magnetic system I believe has to a certain extent the safeness of the battery in being comparatively mild, milder than the action of induction coils depending upon a sudden break of the primary circuit. The use of a magneto machine somewhat like Mr. Henley's, I do not consider could be dangerous to the cable. I prefer the battery, because I think a battery can be managed both for rapidity of signalling and the security of the cable, so as in all respects to be preferable to either the magneto machine, or the induction coils.

2555. (*Mr. Saward.*) It would be slower in its operations, you could not get signals through it as rapidly as with the coil power, could you?—More rapidly when rightly directed, but when used simply with the Morse key, it would depend upon the adjustment of the instruments with which it was used, and the system of contacts used.

2556. (*Chairman.*) What is the minimum amount of battery power that you think would move your marine galvanometer, giving a readable signal through the Atlantic cable if it were perfect?—One cell would give, through a well insulated cable of the same length and gauge, a current strong enough to produce about six scale divisions of deflection in the improved galvanometers I have at present; and would therefore be sufficient to give even tolerably rapid signalling at sea. My land mirror galvanometer can be made one hundred times as sensitive without becoming too slow for signalling at a rate of two or three words a minute. With the Atlantic cable, if I had a power of from 20 to 50 cells, I could secure very rapid signalling by a proper arrangement of battery contacts.

2557. (*Mr. Varley.*) Should you not be afraid, in using such low power, of being embarrassed by earth currents through so long a cable?—They do not embarrass with the mirror instrument, unless they are very bad. The ordinary amount of earth current will not exceed that which from six to ten cells will produce, and they will generally be less than that. Now, even when earth currents are six, or ten, or twenty times the signal current, there is no difficulty whatever in reading the signals with a mirror instrument. A recording instrument of my invention, on the same principle as the mirror instrument, so far as being free from stops is concerned, would do all that the mirror instrument can do, but requires a higher battery power. I have constructed a recording instrument upon this principle, and have found it certainly available to record messages, even when disturbed by earth currents, having in that respect the same quality as the mirror instrument; but I have not succeeded in producing any recording instrument that will work with so low a power as the mirror instrument. Therefore I prefer a non-recording instrument, not only for economy, but for the safety of the cable, in order to use as low a power as can be applied to get the speed.

2558. Mr. Whitehouse stated, that he found a difference of speed in the transmission of signals of one to four between the use of induction coils and the use of battery power. Did you find anything in your experiments to lead you to suppose that so great a difference existed?—No, not at all; no such difference. At the same time I think Mr. Whitehouse's results were the results of real experiments, and simply originated from the circumstance that the induction coil which he used gave an immensely high intensity, and the greatest number of cells that he compared it with, I believe, was about fifty or sixty. Now, a much greater number of cells would be required to give anything like the same amount of effect in rapid signalling. The difference of the induction coils and the battery as regards rapidity is this, that if you increase the rapidity of the signalling by the induction coils, each impulse still throws in the same quantity to the cable, while, if you increase the rapidity of action with the battery when you double the speed, you send only half the quantity in, because the cable gets only half the quantity from the battery in half time; similarly you give only a third of the quantity in each application when you triple the speed of working by battery; and so on. On the other hand, the induction coils keep sending in, at each reversal of the coils, an undiminished quantity of electricity. The true comparison between the battery and induction coils would be this, fifty cells of a Daniell's battery reversed every two seconds compared with the induction coils, 100 cells of a Daniell's battery reversed every second compared with the induction coils, and 200 cells reversed every half second; the comparison being always made with the same set of induction coils. If that comparison be made, the battery action will be found in no way inferior to the use of induction coils.

2559. (*Mr. Saward.*) Without their destructive tendency?—Yes.

2560. (*Mr. Varley.*) Do you believe that the transmission could be performed by means of Daniell's

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battery almost as rapidly as with the induction coil?—I feel quite convinced that it could be performed more rapidly, for this reason, that the battery can be managed in a way that the coils cannot. I have laid this down in various published papers, as the result of mathematical theory, for diminishing, as much as possible, the inductive embarrassment. (See Appendix. No. I.) I have given two or three ways of managing the battery power, by which, undoubtedly, a greater speed of signalling can be obtained, than can be obtained by the induction coils; at the same time I am quite aware that the system of induction coils actually used by Mr. Whitehouse gives considerably greater freedom from inductive embarrassment, than the mere use of a battery with a Morse key worked in the ordinary manner.

2561. (*Chairman.*) Will you favour the committee by stating in what manner the messages were received and forwarded respectively?—At Valentia the messages were all received on the mirror instruments, an observer looking at the spot of light, and pressing a key with his hand when the spot of light went to one side and lifting his hand when the spot went to the other side. The key was arranged so that as long as it was at work a dark mark was made on a ribbon of Morse paper running through an ordinary Morse receiving instrument; when the key was up the paper was left blank. The messages were all sent and received according to the Morse alphabet, being spelt out in full; on the whole, contractions were very much avoided, the words being generally spelt out quite in full. The result of this system was that the messages were sent out and recorded, but they were not self-recorded; they were recorded as it were by human relay, the observer took the place of a relay; and in my present opinion, which, of course, may be liable to alter, a human relay will be found more accurate and reliable than any mechanical relay can be for speeds not exceeding ten words a minute.

2562. (*Mr. Saward.*) Was not this recording instrument on short circuit in the room itself?—Yes.

2563. (*Chairman.*) Was the sending and receiving continuous and uninterrupted?—No; frequently by the cable failing we were waiting for hours, and receiving nothing.

2564. (*Mr. Varley.*) Your observation about human relays of course refers only to the case of long cables working with very low power?—In telegraphs, either land or sea, under 500 miles, I dare say a common relay or a relay with improvements, a good relay in fact, may be found to be the most convenient way of receiving signals; but when there is inductive embarrassment, I feel quite confident that a human relay will give more accurate and more certain work, up to ten words a minute, say, than any mechanical relay of the best possible construction can do.

2565. (*Chairman.*) With regard to the earth currents observed at Valentia, will you be good enough to state if any and what phenomena of interest were presented to you during your observations?—Continually varying earth currents, not such as would have embarrassed signals, were observed. After the failure of the cable, I found that those earth currents could be balanced by opposing a certain measured battery electro-motive force against them. I arranged a little instrument for the purpose, which was made in the workshop there, to facilitate the reading of messages which were still expected long after the cable failed. By this instrument I could show any fraction of the electro-motive force of one, two, or three of Daniell's cells that might be necessary to balance the earth currents. This instrument would have been useful if the messages had come; as it was, it only sufficed to give me a precise measure of the electro-motive force of the earth currents, in terms of that of a cell of Daniell's battery. The electro-motive force seldom reached two cells, generally was less than that of one cell; sometimes positive, sometimes negative. When a copper-plate earth was used there seemed to be nearly as much positive as negative, and when an iron earth was used, the positive earth currents preponderated considerably, but still there would be occasionally negative currents. As the fault amounted at this time undoubtedly to very nearly a perfect earth communication at some such distance as 250 miles in a right line, I concluded that the electro-motive force of the earth current over such an arc of the earth's surface, that is to say, the electro-motive force between two copper plates buried, the one at Valentia and the other buried in the sea 250 miles west, varied between such limits as I have stated, namely, generally, whether positive or negative, falling short of that of two cells of Daniell's battery, and most frequently less than that of one cell; sometimes reaching five or six cells, but that very rarely. I may mention also, as another circumstance of interest, that on one occasion (near midnight of Monday evening, Aug. 24), while waiting for signals from Newfoundland, the mirror was found to be violently deflected at Valentia, so much so, that it had the appearance of being broken from its suspending thread; it turned out to be simply that the mirror was pressed forcibly against the stop at the extreme end of its range. While I was looking into the mirror to ascertain whether there was any such accident, it suddenly turned round and went to the other side, there being no battery applied at all at the Valentia end. When communication was re-established, I asked what was wrong, and was told that a violent thunderstorm had been experienced at Newfoundland. "Great deflections and end put to earth for half-an-hour;" this precaution having been very properly taken by the director of the station there, to prevent the possibility of damage to the cable from lightning.

2566. You remained at Valentia for some time after the cable was silent; did you observe, during that period, the indications on the instruments technically known as reversal signals, that is, currents alternately negative and positive?—Occasionally.

2567. Were you satisfied that those reversals were transmitted to you along the cable from Newfoundland?—I was not satisfied that it was so then, I am now.

2568. Did you cause reversal signals to be transmitted from Valentia?—We sent words regularly, and occasionally they sent reversals.

2569. How did you send them?—By battery.

2570. Are you aware if they were observed at Newfoundland?—Some few indications were observed towards the end of September, I have been led to understand.

2571. You said that at the time you were not satisfied that the reversals which were transmitted came from Newfoundland, but you are satisfied now that they came. In what way have you been satisfied?—Because at the time I found that artificial faults could often be treated so as to simulate signal currents, so much do the faults occasionally vary. This imitation probably arose from bubbles of gas rising at regular intervals, giving indications not at all unlike the indications that came from regular reversals. I think it is possible that some of the indications may be explained by such action of the fault. I am satisfied that many of them were really from the coils at Newfoundland, because I learned that very frequently, if not quite regularly, the coils were applied to produce mere reversals, and what took place about the 20th of October, proves that the cable was then in such a condition that it was possible that the effect of such an application of the coils could be perceived.

2572. What did you observe?—Regularly alternating motions of the needle. I may mention, that the receiving instruments after my return to Valentia were made more and more sensitive almost daily; the first messages were received with so little wire in the coil of the receiving instrument as to give resistance equal to from 5 to 10 miles, and at the same time powerful magnetic adjustment was applied to direct the needle very forcibly into its position of equilibrium. The quantity of wire used

in the receiving instruments was increased almost daily, and the amount of directive power of the magnets diminished, and by these means we still managed just to keep reading the messages at no diminished speed on the whole, until towards the end of August there came to be longer and longer intervals of total confusion, when either nothing would come, or violent disturbances would be all the final indications of the needle. During the whole of the month of September the receiving instruments were kept highly sensitive, so sensitive, that I have no doubt, if the operator at Newfoundland had persevered in sending messages instead of merely sending reversals, we should have had many messages during that month.

2573. Will you state any other phenomena of interest which occurred during your period of experiment and examination at Valentia, after the failure of the cable?—My experiments at Valentia, after the failure of the cable, were chiefly directed to testing the insulation. Many experiments were made in artificial faults and elaborate comparisons with the indications of the cable. The result was such as to confirm the opinion which Mr. Varley and myself had previously expressed: but further to confirm myself in the very low estimate I had made of the insulation at the fault, or the very high estimate I had made of the amount of the defect. I left Valentia at the end of September, convinced that there could not be more than a resistance equal to two or three miles of cable, when the fault was cleared to the utmost by negative currents. It might rise to 15 or 20 miles more by the application of a positive battery current for some little time.

2574. You subsequently went with great care through the electrical records of both Newfoundland and Valentia stations, did you not, when you had returned to Glasgow?—I only had the Newfoundland records for two days in my possession; for a few hours, all the time I could possibly devote to them, during that time I went through them as carefully as possible.

2575. Will you favour us with such results of that examination as you consider to be worthy of record?—The very defective state of the cable at the beginning was demonstrated by the Newfoundland records, which show that they were working on at a time when we were looking out to receive with instruments which, on board ship, would have given large effects, and also show an excessively defective condition of the cable at the time that Mr. France was making his test.

2576. What was that test?—He applied 40 cells of a Daniell's battery at the Newfoundland end, first positive to line and then negative to line, having previously directed those at the other end to notice the results and communicate them back by telegraph. After he concluded his tests messages were sent asking him to give the result, and for two or three days after he left, the message was repeated, but never answered, and seemed never to have been read. I have not had an opportunity of going through the diaries with sufficient minuteness to ascertain which of our unanswered messages had been read. I believe that message was never read, or else it would have been answered. On looking at what the results were according to the Newfoundland diary, I find a common detector needle, the only one used, that being according to Mr. France's direction, and that it went over to the other side, gave a good indication, varying to 40 cells positive, then reversed distinctly at the end of the half hour; after that it wandered from one side to the other, and after that it gave no indication whatever of the negative current which was applied during the second half of the hour.

2577. On what day was that experiment made?—It was made in the night 22nd to 23rd of August. I may mention that after Mr. France's tests, the cable appeared to have suffered so much, that for hours and hours it failed to transmit signals. The battery application which Mr. France made, could not, however, have done any harm whatever to the cable, because it was less than the battery application that was absolutely necessary to move their instruments at Newfoundland. It appears that the half hour's cleaning by the negative current, produced a diminution of insulation in the cable that was not got over for a good many hours.

2578. It required hours of application of the positive current to overcome that effect of the negative current?—Yes, or of leaving the cable to itself. It seems gradually to have mended again, so that the signals were again received. Its capability of conveying signals seemed to vary, going and coming at uncertain intervals.

2579. (*Mr. Saward.*) I believe two messages were sent for Her Majesty's Government after that?—Yes; and some of the best messages from each side.

2580. (*Mr. Varley.*) After the fault was temporarily healed?—Yes; in fact, many messages were sent, when the cable was in such a state that forty cells applied at one end would make no sensible effect at the remote end.

2581. When the cable got into a worse condition, and the fault got much more steady, you were able with a very much reduced speed of signals to read better, owing to the faults being more steady?—Partly to their being more steady, and partly to the instruments being made more sensitive.

2582. I have very often seen in telegraphic wires in the ocean where we have a cable going bad, when the cable ultimately got very bad, it was our last resource; but we were able, by setting our apparatus very delicately, to continue its working, because the signals came much more steadily?—That is quite possible, and it would explain many of the variations that we experienced. The last point of interest that I noticed in the Newfoundland diary was, the original of the message which was read on the 20th of October; the message was entered in the Valentia signal diary as being read thus, "Two hundred and forty tk—" (? two) Daniells now in circuit." That was the reading as entered in the Valentia diary. The message really sent was, "Two hundred and forty trays "and seventy-two liquid, Daniell's now in circuit." So that the word that could not be made out was "trays." That was the last effort.

2583. That was the last message received through the cable?—Yes. A *bad fault* appears to have originated, immediately after that, at some distance, supposed by the Newfoundland operators to be about 400 miles from their station. It is impossible now to say for certain whether the fault which then appeared was really at the distance supposed, or was much nearer. It varied so much in resistance subsequently, that it may possibly have been in reality a beginning of the flaw which suddenly opened out between the evening of Saturday the 15th January and the following Monday morning, and gave signs of almost total loss of insulation, at a distance estimated to be be about 20 miles from the Newfoundland end. If 20 or 30 miles of the cable are lifted the near fault will be reached, and then, but not till then, can it be discovered whether or not there is also a fault about 400 miles beyond.

2584. (*Mr. Saward.*) Are you aware that 14 miles have been lifted this season by Captain Kell, and tested by the system sent out by Mr. Varley, and that the result is, that the fault is put out 12 miles further?—I am not aware of that.

2585. (*Mr. Varley.*) You saw the tests that were made at Newfoundland at the time these batteries were put on?—Yes.

2586. From what you saw of those tests, do you think any conclusion could be arrived at as to the state of the cable between the fault at Newfoundland and deep water, indicating that no damage was sustained by the great pressure to which the gutta-percha was subjected by being at the bottom of the Atlantic?—I think the satisfactory state of testing from the Newfoundland end before the 20th of October is demon-

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strative against any hypothesis that the cable suffered damage in deep water towards that side.

2587. In fact, you think that the cable appears to be in no better or worse state as regards insulation, judging *from* those experiments?—I could not judge from those experiments, as to the precise degree of insulation, because there was no accurate testing of the cable before it went to sea to compare with. There had never, in fact, been a measurement of the resistance *to a* current entering the cable. Till Mr. Varley's coils were brought to Valentia there was no measurement, except by guess, as to the resistance, at either end of the cable. I made as close estimates as I could, after going to sea. I had urged that there should be resistance coils got in the spring of 1857; but I was living at a distance, and had nothing to do with the testing then. When at Valentia, in August 1858, I found the cable getting worse, I wrote to Mr. Saward for resistance coils. Mr. Saward was just leaving London, and it was impossible to get them made in time. I therefore had several more, such as I had previously prepared at sea, made in the workshop at Valencia, of covered copper and brass wire of my own; and soon after Mr. Varley came with his, which proved most valuable. If such coils as Mr. Varley had used habitually had been applied to testing the cable before it went to sea, and between the different trips, to ascertain its state, there would be some definite knowledge upon this point.

2588. (*Mr. Saward.*) Should you consider that the mechanical effect of the pressure of the sea upon the gutta-percha would be to force the pores of the gutta-percha closer together, and thereby make it a better insulator, or to cause the water to permeate those pores and get to the copper wire?—If there is a flaw, I believe that the pressure will tend to send the water in.

2589. If the cable is laid in a perfect state?—If the cable be laid in a perfect state, I do not think that any amount of pressure of the water upon it would give any tendency whatever to let water get through the covering to the copper wire.

2590. (*Chairman.*) Have you made any experiments upon that subject?—I never have made experiments myself, but I know that pieces of gutta-percha have been tested under extremely high pressure in Bramah presses.

2591. Up to what pressure?—I cannot say; I have understood they have been under pressure corresponding to the great depths. I know that cables are regularly tested under pressure at the works at Birkenhead of Messrs. Newall and Company.

2592. Up to nothing like that pressure. There is nothing beyond 1,000 lbs. per square inch?—Mr. Whitehouse mentions experiments that he had made, but I cannot charge my memory as to the precise amount of pressure. He told me he had applied very high pressure to a piece of gutta-percha core, and found that it made not the slightest difference in the insulation. Judging from all we know of the properties of gutta-percha, and the constitution of gutta-percha, and the general physical qualities of matter, I feel perfectly convinced that a piece of sound gutta-percha would not be at all influenced in its insulating powers by being subjected to the greatest water pressure that it can possibly experience at any depth in the ocean.

2593. You think water will not penetrate gutta-percha when submitted to it equally on every side?—I am firmly of opinion that it will not, judging from the general properties of matter, and the constitution of gutta-percha.

2594. To come to the general question of conductors and insulating substances, will you favour us with the deductions you have arrived at from experience, and, first, as to the proper size of a conductor best adapted for rapidity of transmission in cables of the lengths respectively of 1,000, 2,000 and 3,000 miles?—It is impossible to give a general answer as to the question for a thousand miles, there are so many circumstances to be taken into account; but, for a two thousand and three thousand miles' length, I should say that the largest diameter of gutta-percha sheath that is advisable, should first be considered; that must depend upon the mode of laying that is to be followed, whether it is to be laid in one length, what stowage room can be had in the ship, and, if not limited by those considerations, next must be considered to what expense it is desirable to go, deciding by such combinations of considerations, what is the best outer diameter of gutta-percha. Then, I think, the diameter of the copper ought to be kept considerably within a certain proportion of the diameter of the gutta-percha, which is determined as giving the maximum speed. The reason that I say it should be kept within that is, that a considerable deviation from the maximum will make very little deviation in the speed, while diminishing the size of the copper wire is advantageous in two ways,—first, by lightening and diminishing the specific gravity of the cable; and secondly, by increasing the thickness of the insulating medium, and so diminishing the risk of flaws, especially by preventing the copper wire from having the same tendency to go through the gutta-percha in bending that it may have if there be but a very thin coating of gutta-percha around it. That maximum I calculate by a simple process from a logarithmical formula which I shall communicate to the Committee in writing, as also a table of the speeds depending on greater deviations from the maximum. (See Appendix No. III.) Judging from that table then, I would take the diameter of the copper wire within that which gives the maximum speed, so as not seriously to diminish the speed, but at the same time so as to considerably diminish the weight of the copper wire. The diameter of the conductor which I find would give the maximum speed of signalling is, as shown in the tables, somewhat over three-fifths of the outer diameter of the gutta-percha. A copper wire half the diameter of the gutta-percha would give 94 per cent. of the maximum speed; and a copper wire two-fifths of the diameter of the gutta-percha would give 80 per cent. of the maximum speed. With farther diminution of the diameter of the conductor the speed falls rapidly. It should not be forgotten, however, that to improve the conductivity upon what it has been hitherto in all cables is the first thing to be done, to increase the speed, and has none of the danger attending a too great increase of the mass of the conductor.

2595. Will not that table show what you consider to be the ratio in which external surface in conductors increases the retarding induction as compared with the concomitant increase of conductivity arising from the greater cubical content of the same conductor?—Yes.

2596. Supposing that a copper wire of No. 14 gauge were surrounded with a spiral covering of steel wires of the gauge No. 22, and these two metals so united were used to form the conductor of a cable 2,000 miles long, what should you expect to be the effect of this combination of metals conducting in two different ratios on the total conductivity of the cable?—The total conducting power of the cable would be the sum of the conducting powers of the two parts; by the word "conducting power," I mean the reciprocal number measuring resistance. Practically I may say in such a specimen as this (*Mr. Allan's cable*) the conducting power of the whole would be scarcely sensibly increased by the addition of the steel.

2597. It would be the conducting power of the copper alone?—There would be but little increase due to the conducting power of the steel; but on the other hand there would be very great loss of speed due to the increased capacity produced by the presence of the steel between the copper and the insulator.

2598. That is to say, you would require a thicker insulating medium round that core?—Yes; in no case can it be advantageous to introduce any other metal than copper of the highest conductivity for a conductor, unless some other metal than copper can be found of higher conductivity.

2599. The object of that cable of Mr. Allan's is not

to increase the conductivity of the cable, but to place the whole strength of the cable in the centre, it is a mechanical arrangement?—Mechanically I consider it is also very bad, principally for this reason that it puts the most delicate part outside, and gives the thing that bears the strength an interior position; the result would be, that there would be nothing whatever to protect the insulation against abrasion. It is a submarine cable about as well planned as an animal planned with its brains outside its skull.

2600. (*Mr. Saward.*) It is argued that that system of cable is strictly following nature, when it places the bone in the centre of the arm?—I have no doubt you will find a precedent for almost anything in nature; there is every possible inversion to be found in nature.

2601. (*Chairman.*) You think, both in an electrical point of view, and in a mechanical point of view, the arrangement of Mr. Allan's core, as shewn to you, is objectionable?—Certainly; in both respects.

2602. (*Mr. Varley.*) Are you acquainted with the nature of the arrangement of Mr. Whitehouse's compensation, by which he states that he was able to compensate for the irregularity of the waves occasioned by the difference in the time of signalling them, or in producing dots and dashes; are you acquainted with its nature and its value as regards the practical application to a submarine line?—I am not at all acquainted with either its nature or its value. There was an appliance for producing something of the kind on the coil apparatus at Valentia, but it was never got into use.

2603. (*Mr. Saward.*) The compensation spoken of is one which was attached under the pressure of a key?—Yes, I am aware of the position; but it was never used at Valentia to the best of my knowledge.

2604. In consequence of what has occurred with respect to the repeated failures, the ultimate success, and again the failure of the Atlantic cable, has your confidence been shaken in the future permanent success of a properly constructed and properly laid Atlantic cable?—My confidence has not been in the slightest degree shaken.

2605. Do you consider that there are any great or serious obstacles, after the experience which has been gained, in the way of making and laying such a cable as would be permanent?—I do not think that there are any serious obstacles whatever. I consider that there are difficulties to ensure freedom from flaws in the manufacture.

2606. That is a question of good business arrangements and proper attention?—Entirely so. I consider that a cable might be laid with the very best possible prospect of success, with very small chance of damage across the Atlantic. I believe that a cable might be laid now with certainty.

2607. Could you indicate any experiments which you think it would be desirable to try upon certain points connected with deep-sea telegraphy before proceeding to lay another cable?—The most important subject I can see just now for experimenting upon with that view, is the detection of faults, the way to detect faults which do not sensibly impair the insulation of a cable coiled up and dry.

2608. Would that be accomplished satisfactorily by making and subsequently attempting to detect artificial faults?—Very much so; a great deal could be done in that way, but no system of detecting faults can possibly supersede the most watchful care upon every point connected with the manufacture of the cable.

2609. (*Chairman.*) Have you seen this insulator of Mr. Daft's, and formed any opinion upon it?—I do not think india-rubber is strong enough. I cannot be positive that we know from experiments that it is so. As far as I can judge, gutta-percha is a much tougher and stronger substance than any india-rubber that could be found; and gutta-percha free from flaws is certainly a good enough insulator for the longest submarine cable yet projected. If the Red Sea cable lasts perfectly, the difficulty as to heat may be considered as having been satisfactorily settled; for no cable need be more exposed to heat than that necessarily will be.

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2610. (*Mr. Varley.*) What would be the result of adding a resistance at one end of the cable equal to the resistance of the cable itself, of course without any inductive action. Supposing resistance coils equal to 2,000 miles of cable were added to the sending end, what would be the effect on the speed of transmission through the cable?—It would somewhat diminish the speed of transmission through the cable, I believe, but not to anything like the same extent as adding an equal length of submerged cable would do. It is easy to calculate what the effect would be precisely; but judging from general knowledge, it is as I say. It would diminish the speed, but not to anything like the same degree that an additional equal length of submarine cable would do. If, however, the resistance added, instead of being equal to that of the whole cable, bear but a small proportion to that of the whole, the effect in retarding as well as in weakening the signals will be sensibly the same as that of adding an equal length of actual cable, with its regular electrostatic capacity; and this statement holds, to whichever end of the cable, whether the sending end or the receiving end, the supposed addition of a resistance wire without electrostatic capacity be made. Thus, for instance, if in working through a submarine cable 2,000 miles long, a battery or coils exercising a resistance equal to that of 150 miles of the cable be used to transmit from from one end, and an instrument with a coil resisting as much as 183 miles of the cable, to receive at the other end, the speed attainable with such arrangements will scarcely exceed what could be obtained through a cable 2,333 submerged miles, by using sending and receiving instruments constructed to exercise no sensible resistance. The loss of speed in this case due to the supposed resistance in the sending and receiving instruments (which is far from an over estimate, but is rather an under estimate of the resistances admitted by some of even the best instrument makers,) cannot be much, if at all short of 30 per cent. of the whole working speed attainable. I make this remark to show the great importance of what I have always urged for the improvement of telegraphic instruments, namely, the diminution to as low an amount as possible of the resistance, both of batteries or sending coils, and of receiving instruments.

2611. If that resistance were added to the sending end, would it not produce a great amount of retardation at the receiving end?—It would produce no more than if applied at the receiving end. The effect on the rapidity of signalling as well as on the strength of signals is precisely the same, whether the supposed resistance be added to the sending or the receiving end.

Appendix No. I.

Description of Professor Thomson's Diagrams.

At the request of the committee the accompanying diagrams of curves, illustrating the conclusions of my mathematical theory as to the effects of electrostatic induction in diminishing the sharpness of signals transmitted through a submarine cable, are added, as an extension to the answer given before the committee to question No. 2452.

In figure 1, curve No. I, represents the strength of the current passing through one end of a cable if put to the earth, when, after the cable has been in a perfectly discharged condition, one electrode of a constant battery of small resistance is suddenly applied to the other end of the cable, while the other electrode of the same battery is put to earth. Curve No. II. similarly represents the current through the end to earth, consequent upon a very brief application of a very intense battery or of electromagnetic induction coils at the other end. Of the smaller curves under No. I., the first (1.) represents the rise and fall of the current through the remote end, when the end of the cable operated on is put in connexion with the earth, after having

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been kept for a certain time (T.) in connexion with the battery; (2.) represents similarly the effect of the battery for a time 2 T; the third (3.) for a time 3 T, and so on.

The horizontal lines measured from the left of the diagram, represent durations of time from the instant of first application of the electromotive force, the length of the chief divisions in these lines represents the interval of time denoted above by capital T. The distance of any point in any one of the curves measured perpendicularly from the lowest horizontal line of the diagram, represents the strength of the received current at the remote end of the cable at the instant of time represented on the horizontal scale in the manner which has been explained. Thus the principal curve (1.) represents the rise of the current in the remote instrument, when the end operated on is kept permanently in connexion with the battery. It so nearly coincides with the line of abscissas (that is to say, the lowest horizontal line of the diagram), at first as to indicate no sensible current until the interval of time corresponding to T has elapsed.* After the interval T, the current very rapidly rises, and after about 4 T more, attains to half its full strength. After 10 T from the commencement, it has attained so nearly its full strength, that the farther increase would be probably insensible. The full strength is theoretically reached only after an infinite time has passed.

Figure 2 represents the same series as (No. I., 1, 2, 3, 4, 5, &c.); but on a scale reduced to $\frac{1}{10}$ in all dimensions, to allow an extension of the series to longer periods of time to be included.

Figure 3 represents, on similar principles, the current through the remote end of the cable, when at the end operated on, positive and negative battery applications are made alternately during successive equal intervals of time, the length of each interval being equal to that denoted by T in the preceding statements. The chief curve in black throughout represents the effect of eight such battery applications, four positive and four negative, with the operating end of the cable put to earth in conclusion. The curve beginning with this chief curve and continuing along the first long dotted line, represents the effect of a single positive battery application during the interval T. The curve beginning with the same chief line, and continuing by the first long and short dotted line, represents the effect of one positive and one negative battery application. The same chief curve, continued along the second long dotted line, represents the effect of a positive and a negative and a positive battery application. Similarly the effect of a positive, and a negative, a positive, and a negative, are represented by the second long dotted line.

Figure 4 represents a similar series of effects, modified by the diminution of the initial battery application to half strength; this being a rough approximation to what the mathematical theory indicates as the proper compensation of the first order for inductive embarrassment in a series of signals of the kind aimed at in the operations of which the effect is represented in figure 3. The complete series of such signals would mark four dots were it not for the inductive accumulation in the conductor; how far it is from doing so when worked at the speed specified is shown by the chief curve (black), which only cuts the datum line once, and therefore represents a case in which a relay would return only once, after having been moved once from its position of rest, which may be supposed to be that to which it is thrown by a negative current. Thus, instead of the four dots, the relay would mark one long line. On the other hand, of the succession of signals to be produced, not one is lost when the operations are made in the manner described with reference to figure 4. A common relay would mark as the results four not very unequal lines, which could hardly be mistaken for anything but "four dots." These two diagrams may serve to illustrate the importance of the principle of compensation which the mathematical theory has suggested. I have proposed various modes of working it out in practice, which although substantially the same in principle as that which is partially represented in these diagrams, involve various modifications of detail which cannot be here explained.

* Strictly speaking the effect at the remote end is instantaneous, *i.e.* according to data limited as regards electricity, to such as those assumed in hydrodynamics when water is treated as if compressible, or the velocity of sound in it considered infinitely great, which requires instantaneous effects to be propagated through the whole mass of the water, on a disturbance being made in any part of it.

Appendix No. II.

Account of the Accident, July 29, 1858, given in writing by Professor Thomson to Capt. Preedy, R.N., at sea, during the laying of the Cable.

"July 29, 1858.—Immediately after 10 p.m., Greenwich time, at the beginning of what ought to have been the answer to our 40 mile signal, the current from the 'Niagara' failed. Tests then applied showed a breach of continuity, far on the line, with no loss of insulation. As this happened while proceedings in the hold were being taken to put right a piece of the cable which had suffered injury, probably in the gale, it seemed likely that the interior conductor might have sprung in consequence. After repeated trials, however, by pricking through the gutta-percha and testing, it was ascertained that the whole cable in the ship, up to a point very near the outgoing part, and including the mechanically defective spot, was perfect electrically. The core was then cut at the place of the injury, and the end leading to the outgoing part was tested. The electrical indications still showed the same defect as before.

"The insulation was so perfect as to reduce to a very small amount the constant current from a powerful battery; but the capacity of the wire for charge was so great as to show that the fractured end must be at a considerable distance; probably more than 50 miles, though certainly much less than 200.

"The severed conductor was then re-united and tested, and the strands of the cable were put together and secured round it, just soon enough to go overboard when required. Great credit is due to those who conducted this most critical operation in the hold under so urgent pressure of time. An anxious quarter of an hour followed, during which the soundness and strength of the splice were proved by submergence in water two miles deep.

"The insulation and the want of continuity continuing as before, nothing remained but to wait for, at least, the time prescribed by the Board, paying out only enough to prevent the cable breaking away from the ship's stern. Accordingly, as soon as such a length had been allowed to run out very freely, as should put the splice beyond the reach of strain, the brakes were loaded, and the ship was moved ahead at a slow rate, so that, while waiting against hope for the recovery of the electric circulation, as little as possible of the cable should be thrown away. A few minutes later (at 12.27 p.m., Greenwich time) the electrical tests showed instead of insulation 'dead earth,' in some position between the ships, or not far from the outgoing portion in either, and the too probable conclusion that the cable had parted, nearly destroyed the little hope that was left. That the cable had been cut on board the 'Niagara,' and had not parted in the sea, became clear, as after three minutes more, that is to say, at 11.30, a strong current came through the cable—so much stronger than any that could be expected from the earth, as to be only attributable to the batteries in the 'Niagara,' and signals were now interchanged between the two ships. Immediately the brakes were released, and the 'Agamemnon' was moved ahead up to regular speed.

"At 12h. 0m. (midnight, Greenwich) the 'Niagara' signalled 40 miles paid out; and at 12h. 30m. the signal from the 'Agamemnon' was '50 miles.'

"The electrical tests continued to indicate earth in the same position as before, until some time between 12.20 and 12.30, when it appeared the 'Niagara's' cable must once more have been thrown into circuit.

"Until intelligence is received from the other side of the Atlantic, it is impossible to say for certain where the breach in the conducting wire was: if it had been beyond the point where the 'Niagara' cut, we should probably have found 'air,' not 'earth,' during the time which passed before their battery was applied to the cut end; and it therefore seems more probable that it was between us and that point, and must have come together again while those in the 'Niagara' were waiting for signs of continuity after they had cut, and we after we had cut and mended."

W. Thomson.

Appendix No. III.

Formula and Table referred to in answer to Question 2594.

Let D denote the diameter* of a copper wire of circular section symmetrically covered with gutta-percha to a diameter D^1; let also I denote the specific inductive capacity of gutta-percha, and let log. denote the Napierian logarithm of whatever symbol it is prefixed to; my expression for the electro-static capacity of an unit length of conductors thus

* When the copper conductor is of strand instead of solid wire, and is therefore not of simple circular section, the capacity will be nearly the same as, but in reality a very little less than, that of a solid wire of circular section, equal to a circle circumscribed about the different wires of the strand. It is a mistake sometimes made to suppose that the multiplication of the surface in a strand conductor gives rise to a multiplied capacity.

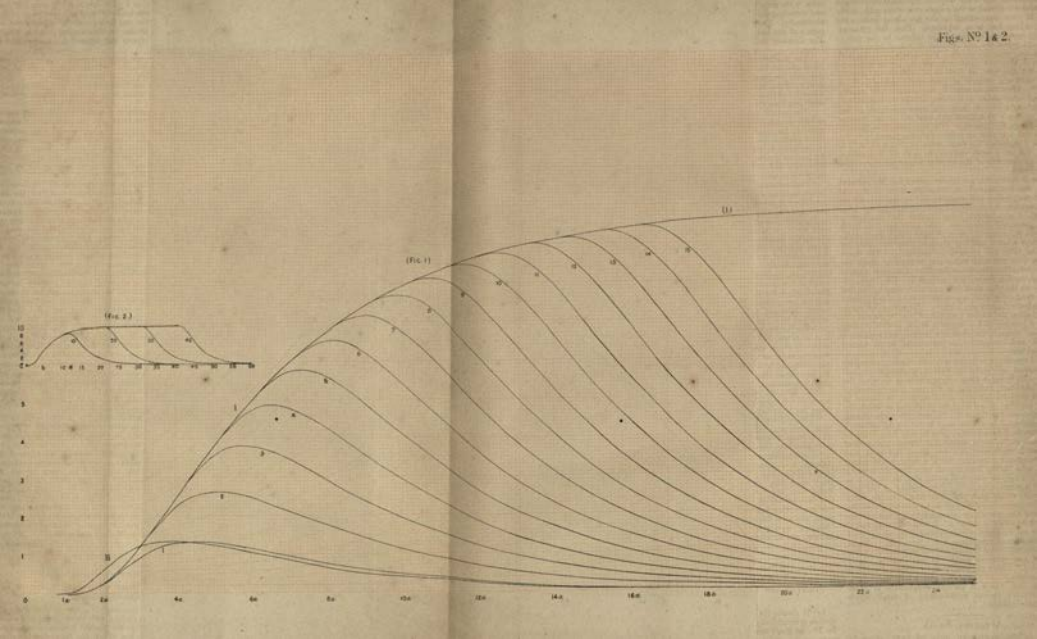
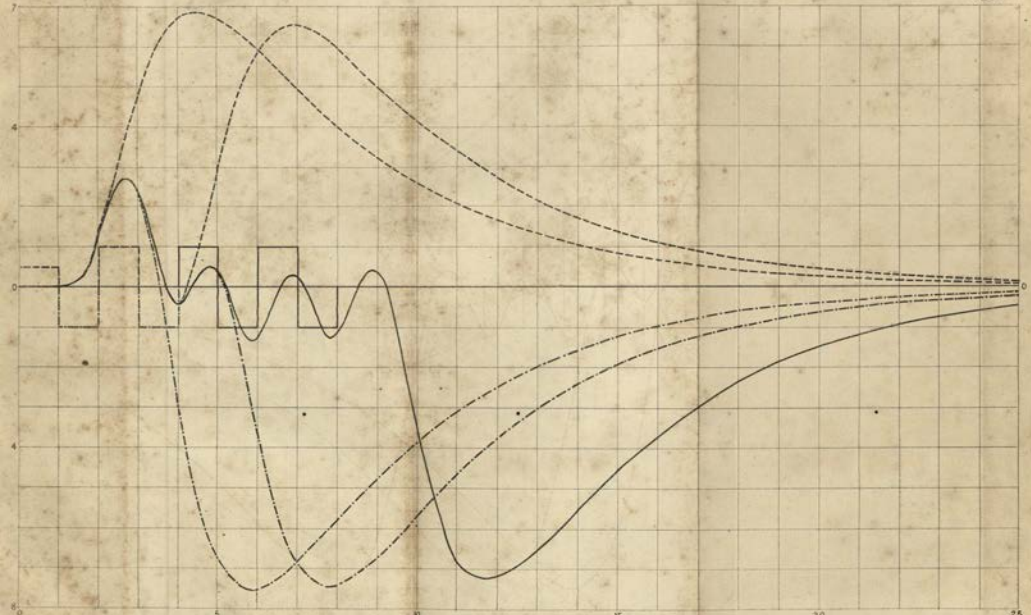
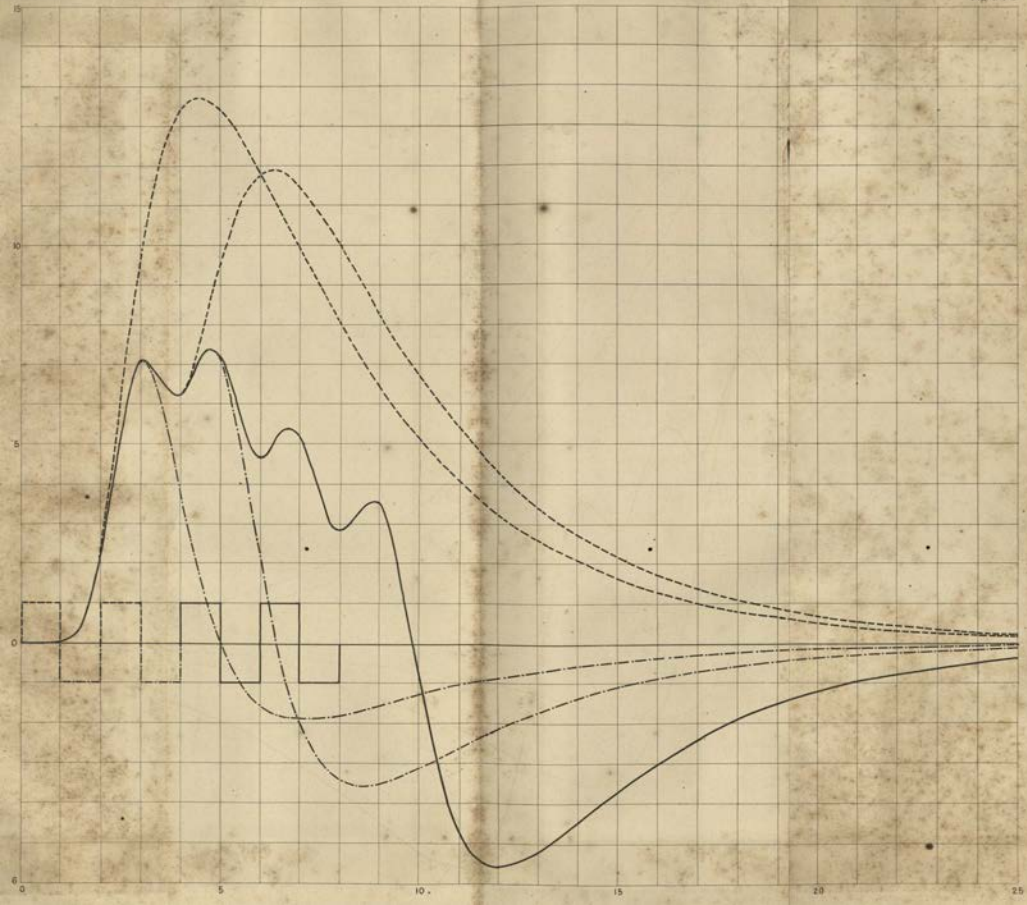

(Fig. 1)
(1)
(Fig. 2)

Fig. N° 3.




15
10
5
0
0
5
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15
20
25

insulated, when the outside of the gutta-percha is kept in communication with the earth, is—

$$\frac{1}{2 \log \frac{D^1}{D}}$$

This is the mathematical formula which I stated in answer to Question 2594. It applies either to a mere gutta-percha covered wire immersed in water, or to a gutta-percha covered wire surrounded by tow, and sheathed with iron in the ordinary manner of submarine cables, the tow perfectly moistened, as it is in the circumstances, being so far as the electric action to which it is exposed is concerned, a good enough conductor to show no sensible difference from what would be found from an absolutely perfect conductor in its place.

According to the other answers which I have given with reference to the general results of the mathematical theory of signalling through submarine telegraphs; the rate of signalling attainable through a stated length of submerged wire is inversely proportional to the capacity, and directly proportional to the sectional area, and to the conductivity of the metal employed for the conductor, hence for the same quality of wire the rate of signalling is proportional to—

$$D^2 \log \frac{D^1}{D}$$

The questions at present to be answered are,—What diameter of copper wire with a stated outer diameter of gutta-percha would give the most rapid rate of signalling, and what comparative rates would be attained with any other diameters of copper wire. Supposing D^1 to be given, and D to be variable in the preceding formula, we find, by the ordinary method of the differential calculus for maxima and minima, that $\frac{D^1}{\sqrt{\epsilon}}$,* or $D^1 \div 1{\cdot}649$, is the value of D, which makes the value expressed by the formula the greatest possible.

The following Table shows the rates of signalling with various diameters of copper wire inside a gutta-percha insulating coat of one constant outer diameter, in terms of the maximum rate reckoned as 100:—

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Ratio of Diameter of Conductor to Outer Diameter of Gutta-percha, $\frac{D}{D^1}$.	Ratio of Outer Diameter of Gutta-percha to Diameter of Conductor, $\frac{D^1}{D}$.	Rates of signalling attainable, $200\,\epsilon\left(\frac{D}{D^1}\right)^2 \log \frac{D^1}{D}$.
·1	10	12·52
·2	5	35·00
·3	3·333	58·91
·4	2·5	79·71
·5	2	94·21
·5263	1·9	96·62
·5556	1·8	98·61
·5882	1·7	99·81
·6	1·667	99·96
·6065 $= \frac{1}{\sqrt{\epsilon}}$	1·649 $= \sqrt{\epsilon}$	100·00
·6250	1·6	99·82
·6667	1·5	97·91
·7	1·429	95·54
·7143	1·4	93·33
·7692	1·3	88·20
·8	1·25	77·64
·8333	1·2	68·83
·9	1·111	46·94
·9091	1·1	42·82

It is easy to understand why there should be a particular diameter of copper, which will give a maximum rate of signalling with a stated outer diameter of gutta-percha, since, if the copper wire is too small there is a loss of speed, owing to too large resistance not compensated by the smallness of the electro-static capacity; while, on the other hand, if the diameter of the conductor be too large, as for instance, if it fall but little short of the outer diameter of the gutta-percha, the thinness of the gutta percha coat gives rise to a greater loss of speed by increased capacity than is compensated by the gain owing to diminished resistance.

* ϵ denotes the base of the Napierian system of logarithms, of which the value is 2·7182818.

Adjourned to Tuesday next at Two o'clock.

Tuesday, 20th December 1859.

PRESENT:

Captain Douglas Galton.
Professor Wheatstone.
Mr. Saward.

Captain DOUGLAS GALTON in the Chair.

William Henry Preece, Esq., examined.

W. H. Preece, Esq.

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2612. (*Chairman.*) I believe you are a telegraphic engineer?—I am.

2613. Have you been connected for some years with telegraphy?—About nine years.

2614. What lines have you been connected with?—During that period of nine years I was acting for about three years as assistant engineer to the Electric and International Telegraph Company, and for four years I have been the superintendent of the South-western district of that Company, embracing about 3,000 miles of telegraph. I am now also engineer to the Channel Islands Telegraph Company.

2615. Have you had any experience in submerging cables?—No; my experience in submerging cables has been small, except in the operation of repairing. The only cable which I have been engaged in submerging was the Irish cable, between Dublin and Holyhead.

2616. What size is that cable?—It is a small cable, similar to what is generally called the Hague cable.

2617. Does that contain one wire?—One solid copper wire.

2618. (*Mr. Saward.*) You are speaking of the former Hague cable, are you not?—Yes, we call the small cable the Hague cable; the Zandvoort cable is a large cable.

2619. (*Chairman.*) What is the size of the outer covering of the Irish cable?—No. 8 wire.

2620. Did you submerge the Channel Islands cable?—No, it was submerged by the contractors, Messrs. Newall and Company.

2621. What size cable is that?—It is a single conductor of stranded wire, No. 14 gauge, covered with gutta-percha, surrounded with hemp as usual, and protected by nine No. 6 iron wires. It weighs about $2\frac{1}{2}$ tons to a mile.

2622. What length is that line?—102 miles of submarine wire, and about 22 miles of underground wire. I believe the circuit is about 124 miles in length.

2623. What is the greatest depth at which the Channel Islands cable is submerged?—About 60 fathoms.

2624. Have you experienced any difficulties in the working of that cable since it has been laid?—We have had considerable difficulties from accidents to the cable; when the cable was first submerged it tested and worked beautifully.

2625. What is the weight and thickness of gutta-percha per mile in that cable?—It is about $\frac{3}{8}$ths of an inch thick, No. 1 gauge gutta-percha covered wire; I do not know the weight.

2626. Are there two coverings?—It has three coverings of gutta-percha to it, forming altogether a No. 1 gauge, similar to most of the cables that have been laid.

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2627. (*Mr. Saward.*) Is it not about the size of the Atlantic core?—Yes; but rather smaller. The Atlantic we call No. 0; this is No. 1; and No. 1 is the size almost invariably used for submarine cables.

2628. (*Chairman.*) In what condition was the Channel Islands cable when first laid?—Very good.

2629. Was it in good condition when you first came to it?—Yes, in perfect condition.

2630. Has it not since deteriorated?—Yes, but it has deteriorated from causes different to those affecting any other cables. The causes which have tended to injure the Channel Islands cable have been entirely natural causes, such as friction upon rocks from motion at the bottom of the sea; in fact I have brought with me several specimens of the accidents that have occurred to this cable. (*The witness produced several specimens of broken cable.*)

2631. When you speak of its being injured from natural causes, you do not mean that it has been injured by anchorage?—No, we never had a single accident from anchorage.

2632. Is the bottom over which the Channel Islands cable passes rock or mud?—Rocky the whole way; in some places there is sand, but it is chiefly rock.

2633. Is there any shingle on the coast?—In one or two places.

2634. Does the shingle injure the cable?—Not at all.

2635. Does the cable become embedded in the shingle?—Yes.

2636. And the shingle adheres to it?—It forms a sort of concrete around it.

2637. Which prevents any further decay of the iron taking place?—Yes.

2638. (*Mr. Saward.*) Are not some of the rock strata at the bottom of the sea in that district metallic?—In the neighbourhood of the Channel Islands they are granite; and along the coast of England it is the Portland, a sort of oolitic stone, I think.

2639. You do not go across any copper?—No.

2640. (*Chairman.*) Will you describe the action which the sea causes upon the cable?—I think I could not do better than describe to you, in succession, the accidents we have met with. I must tell you that this cable has only been submerged 17 months, and during that 17 months we have had four breaks.

2641. What was the date of its submergence?—August 1858. The first accident that we had was in February; it took place on the Jersey shore, where the landing place is very rocky and precipitous. The cable, which was a very stout cable, had been submerged between some rocks, but had not been fixed to those rocks. After a very violent gale the whole of the sand in the "little" bay in which the cable was landed was washed away; the cable became loose, and the effect of the heavy waves rolling in brought the cable upon a rock, and after striking repeatedly against the edge of the rock it broke clean off.

2642. Were the shore ends of the cable thicker than its body?—Yes.

2643. What was the size of the shore ends?—The shore ends were nearly the same size as those of the Atlantic cable.

2644. Were they anything like so thick as the Gibraltar No. 1 shore end?—Yes, about the same size.

2645. (*Mr. Saward.*) As nearly as you can recollect, what time did it occupy to effect the breakage shown by the specimen you have produced?—The breakage occurred after a series of very severe gales. I should think that it could not have been under action for less, perhaps, than a month.

2646. Subjected to these influences for a month?—Yes. That breakage took place at very low-spring water mark, so that we very easily repaired it, and we did so by substituting a much stouter cable. I took the opportunity also of strengthening every shore end that I had, of which there are five landed in different rocky places, by clamping them well into the rock, and putting down a very stout cable.

2647. (*Chairman.*) How did you fasten them to the rock?—With iron crutches, forming a sort of fork with a long foot to it, which we leaded into the rocks. We rested the cable in this fork, and then screwed a piece of iron very tight upon it, covering the cable with a bed of spun-yarn.

2648. Was it done by divers?—No; it was done at low-water mark; these cliffs extend about 200 feet high, and the cable has to go over the rocks in a very zigzag way.

2649. Did the fracture take place at low-water mark?—Exactly at low-water mark, where the effect of the waves would be the greatest upon it.

2650. Has not the cable suffered since then?—Not at that part.

2651. Has the cable been broken by the sea below low-water mark at that place?—We have never seen signs of the slightest movement since it was firmly fixed.

2652. (*Mr. Saward.*) It has stood the recent heavy gales perfectly well?—Yes; it has stood many severe gales. I have examined it many times, and I cannot perceive the slightest movement of any kind. I was going to say that this cable which we put down is a very strong cable; in fact, it is the strongest cable that ever has been made. It is first of all, the small Atlantic, covered with spun-yarn, surrounded with wires of the same size as this, No. 1, Gibraltar. That again, for the whole distance where it extends over the rocks, was served with spun-yarn, and outside of all we served 20 of the largest iron wires we could get (No. 1), so that it makes the cable as stout as my arm, weighing something like 15 tons to the mile.

2653. (*Chairman.*) In what depth does that extend?—Only to low water over the rocks; so that, where it rests upon the rocks we have given it every possible strength we can, and since that has been done there has not been the least sign of any movement of any kind. Although there was not an actual accident on the Alderney shore, there was the very same thing going on, and had this accident not occurred, we should have found the same thing occur at that place sooner or later.

2654. When you mentioned the length of the cable, did you mean the length of the cable from England, or are there two lengths?—I mentioned the whole length; there are three lengths; there is the cable extending from Portland Island to Alderney, which I think is about 57 miles.

2655. Nautical miles or statute miles?—Statute miles; there is another cable from Alderney to Guernsey, about 21 miles in length; and another from Guernsey to Jersey, about 18 miles; I cannot speak distinctly to the correct distances. It was only at Jersey and Alderney that I saw any signs of this action taking place.

2656. How is the cable landed at Guernsey?—It is landed in a similar sort of place, only in laying out the cable they started from Guernsey, by which they were able to take it out of the bay in a straight line, and pull it very taut, so that it remained in a firm position by its own tautness; but as they landed the end from the ship at Jersey, there was a good deal of slack. In landing an end you cannot put on the same strain from the shore as you can on board ship; therefore the end you start from is always taut, while the end you finish with is generally slack.

2657. What was the next accident that occurred to the cable?—The next accident occurred in April, four miles off the island of Portland. This is a very curious specimen; the wire had been working upon the sharp ridge of a rock in 25 fathoms of water, and by the vibratory motion given to it by the tide, it that gradually completely sawn itself through; every wire shows signs of attrition.

2658. What was the bottom in this case?—Hard rock.

2659. How many months did it take to wear through?—From August to April, eight months.

2660. How did you remedy that defect?—By picking up the whole of the cable from the shore to the fault. I picked up altogether five miles, and

relaid it in a course, taking the cable considerably away from this rough ground.

2661. (*Mr. Saward.*) Are you aware whether soundings had been taken with a view to laying the cable previously to the contractor having laid it?—Never; no attention whatever had been paid to the bottom of the sea.

2662. Do you attribute your troubles in some measure to that?—I attribute the troubles that we have had entirely to the neglect either of the contractor or of the company in not surveying the ground properly, and in not selecting a proper route where the cable would not be liable to the accidents which have occurred. Had the cable been laid from the Isle of Wight to Alderney, in place of being laid from Portland to Alderney, I think there is not the slightest doubt but that we should be working at the present moment without having incurred a single break.

2663. (*Chairman.*) What bottom would you select upon which to lay a cable?—Either sand or chalk, a soft bottom.

2664. Is a chalk bottom sufficiently soft for the purpose?—It is generally soft. In sounding on a chalk bottom the lead always comes down upon the bottom dead, as if it made a slight way in. In fact, you will find that the lead or the anchor comes up from a chalky bottom covered with a sort of white mud, very like the ooze which has been described in the Atlantic. It is a very remarkable thing, that wherever cables have been laid in a nice soft sand or in a soft chalk bottom, there they are as sound now as when they were first submerged; but wherever they have been laid upon a rough, rugged, rocky bottom, or a hard bottom that is swept over by tides, there they invariably show signs of decay.

2665. Do you consider that the first accident which you mentioned was due to the action of the tide?—Entirely.

2666. What is the velocity of the tide?—At this place, which is called Portland Race, there is a velocity varying from six to seven knots an hour; the tide is very severe.

2667. Does not that velocity of the tide extend nearly all the way across the channel from England to France?—No, I think not more than ten miles. In the Race itself, there is a current of about seven knots an hour, and further out five knots; in the centre of the channel I do not think the velocity is more than four knots an hour.

2668. What is the nature of the bottom at the place where you have now laid the cable?—It is much the same, still a rocky bottom.

2669. Why do you imagine that it will not be seriously affected by the tide? Is the velocity of the tide slower?—I have no reason to believe that the action of the tide will be reduced, but I was led to believe, from incorrect information and erroneous charts, that I should have avoided rocks, and obtained gravel and shingle, in taking the course I did. This was the first break that we experienced. In the month of September last we had another break in the very same place, only further out. We had, very curiously, two breaks at the same time—one 6¾ miles out, and the other 12 miles out. These breaks were precisely similar, the wire showing signs of this attrition in both cases; and more than that, in some half a dozen different places we found marks of this action commencing. I have here specimens which will show you the action from the first commencement to the subsequent break. In this specimen you will see that it has just commenced; and in this it is something worse.

2670. (*Mr. Saward.*) You would not call this a very good wire, would you?—No; it does not appear to be of a first-rate quality.

2671. (*Chairman.*) Would not wire of any quality or strength be worn away by such action of the sea as this?—Yes. There is a still worse specimen; but in addition to this chafing action that has been going on, there are great signs of a kind of chemical or corrosive action. You will see it in different places; it is a species of honeycombing.

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2672. (*Mr. Saward.*) That is to say, a corrosive action upon the external iron?—Yes.

2673. When you allude to the great danger of cables being laid on a rocky bottom, have you the same apprehension of cables laid in deep water if there chanced to be rocks, say in 2,000 or 3,000 fathoms?—No; where the cable was laid in a depth like that the tides would not exert themselves upon it at all, and you would have none of these effects; but we do not at present know how far the tide extends. I was told in Alderney that the divers are able to carry on their diving operations, in a severe tide running about six knots an hour, without any inconvenience.

2674. (*Chairman.*) What depth is it there?—About 17 fathoms.

2675. Are you aware that at Spithead, when the tide was running strong, it was felt by the divers at 13 fathoms?—Yes. In Alderney and Portland they say that they do not feel it; but then there may be a cause. Alderney lies in a sort of bay, the current sweeps outside of it, and the great force of the tide, perhaps, would not fall inside this bay—it would expend itself outside; so that we should only have just the surface of the tide close in shore, where they carry on their diving operations. Our cable, however, shows undoubted signs that the tide acts upon it at 30 and 35 fathoms.

2676. Do you attribute the corrosive action which you have mentioned to the rust which the water would form upon the surface of the iron being washed off repeatedly by the action of the currents, or to any peculiar chemical causes?—There are three kinds of corrosion. There is the pure corrosion, simply produced by the action of running water over it, first encrusting it with rust, and then washing it off again. There is another corrosion, which I have noticed more particularly in the short cable which crosses over to the Isle of Wight, where we pick up a peculiar stone which has some iron in it.

2677. Is not that cement stone?—Yes; wherever the cable has rested upon this stone it has been completely eaten through; the corrosive action of the stone making deep holes in the outside iron wire, and rotting it away entirely.

2678. Simply upon the part where the iron rests?—Simply upon the part where the cable rests.

2679. Do you imagine that that arises from any affinity of the stone for the iron?—I think it is owing to some galvanic action that is going on between the iron in the ironstone and the iron in the cable. The one is more positive than the other; if you put them in close contact in a solution like the sea a galvanic action goes on, and the cable being the most negative would perhaps be destroyed first. Then there is another corrosion that we find; I will not speak of it with certainty, but it appears to be owing entirely to the zoophytes and vegetation that become attached to the cable. This was particularly the case in several spots off the coast of Portland. There the zoophytes and vegetation were abundant; an enormous quantity of mussels also adhered to the cable, and wherever it rested on such places it was very much corroded.

2680. (*Mr. Saward.*) Do not you think that the effect produced by the zoophytes would probably be due to the carbonate of lime, which is present in that class of animals in excess?—We do not know what chemical matter there is in these little animals. It is an extraordinary thing to see a cable come up covered with those little fellows. Their roots may secrete an acid of some kind which acts upon the cable.

2681. That does not appear to have the effect of seaming the iron like ordinary rust?—No; it is very curious to notice how the fibres are brought out in cases of ordinary rust. I do not think I have a specimen to show the different effect produced upon the different qualities of iron; but you may take a piece six feet long and you will find one strand all the way quite perfect, while all the rest are greatly cor-

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roded. The chief matter of interest is the corrosion arising from the presence of these zoophytes.

2682. (*Chairman.*) How long has the Channel Islands cable been down ?—Seventeen months. It was relaid in April of this year, off Portland. In picking up that first break, we brought up, attached to the cable, a stone weighing about 20 pounds, in which the cable had worked its way, so that there is a perfect representation of the cable, showing every wire distinctly in a groove about an inch and a half deep.

2683. (*Mr. Saward.*) Is that Portland stone ?—Yes.

2684. (*Chairman.*) Had the wire sunk into the stone ?—Yes; it looks as if the stone had been a soft substance, into which the wire had been pressed and had since hardened.

2685. (*Mr. Saward.*) In fact it looks like a calcareous formation around it ?—Yes; but it is a question whether the cable has embedded itself in the stone by mechanical, or whether it has eaten itself in by chemical, action.

2686. (*Chairman.*) Or whether the stone has been formed round the cable ?—There is no question about that, because the stone has marks of the action of insects and the sea upon it; it looks as if it had been down for hundreds of years. The stone has no appearance of being newly found. The late Mr. Stephenson was of opinion that the cable had gradually, by a slight screwing motion, worked itself into the stone, but that is almost out of the question. It is impossible to conceive the communication of any screw-like action to a cable lying taut on the bottom; I am more inclined to favour the chemical action. I have always attributed it to some chemical action that has been going on between the stone and the iron. It was taken up in 25 fathoms water. There was no evidence of the cable itself being injured at this spot.

2687. Is there any other cause of corrosion that you have observed ?—No; I think that I have mentioned the three different kinds, pure corrosion, chemical corrosion from the effect of metallic stones, and corrosion that seems due to the action of animal life and the formation of vegetation upon the cable. I have never observed any others.

2688. Will you mention the other causes of accident that the cable has experienced ?—The first accident arose from the direct action of the waves beating the cable upon the rocks; the second took place from the vibratory action of the current abrading the cable upon a sharp rock. But here is a very curious specimen of breakage of a stout shore end.

2689. In this case the copper wire has been entirely abraded away ?—The copper wire and half the gutta-percha.

2690. As soon as the copper wire was abraded, I suppose the communication was stopped and you took up the cable ?—Of course we were stopped at once.

2691. If that attrition had gone on would not it have cut the cable entirely ?—Yes; as soon as it had cut the copper wire it was quite sufficient for us, we could do no more work through it. It is a curious thing in this case, that although the ends of the copper wire were separated by a distance of nearly three inches, we were able to get currents through that space, but so slight that we could not depend upon them to work with. If we had used Professor Thompson's instrument we might have read them distinctly for a short time, only it would have got worse. In that instance, however, we got visible currents, which in the Atlantic cable would have been called almost powerful currents.

2692. When there was no copper wire ?—Yes.

2693. (*Mr. Saward.*) By working through the oxide in the water ?—Yes; you see the oxide exteuds along the abrasion, where there is a sort of groove, in which the wire rested, which was covered with a green-looking oxide.

2694. (*Chairman.*) Still it was perfectly open to the water ?—Completely.

2695. (*Mr. Saward.*) Have you any further accidents to describe ?—Yes; there was an accident from a kink which occurred in the Isle of Wight cable. (*Producing two specimens of gutta-percha core.*)

2696. (*Chairman.*) Was that kink in the original laying ?—Yes; it had not gone bad, but you see that there are signs of its quickly going.

2697. The action of the kink being to facilitate the wearing away of the cable by attrition ?—Yes; leaving a small portion of the gutta-percha exposed that rested upon the bottom; the vibration given to it by the tide would soon wear its way through.

2698. If there had been no attrition the kink itself would not have injured the core ?—Not at all.

2699. In this case had the outer covering of the core been worn away in the same manner ?—Yes.

2700. Is not that cable made of india-rubber ?—No, not this particular portion, which is a length of about a mile and three-quarters. There was one very curious accident that I had with this cable. It is a very stout cable (similar to No. 1, Gibraltar). There were two kinks thrown into it when it was laid out. A ship happened to drop its anchor over the cable and got hold of it, placing a very heavy strain upon it, breaking the copper wire inside the gutta-percha in both the kinks. The cable was brought up to the bows of the ship and slipped, but communication was interrupted, and when we picked it up to examine and repair it, in both of those kinks the solid copper wire had gone; had that copper wire been a strand we should have been all right.

2701. Because it would have stretched ?—Yes; that is the only instance that I have had where I have seen the benefit of a strand of wires, it being the only case of broken wires inside the cable.

2702. (*Mr. Saward.*) Can you describe any peculiarities in the manner in which the various faults that have come under your notice in these different accidents have come on; is there anything peculiar or interesting in the manner in which they have come on ?—No; they have all of them, with the exception of the heavy fault, the second that occurred to the Jersey shore end, been sudden breaks, which have shown themselves in an instant; we have gone on till the moment communication ceased, and we have seen no signs beforehand.

2703. Was that, to use a medical phrase, intermittent, or was it a constant gradual losing of power ?—It was at first intermittent; it then became a steady decay, gradually losing itself; from the first sign to the last break-down, I suppose, must have been a fortnight. At first I attributed that fault to lightning, and I had for about 12 hours a very strong positive current sent through the wire, which we found covered the wire with a sort of oxide, that was an insulator, and it enabled us for a few minutes to work very nicely, but the opposite current in a very short time worked away that effect, till at last the fault gradually got so bad that it stopped the communication altogether. This was when the fault first showed itself, and evidently before the copper wire was broken.

2704. (*Chairman.*) Were there any other accidents to the Channel Islands' cable ?—There were four in all. I have named the two accidents to the Jersey shore end, the two breaks from attrition off Portland, and the two faults in the Isle of Wight. I have had several breaks in that cable from simple ruptures owing to an anchor's dragging; but those are all the breaks of any interest. The corrosion arising from the effect of ironstone has been more evident in the Solent than in the Jersey cable; the Jersey cable, perhaps, has not been down long enough to observe any effects of that kind, but in the Solent it has been very remarkable.

2705. Have you ever observed any injurious effects produced by lightning on submarine cables ?—Yes; last summer, about June, there was a very severe thunder storm in Jersey. A flash of lightning seems to have struck the wire on the land, at the top of a hill; a portion of it rushed into our office, totally destroying the instruments there; another portion found

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its vent in two places, producing small punctures in the wire, and the remainder of it disappeared in the cable, producing a fault in that cable. The faults in the land wires were very soon found out; they were simple punctures, giving what we call a bad earth, but not of any serious consequence; the fault in the cable remains there still; it gives a resistance of about 160 miles, and from that day to this it has been no better and no worse; it remains about the same. The peculiarity of this accident is that the land wires in connexion with the cable are underground, about 20 inches to two feet deep.

2706. At what distance from the shore is the fault supposed to be?—About 2½ miles from the Guernsey shore, as nearly as we can test.

2707. It was in Jersey that the effect arose?—Yes. It is very curious that the lightning, before it produced this fault in the cable, seems to have run along the cable a distance of about 16 miles; it there found out a little weak spot, and made its way out.

2708. Do you suppose that there was an eccentric part where the wire was nearer to the water than at any other place?—Most probably, or it may be an air-hole in the percha; but it is not a serious fault in such a short circuit; that is the only instance I have experienced of a cable being seriously damaged by lightning. There is an effect in one of the specimens that I have produced before the Committee which I can only attribute to lightning; at one end of the wire it appears to be a good deal blown out.

2709. Do you imagine that that was produced by a thunder-storm?—There was a thunder-storm between the break and the time it was repaired; it has such a peculiar effect that I attribute it to lightning.

2710. If lightning passed along the wire, would it not escape at the part where it had the nearest access to water?—Yes.

2711. (*Mr. Saward.*) This specimen looks like the inside of a fuse after it it is discharged?—Yes; just as if it had been fused.

2712. (*Chairman.*) Do you adopt any special precautions, where submarine cables approach the land, to prevent injury from lightning?—I have not hitherto done so; but I have ordered lightning conductors for each shore end, and intend fixing them.

2715. What class of lightning conductor?—A lightning conductor schemed by Mr. Latimer Clark and myself; there is a plate of metal in contact with the earth, and another similar plate above, fitted with numerous fine points, having a film of silk between, one plate being in connexion with the wire and the other in connexion with the earth, but separated from each other by the silk and a thin stratum of air. When the lightning strikes the wire it will rush to these small points, and fly across close to the plate in contact with the earth. There are several kinds of lightning conductors schemed, but I have not seen anything better than that; the only thing is to bring the earth into close connexion with the conductor without being in actual contact; we do not adopt any precautions of that sort in our offices.

2714. You do not mind the coils being fused?—No; we have always spare instruments, and we find more accidents and derangements arise from the use of lightning conductors, than benefit.

2715. You would only adopt lightning conductors in submarine cables, but in those cases you would invariably use them?—Yes.

2716. Did the lightning appear to enter at the underground wires?—Yes.

2717. Not forming a connexion with the ground wire?—No; it appears to have entered the underground wire itself.

2718. Does the underground wire run to the office the whole distance?—Yes, there is no wire above ground between the office and the sea.

2719. How can you account for that?—The hill upon which the lightning seems to have struck is a granite hill; the wire goes under the road to the top, and the lightning seems to have struck the top of this hill. There is a brook running down one side of the hill, and the lightning seems to have struck somewhere near that spot; at that spot it seems to have gone into the wire; the mark of its going into the wire is at that very place. It is a certain peculiar appearance that we have often noticed in accidents to underground wires from lightning; they have a peculiar look, as if they had been burst outward; in this case it seemed to take an opposite direction.

2720. (*Mr. Saward.*) Is not that the appearance it generally puts on (*handing a specimen of core injured by lightning to the witness*)?—Yes; it is very similar to that. This shows as if it had come out of the wire; but this, on the contrary, was like a crater, indented in just the opposite way.

2721. Did you cut the gutta-percha out where it was indented?—Yes; we took a piece out.

2722. Did you find where it was indented that the copper was fused?—Yes; it was burnt, as if a blow-pipe flame had been acting upon it. There was also a spot at the bottom of the hill where it had evidently got out; and there is a fault existing in the cable, which I have not the slightest doubt is something similar to that. It is a fault so trivial in amount that we have paid no attention to it. It does not reduce the strength of the current to any great extent.

2723. (*Chairman.*) Have you experienced any inconveniences from water filling the interstices between the strand wires?—We had a very curious instance of that when we were repairing the Channel Islands cable in October last; the conductor of the Channel Islands cable is 7-strand wire, exactly similar to the Atlantic cable. The man, in making the joint, soldered the wire, put the thick gutta-percha round, and was heating it with a spirit lamp, when it burst in his hands, giving a regular explosion. He was rather startled by this, for he did not know whether it was lightning or current, but he worked at it again, and the same effect again occurred. When I came to examine it, I found that there was a quantity of water in the strand of the cable. I cut a few feet off, but I found there was still more water, and on taking the gutta-percha and squeezing it, we could make large drops of water ooze out. We had to cut 30 fathoms off, before we could get rid of this water.

2724. To what do you attribute the presence of the water?—The water had entered the end of the wire; it being stranded wire, there were small interstices in it, and either by capillary attraction, or by the pressure of the head of water (it was in 30 fathoms), it had gradually forced its way 30 fathoms along the wire.

2725. How do you imagine that it entered the interstices?—It was in the bare ends of the broken cable, which bare ends had been exposed in the water for several days.

2726. Do you attribute the presence of the water entirely to the fact of the wire having been damaged in the first instance?—Of course, to its having been exposed in the first instance.

2727. If the water had been left in the wire, do you anticipate that any bad results would have followed?—I think that the sea-water would have acted chemically upon the copper wire, and, perhaps, in the course of time, have destroyed it.

2728. Do you think it is objectionable that stranded wires should be used without some means being taken to fill up those interstices?—Decidedly; those interstices should be filled up.

2729. Do you imagine that if a small fault occurred in the submarine cable the water might penetrate through that fault, and cause great injury to the cable by the destruction of the strand wire; whilst, if the fault alone remained, the cable might, perhaps, continue for a long time to be worked through?—I think it is impossible to say what would be the effect. Take a deep-sea cable, say at a thousand fathoms, if there is a slight puncture in the gutta-percha, and those interstices in the strands are not filled up, it is impossible to say what effect the pressure of the water would

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have upon it; it might burst the cable up. I do not know what it would do, but it would certainly produce a very serious fault.

2730. The pressure would be equal inside and outside, would not it?—I scarcely can say; the six strands outside form a tube round the interior wire, and there are those six interstices filled with air. I do not know what the pressure would be inside; it could not be the same inside as out; it is rather a difficult question to answer.

2731. It depends upon the compression of the gutta-percha on the outer covering?—Scarcely; because no compression would make the gutta-percha fill those interstices.

2732. The gutta-percha would be pressed close into the strand?—Not into. Draw a section of the stranded wire, and you will find that, unless those strands are very loosely laid, no gutta-percha could enter the interstices by compression. Whatever is used to fill up these tubes, as they may be called, must be forcibly injected in by pressure. I should anticipate no danger if the wires, first of all, before being stranded together, were coated with some cement, something like Hughes's or Chatterton's compound; again, after the wire had been stranded, it should be put through a die, one made into a solid wire upon the patent taken out by Clark, Braithwaite, and Preece. At present there is considerable danger. Perhaps you would allow me to observe, in speaking of corrosion of cables, that I am decidedly of opinion that no cable should ever be submerged upon a rocky bottom, or on a bottom likely to be swept by tides, without having some exterior shield to protect the iron from this corrosive action. We are going to lay another cable, most probably, to the Channel Islands; and if we do so, I shall most certainly have it surrounded by Mr. Latimer Clark's asphalting process, with which I have no doubt the Committee is acquainted. I do not think any cable should be submerged where this corrosion is likely to take place, without having that, or some similar protection.

2733. Have you no fear of accident in the application of that process?—Not the slightest.

2734. Was not the Isle of Man telegraph, which was coated with it, injured?—A portion of it; that was the first experiment; you must not judge of the merits of an invention by the result of a first experiment; it was certainly injured, but that was simply through careless manipulation.

2735. (*Mr. Saward.*) Ought not it to be applied at as small a heat as it can be?—Yes. I am speaking of the process when complete; the manipulation of that process is simply a mechanical question, which any man, with a little engineering talent, can easily master. I am sure it might be done much better than it was in the Isle of Man cable; but you must recollect that that was the first time it was tried. I did not at all approve of the plan then adopted; but I can see no difficulty in applying it.

2736. (*Chairman.*) You do not believe that any accident need occur to the gutta-percha inside the wires by the asphalting process?—No. Even supposing that in this case it did produce the injury, I think there would be no difficulty in applying the process without using heat; they might use a hemp strand, and coat that well first with creosote or tar, or something of that sort, and they might even apply some cold compound to it which would speedily solidify in water.

2737. It is a question of the compound to be used?—Yes; I do not wish to say a word about that at present. All I wish to say is, that cables should be coated by some compound, particularly the Gibraltar cable. If it landed at Falmouth, it should be certainly coated with something like that for a distance of 20 or 30 miles, or you are safe to have a repetition of all these accidents that we have had in the Channel Islands cable; when you get the wires away from the effects of currents and tides, then you need fear nothing, and if the iron wire rusts away it is of no consequence.

2738. Do not you fear that the asphalte compound would be liable to be worn off in the same way that the iron has been worn away?—Undoubtedly, it would be liable to attrition; the asphalte will not prevent attrition, but it will prevent corrosion arising from the effect of running water, and it will prevent the corrosion arising from the chemical action of the ironstone or the effect of vegetation.

2739. (*Mr. Saward.*) With regard to the attrition, if I gathered correctly what you said formerly, you would take care to have proper soundings taken, so as to get out of those influences as far as possible?—Undoubtedly; and moreover, supposing that you had a rock-bound coast that you cannot avoid, I should take care to place a very much stouter cable there than any cable that is down at present.

2740. Would attaching weights to a cable in that position be of any service?—No, I think not; the cable should be made sufficiently heavy of itself; for instance, take this Gibraltar cable, the cable for the shore ends. Such a cable should be composed first of all of this as the core (*not deep-sea Gibraltar*), let it be served with hemp, and let that be served outside with 12 or 13 strands of No. 1 wire, and after all that, let it again be served a third time, where it is likely to be acted upon by rocks, with a stout wire; that would form a stout cable; it would not resist for ever the action of the rocks, but it would resist the action of the rocks for a very long time.

2741. Its own weight would have a tendency to keep it quiescent?—Yes, it should be made as heavy as possible, and as strong as possible.

2742. (*Chairman.*) Have you experienced much difficulty in picking up cables?—No, only in cases where the line of the cable has been crossed by a sand or shingle bank; there it has been buried, and cannot be lifted up. We are compelled to break the cable, and go to the other side of the bank and pick it up again, leaving the piece in the bank.

2743. Have many such cases occurred in the Channel Islands telegraph?—Only one, there is a bank about four or five miles off Portland, called the Shambles; there during this last break-down I was obliged to leave about 400 yards of cable, as we could not get it up; and in repairing the North Sea cables, there is another bank where they have at different times left some two or three miles of cable.

2744. Does the weight of the cable interpose a serious obstacle in the way of picking it up generally?—Not in my experience; my experience has never been in greater depths than 60 fathoms, and in 60 fathoms there is no difficulty whatever in hauling the cable up.

2745. In 60 fathoms could you find a cable though you did not know its exact position?—Yes, cables are repairable in 100 fathoms.

2746. Would there be difficulty in picking a cable up in 150 fathoms?—There would be no difficulty in picking it up, but great difficulty in finding the end.

2747. Assuming that you did not know the exact position of the cable, in what depth would you consider it secure from being picked up by an enemy?—It would be easily picked up in depths under 100 fathoms; from 100 to 150 fathoms, it would be picked up with great difficulty; and from 150 to 200 fathoms, and beyond that, it would be almost an impossibility.

2748. Could any additional weight be given to the cable in shallow water to render the picking up a matter of great difficulty?—The heavier the cable, of course the greater the difficulty of picking up; the cable might be so heavy, that there would be very great difficulty in picking it up; with all the cables that are now down, I should not anticipate the least difficulty in picking them up in 50 or 60 fathoms. The strongest cable is the Zandvoort cable, and the Calais is about the same; there would be no difficulty in picking them up; the only thing is to have stout machinery.

2749. After 200 fathoms or 250 fathoms you think it would be impossible to pick the cable up?—Almost

impossible; that is, I mean of course, in grappling for the end of a cable whose position you knew.

2750. I am assuming that the exact position is not known?—Supposing the cable crossed this room, and we work from this end and creep across, we must pass it, but at 200 fathoms there would be great difficulty in picking it up; under 100 fathoms less difficulty. When once you haul the end on board, I should anticipate no difficulty in picking up any cable in any depth, provided the strength of that cable were such as to bear its own weight from the stern of the ship to the bottom of the sea, plus the friction of the water opposed to the passage of the cable in hauling in. It is then simply a question of suitable machinery.

2751. I believe you have had considerable experience in the use of india-rubber as an insulator?—I have.

2752. Have you a line laid between the Isle of Wight and the main land, which is covered in with india-rubber?—There is a telegraph extending from Southampton to Osborne, and the cable connecting the Isle of Wight with the main land crosses the Solent at Hurst Castle. From a place called Keyhaven to Hurst Castle, a distance of about a mile and three-quarters, it crosses some very thick mud; this mud is always covered from half-flood to half-ebb tide. It is an india-rubber covered wire; and there is also another piece of similar cable, about a mile in length, crossing the Yarmouth river in the Isle of Wight. There is a plan which shows the two places (*handing in a plan*).

2753. Is the india-rubber cable which you have mentioned, covered with hemp and iron?—It is what is generally called West's cable.

2754. Having a covering of plaited iron?—Yes. That cable was constructed by Mr. West, for the Irish Submarine Company.

2755. Can you describe what is the covering of the india-rubber; is it bitumen?—No, it is a copper wire covered with india-rubber, which is coated outside with spun-yarn, and then outside of all by iron wire. This is a specimen (*producing the same*). It is a piece of cable that was made by Mr. West for the Irish Submarine Telegraph Company in the early part of 1852; it remained coiled up in a yard during the whole of that summer and autumn, and was eventually submerged in its present position in the early part of 1853, where it has remained undisturbed to the present day. I have a specimen here, which at Mr. Clark's request I cut out of the cable in a certain place where it had been always under water.

2756. (*Mr. Saward.*) Is it partially exposed at half tide?—No, it has been under water the whole time.

2757. (*Chairman.*) There are four copper wires in this cable?—Yes.

2758. Do you know how the india-rubber was put on; was it by dies or by winding it round?—I believe by winding it round.

2759. Has the cable worked perfectly?—It has worked well. I have marked the position in which that part which I cut out was, in the plan that I gave in; it was in a small creek, and it always has been under water.

2760. The cable itself was imbedded in the mud?—It was well imbedded in mud some three feet deep; it may be said to have been always in a good equally moist bed. It has only failed once since its first submersion, and that occurred at the spot marked B. on the Yarmouth river, a place where at neap tides the mud becomes dry, and the india-rubber in consequence became pulpy and decayed. A length of about two feet of all four wires became faulty. The piece was replaced by prepared gutta-percha, and it has since remained good.

2761. How is its insulation as compared with that of gutta-percha?—The length is so short, and moreover we have been so very much troubled with the joints, particularly the joints between the gutta-percha and the india-rubber, that I have never been able to make any fair test upon its comparative insulation. For instance, in the part where that fault was cut out, it was replaced by gutta-percha for a length of about six feet, so that in testing the cable I should have to make allowance for the gutta-percha.

2762. On the whole, are you satisfied with india-rubber as a covering for the conductor, or do you prefer gutta-percha?—Judging from the material hitherto supplied, I am not. In certain circumstances I find that the substance of the india-rubber varies considerably. In some pieces of cable we have a nice hard, firm material, but in other pieces the india-rubber has become pulpy and gummy, like pitch. You saw a specimen just now where the india-rubber has been really good; it is as good at this day as when it was first laid down. But where it has become pulpy, it has given us a great deal of trouble. I have several pieces of india-rubber crossing the railway and passing through tunnels at different places, but I have always been particularly careful in selecting good stuff. Where you have bad stuff, you cannot depend upon it. Gutta-percha, on the contrary, never gives trouble from the impurities of the gum.

2763. Can you tell good india-rubber in the process of manufacture?—I have not had sufficient experience to say; I can tell it after exposure.

2764. How do you tell good india-rubber by exposure?—We expose the india-rubber to the atmosphere for some months before we lay it down. When we find that it is firm after exposure we take it as being good.

2765. Do you refuse to buy india-rubber upon any other terms than that of its standing the test of exposure?—No; we have not generally purchased india-rubber. The wire I am speaking of was a portion of a quantity of cable constructed for the Irish Submarine Company, taken off Mr. West's hands by our company, and of which we have made the best use that we could. Some portions have been constructed of bad india-rubber; these we have discarded. Some portions were good, and they have been used; I have used up a considerable quantity of it. There is one tunnel on the Portsmouth line where it has been since 1854 exposed to the air through the tunnel, and there it is as good as when it was first laid down. It was very well coated with pitch and tar.

2766. (*Mr. Saward.*) As regards the condition of that short length of india-rubber cable, I presume that in a short length like that, practically, you could work well through it with a good deal of loss upon it?—Yes.

2767. Therefore, you would not consider that conclusive in favour of india-rubber without a much longer length being laid down?—No; I consider that as an insulating medium india-rubber when pure is in every respect as good as gutta-percha. As an insulator, I think Professor Wheatstone himself could say that it is very much better; its durability is quite equal to that of gutta-percha, and its greatest superiority over gutta-percha is, that it will bear a greater temperature. So that for submarine cables in a great depth of water, in selecting india-rubber or gutta-percha as an insulating medium, I should be guided more by price than by anything else; but where you have shore ends, or where you have a cable that is likely to be exposed to a high temperature—any temperature say, over 80°—I should prefer india-rubber to gutta-percha. The only thing is, that the greatest care must be exercised in selecting proper stuff, because the whole success of the cable depends upon whether good india-rubber has been selected. It has behaved itself quite equal to gutta-percha, both in the open air and in the sea.

2768. Have you considered whether copper affects india-rubber?—No; I have never observed any action of the copper upon good india-rubber.

2769. I have some specimens in my office, which have been in a drawer for *some four months*; they have never been brought out; they were supplied to me by Messrs. Silver, but the process of decay is now commencing?—I can say this, that I have noticed that my men in repairing these cables will say, "Oh,

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"this is a good piece." I can see at once that they have arrived at that conclusion by the appearance of the copper wire. Where the copper wire is bright, and looks new, the india-rubber is good; but occasionally you will come across pieces where the copper wire is darkened and covered with a sort of oxide of some kind, and the india-rubber is sticky and glutinous, such pieces have been put aside as being bad. I have myself noticed, particularly in this cable submerged in the Yarmouth river, that on cutting off the india-rubber we found the wire perfectly bright, and no signs of any action either upon the wire or india-rubber.

2770. (*Chairman.*) Your opinion is, that india-rubber requires the greatest care in selection before it can be used with safety, and that with gutta-percha you need not exercise such minute care?—Exactly so; we have never observed any decay in gutta-percha arising from impure materials, we have in india-rubber, and, therefore, I conclude that in using india-rubber the greatest care should be exercised in obtaining the best material. This piece has been submerged in water for eight or nine years, and the ends have been exposed to the air for two or three months, and yet the india-rubber is perfectly sound, whereas in another specimen it is quite gummy, like tar or pitch.

2771. I believe you have devoted some attention to the question of the retardation of currents of electricity in submarine wires?—Yes; I have paid great attention to it since the question first arose.

2772. Can you give the Committee an account of any experiments that you have made, or results that you have obtained?—I have been engaged with Mr. Latimer Clark ever since the peculiar effects of retardation were first observed upon underground wires in 1853. We had some very careful experiments at Lothbury, at which Professor Faraday was present, and Mr. Clark and myself have frequently experimented at the Strand station and at our stores. The only independent series of experiments that I made myself, which are interesting, were made at the Strand on the 21st of May 1854. The object of the experiment was to see the difference of retardation of signals with different instruments. I had a series of ordinary galvanometers, a series of relays, and a Bain's chemically marking instrument; the length of the circuit was 1,100 miles; upon using the relays we found that in a distance of 1,100 miles, before any signal could be registered at the distant end three seconds elapsed, but with a simple galvanometer it required only eight-tenths of a second. It was very curious that with one instrument I could not get the first appearance of the current until three seconds after I had made the contact, while with the other instrument the effect was obtained in eight-tenths of a second. There is a vast difference between the two.

2773. At what distance was the other end of the wire?—They were both in the same room. They were six underground wires, extending to Manchester, in pipes underneath the railway; and in their effect, and everything else, they may be taken as equal to 1,100 miles of direct line, because we tried very careful experiments to see whether there was any contiguous effect; that is, whether one wire had any effect upon the other, and we could not trace anything at all. They were separate gutta-percha covered wires.

2774. Neither end went to earth actually?—Neither end went to earth; both ends came into the same office. We sent the current at one end, which went backwards and forwards, and came out at the other, and thus we were able to observe the effects.

2775. To what do you attribute the difference of speed?—It arose thus: the current, on its appearance at the distant end of the wire, assumes the formation of a wave; the first appearance of the current cannot be noted; in fact, I am of opinion that the arrival of the current at the distant end is instantaneous.

2776. Then it depends upon the delicacy of the instrument whether you can register it or not?—Yes. It first arrives instantaneously. It comes as a wave, gradually swelling up to its apex, and when the contact ceases it subsides again as a wave. If you represent this wave by a curve, you can get a fair indication of the nature of the current. Supposing you have a very delicate instrument, perhaps at a distance of 2,000 miles you get your first indication at half a second. If you adjust the instrument a little stronger, you get the same appearance after a lapse of a second, and so on till you get up to three or four seconds. So that all the experiments made upon the retardation of currents, particularly those experiments made by Mr. Whitehouse, and some others, are of very little avail in arriving at any conclusion upon the speed of currents through submarine cables, because we do not know what adjustment of instrument was used.

2777. (*Mr. Saward.*) To attain that speed you would concentrate your attention upon the delicacy of the relay?—Yes, to the delicacy of the relay, certainly. I consider that the greatest speed in working a submarine wire of a given length will first of all depend upon the size of the conductor, presuming the thickness of the insulating medium to remain about the same as now usually adopted for commercial and economical reasons. In the next place it will depend upon the nature of the current that is used, because there is a given ratio of quantity and intensity in the current, at which you can get the greatest speed through the cable; for instance, with heavy battery power you perhaps will get one signal through in one second, but if you use electro-magnetic power you may increase that speed perhaps to three or four signals a second. With a very large conductor, with proper power, and with very delicate instruments, you will get the maximum rate of signalling through a cable.

2778. Have you compared the result of working respectively with battery power and electro-magnetic force direct?—No; my experiments have all been made with battery power, and for the effects produced by electro-magnetic power I have depended solely upon the results of the experiments of others I have tried to see whether increasing the intensity of the battery power has any influence upon the rapidity of signalling, and I cannot see that it has.

2779. (*Professor Wheatstone.*) In the experiments you have made on retardation, you said that the wire returned to the station; had you no earth communication at the station where you were experimenting?—Yes, we used the earth; it was not a metallic circuit.

2780. (*Mr. Saward.*) Is not the longest circuit of the Channel Islands telegraph about 50 or 60 miles?—No; we work from Southampton to Jersey, which is a distance of 180 miles.

2781. Is your speed practically unlimited upon that length?—Practically quite unlimited; it depends upon the rate at which the clerk works.

2782. Are the instruments that you use ordinary Morse instruments, or prepared for the purpose?—The instruments are ordinary Morse instruments.

2783. With the ordinary kind of relay?—With the ordinary kind of relay; they are instruments constructed by Siemens and Halske of Berlin. I may mention that when this circuit was connected up to the Channel Islands, each island, that is Alderney and Guernsey, were connected up as relay stations; so that when Southampton spoke to Jersey, first of all it spoke to Alderney, Alderney brought in a relay, working Guernsey, and Guernsey brought in a relay, working Jersey. We worked with single currents, that is, a positive current made upon each contact, but I found that we worked so very slow, not at a rate of more than ten words a minute, that I very speedily knocked away the relay stations, and worked through intermediate, as we term it; Southampton, Alderney, Guernsey, and Jersey being all upon the same circuit.

2784. Did you use alternate currents in working?—Yes, subsequently I found, after removing the relays, that we did not attain the speed I should like to

have seen, and I adopted the system of using alternate currents. As soon as that was done we worked at an unlimited speed.

2785. Does any portion of the cable cross any tideway at right angles ?—Very strong sideways.

2786. Do you experience any difficulties from earth currents ?—No ; I have endeavoured to trace the existence of currents produced by running water, which Wollaston wrote about, but I can see nothing of the sort. There are always delicate earth currents in the wires at any period of the day, or at any time of the night, whether land or submarine wires ; but it is only when the aurora borealis is visible that we find any serious effect arising from those earth currents.

2787. Practically they do not embarrass your working ?—Practically it is only when the aurora borealis is visible that we are embarrassed; we have been very seriously embarrassed two or three times this year.

2788. Does not the cable lie nearly south-east and north-west ?—Almost direct north and south between Alderney and Portland, then it runs south-west, and then again about north and south ; but the cable which I have examined more carefully for these currents, has been the Isle of Wight cable. There is a very strong tide running seven or eight knots an hour over the cable for five hours in one direction, and seven hours in another, but there there is no sign of these currents ; if they were of any consequence we should very speedily observe them.

2789. (*Chairman.*) Are the earth currents stronger by day than by night; is there any difference ?—No ; we observe them more at night, because the wires are less occupied at that time ; but I have examined them at all hours of the day ; I cannot trace any particular law by which they are guided. There is a steady current in one direction in the morning, and generally in the other direction in the evening. There is one time of the day, about 12 or 1 o'clock, I think, when generally speaking the wires are perfectly neutral ; but the strongest time of the earth currents is generally early in the morning and about six in the afternoon ; they are generally equal about those times.

2790. Is the earth current always in the same direction in the morning of one day as another ?—No, sometimes it is positive and at other times negative in the morning ; when it is positive in the morning it is negative in the evening, and when it is negative in the morning it is positive in the evening.

2791. Is the turning point generally about the middle of the day ?—Yes.

2792. Does it appear to follow any rule for a month, that for half the month in the morning it is the same one way, and the other half the same the other way ? —I have not observed it sufficiently to answer that question. For this reason, that our wires are generally so occupied that we can get but little time when we want to try it; if we put any delicate instrument at work some station comes in with strong power and knocks our instrument all to pieces. I have never made any systematic observation upon these currents except this, that whenever the currents have been observed so strong as to interfere with the working of the circuits I have kept a regular series of observations for the last two or three years, which I have communicated to the Astronomer Royal. We find invariably a connexion between "earth currents," as we call them, and magnetic storms, generally commencing and ending about the same time.

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2793. Has any comparison of the direction of the current and the direction of the sudden deflections of the needle been made ?—I cannot say for certain ; I have always given, when I have sent the observations to the Astronomer Royal, the direction of the current ; whether any similarity has been observed between them I cannot say.

2794. Does the earth current turn at any hour of the night or does it appear to be uniformly in one direction ?—It is generally speaking in one direction in the evening, and in one direction in the morning ; therefore it must turn at some hour in the night, but at what hour I am not prepared to say.

2795. (*Professor Wheatstone.*) Do you measure these earth currents by a delicate galvanometer ?—Yes.

2796. (*Chairman.*) What description of galvanometer ?—A simple horizontal galvanometer, the needle being delicately suspended by unspun silk.

2797. Have you compared the earth currents with a single cell ?—No.

2798. A single cell will send a much stronger current to the remote end than an earth current ?—Yes. We could do it by reducing the strength of a single cell and by inserting resistance, but I have never done so. There are lines, which are called "neutral lines," that never are affected at all by earth currents ; supposing there are earth currents to-day observable upon a wire running north and south, and say that the earth current upon that wire running north and south gives a deflection of 20°, an earth current running north-west and south-east will perhaps give a current of 10°, but when we go to-morrow we shall find it just the reverse, while a line running between them has *perhaps not been affected at all ; we find* that wires coming east and west have rarely earth currents.

2799. Is there any rule as to which wires give the strongest earth currents ? — Generally those wires running north-west to south-east, and *particularly* so when the aurora borealis is visible.

Adjourned to Thursday next, at Two o'clock.

Thursday, 22nd December 1859.

PRESENT :

Captain DOUGLAS GALTON.
Professor WHEATSTONE.
MR. SAWARD.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

FLEEMING JENKIN, Esq., examined.

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2800. (*Chairman.*) You are a civil engineer, I believe ?—I am.

2801. Have you not had considerable experience in both the manufacture and the laying of electric telegraphs ?—I have been in the employment of Messrs. R. S. Newall and Co. for rather more than two years, during which period I have fitted out all their various expeditions for the purpose of laying and lifting submarine cables.

2802. Have you accompanied any of these expeditions ?—I have accompanied several of those expeditions.

2803. You have turned your attention to the insulating properties of gutta-percha at various temperatures, have you not ?—Yes. I made a special series of experiments upon that subject in the month of September of the present year. For that purpose two coils were procured from the Gutta-Percha Works in London ; one was prepared with pure gutta-percha, and the other was prepared with Chatterton's com-

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pound. I wished to ascertain the change in the insulating properties of those two materials at high and low temperatures; and, generally, to observe all their properties and relative value as insulators. The first coil that I made my experiments upon was one knot in length; the diameter of the gutta-percha was 0·3in.; that of the copper 0·06in.; the weight of the copper was 70 lbs, and the weight of the gutta-percha 160 lbs. That coil, together with a coil of similar weight and dimensions, which I shall call No. 2, prepared according to Chatterton's patent, was tested under a pressure of 600 lbs. per square inch in a tank to discover if there was any flaw in the gutta-percha; and the tests, by comparison with the ordinary tests of the gutta-percha covered wire received for the manufacture of cables, showed that those two coils were quite perfect; I then removed the coils into a wooden tub, in which I could place warm or cold water at pleasure; and I made a series of tests upon those two coils, using the connexion for the ordinary insulation test, that is to say, the test of the quantity of electricity which passes between the inner surface and the outer surface of the gutta-percha.

2804. How did you make that test?—One end of the copper conductor was insulated; the other end of the copper conductor of the coil was connected with the battery; the other pole of the battery was connected with earth, and I had the means of reversing the connexions of those poles. A galvanometer was placed between the coil and the pole of the battery, connected with the coil, so that any current which passed between the internal surface of the gutta-percha and the external surface of the gutta-percha which was in water in connexion with earth, was shown by the galvanometer. The galvanometer employed was a sine galvanometer, manufactured by Messrs. Siemens and Halske, of Berlin, of very great delicacy, a quality rendered necessary, by the thickness of the gutta-percha covering the copper wire, and the short length of wire tested. By these means I could measure the loss, as it is generally called, the copper being considered as a sort of reservoir of electricity, and the current which passes to the outside of the gutta-percha being looked upon as electricity escaping from the copper. I could, by means of the instrument used, measure with very great accuracy the relative loss at various temperatures. I took every precaution to ensure that the gutta-percha should be thoroughly heated through in each experiment, by leaving the coils in water at a given temperature for a considerable space of time, about seven or eight hours in most cases. I took observations at the beginning of each experiment, for the purpose of ascertaining whether there was any slight loss from the different connexions, as was occasionally the case, owing to a slight humidity covering the gutta-percha and the different binding-screws; this loss, in most cases, was extremely small, and the corrections that it was necessary to make on that account were easily applied. I also measured very carefully the strength of the battery which was composed of 72 Daniell's cells, in a way which I will afterwards describe. By those means I made a very considerable number of experiments, at different temperatures; and after all the reductions were made, I put the result into the form of curves, which I have brought with me, and tracings of which I will furnish to the committee. These are the two curves that resulted from tests upon the coil of pure gutta-percha made with positive and negative currents, respectively, (*producing a diagram*), (Plates I. and IV. in Mr. Jenkin's paper, Appendix No. 14), at different temperatures. The temperatures are marked on the horizontal base line. The ordinates of the upper of the two curves shown in each figure represent the loss at the corresponding temperature, one minute after the coil was connected with the battery. Thus the rapid rise which takes place in the curve at the higher temperatures shows the great increase of loss, or diminution of resistance in the gutta-percha at those temperatures. The tests were made first with the zinc and secondly with the copper pole of the battery connected with the coils. There was very great symmetry in the observations. The two curves which result from these observations, after the necessary corrections are made, are identical between the temperatures 50° and 75°, and even where they rise above 75° the difference is not great. The curves spoken of are the two upper curves shown in the two diagrams. You will, however, observe that there are two curves in each diagram. To account for that, I must mention a phenomenon which I observed, and which I have never seen noticed anywhere else, namely, the change in the resistance of the gutta-percha due to continued electrification, or continued contact with one pole of a battery. The increase of the resistance of the gutta-percha is very considerable for the first five minutes. It is difficult to ascertain what the increase of resistance is during the first minute, because the observation being made upon a galvanometer, the needle of the galvanometer takes some time to come to rest, and during its oscillations no observations can be made. But seeing that there was a continued and very considerable change, I made, in each case, five different observations; the first, one minute after the battery was connected with the coil; the second, two minutes; the third, three minutes; the fourth, four minutes; and the fifth, five minutes after the connexion of the battery with the coil. I invariably got a very much larger resistance after the fifth minute than after the first minute.

2805. That is to say, less of what you call loss?—Yes, less loss, which indicated increased resistance after five minutes' continued application of the current.

2806. To what do you attribute that?—I have not yet formed any very decided opinion upon that point. I am inclined to believe that it is really owing to a change in the body of the gutta-percha itself. Whether that change is due to the moisture which may be contained in the pores of the gutta-percha, or whether there is some change in the nature of the gutta-percha itself, I cannot decide as yet. I have reason to believe that it does not depend upon the contact of the copper with the gutta-percha, because I have found a very marked increase in the effect whenever the mass of gutta-percha has been increased; and this leads me to believe that it is in the body of the gutta-percha that the change takes place. It is just possible that it may arise from the polarization of the moisture in the molecules of the gutta-percha and not from the resistance of the gutta-percha. I have observed the effect still to be more marked in observations which I will presently describe.

2807. Will not the reversal of the current immediately destroy the whole of that effect?—Yes; very nearly, or with pure gutta-percha exactly, the same result is given by the reverse current as was obtained from the original current. You will now perceive the meaning of the double curve. The observations after the first and after the fifth minute give different curves. The lower line on the diagram bounds the lowest observation, or the observation made after five minutes, and the upper line bounds the highest observation, or the observation made after one minute.

2808. If you reversed the current and immediately afterwards sent the current in the original direction, would you get only the effect of the observation of the first minute?—If the original current had been for some time passing through the gutta-percha, rapidly reversing and returning to the original current would not reduce the resistance quite so much as if the reverse current had been longer applied, but when rapid reversals are made, none of which continue for any length of time, the resistance is that due to the first minute's connexion nearly, so that the change observed is of no permanent benefit to the gutta-percha.

2809. Do you leave the current for any time positive?—For twenty minutes or so.

2810. You found that there was no difference produced, after reversing the current, by again sending a current in the original direction?—No; it invariably returned to the first deflection. You will see

five little crosses indicating five observations at each temperature.

2811. You think that no injury could have occurred to the gutta-percha?—No injury occurred to the gutta-percha. I have never experimented upon any coil of gutta-percha in which I have not found a corresponding effect since I first noticed the change of resistance. The second coil, prepared according to Chatterton's patent, gives results of a very different nature in some respects.

2812. Was that coil made of alternate strata of Chatterton's compound and gutta-percha?—Yes; there was first a stratum of Chatterton's compound, then a stratum of gutta-percha; then a stratum of Chatterton's compound, and then a final stratum of gutta-percha. The curves in this case are very different; they are nearly straight lines. (Figures 1 and 2, Plate II. in Mr. Jenkin's paper. Appendix No. 14.) There is no longer that very rapid rise at the high temperatures, and the distance between the two lines in each diagram is considerably greater. There is a want of symmetry between the tests with the positive and negative pole of the battery. The tests with the positive pole of the battery are lower than with the negative pole of the battery, showing increased resistance. The tests with the positive pole of the battery are extremely irregular; the loss does not seem to increase at all regularly towards the high temperatures; they are so irregular that I have not attempted to include them between two curves. With the negative pole the tests are very regular, and can be included between two straight lines up to 80°, beyond that temperature I have not tested.

2813. With the positive pole the tests did not follow a uniform law?—Not a uniform law apparently.

2814. Nor any law?—No. By an examination of the observations you will see what I allude to, that up to 62° they go pretty regularly, and beyond that they are extremely irregular. Of course these observations may be defective observations, but you perceive that there are five observations made upon each occasion. I did not reject any other set of observations because they did not fall within the curves, as that would have been a very unscientific proceeding, therefore I am inclined to think that in that coil there was an irregularity. It is just possible that there may have been a peculiarity in the coil itself, though certainly there was no flaw: a flaw would have shown equally with the negative current. I am inclined to think, therefore, that there is some slight chemical action which takes place between the gutta-percha and the compound, or between the compound and the copper; it is difficult to say which causes the change of resistance, when the copper is positively or negatively electrified, whether the compound or the copper.

2815. Do you know for what length of time Chatterton's compound has been used in submarine cables?—It has been used for rather more than a year to my knowledge.

2816. I presume we have not much knowledge of any chemical effect that might take place in submarine wires from the use of Chatterton's compound?—I am not aware; it has not been brought under my notice for much more than a year. Certainly at high temperatures it is a very great benefit. These two coils being precisely similar in their dimensions the vertical ordinates of the curves may be directly compared. The one prepared with Chatterton's compound is more perfectly insulated than that prepared with gutta-percha at high temperatures; but, on the other hand, and this is a point of immense importance, at low temperatures the gutta-percha is actually the better insulator of the two materials; and I have found that result to be confirmed by observations upon other coils, of which I will speak presently.

2817. Even at such a temperature as 50°?—Even at such a temperature as 60°. There was a large quantity of Chatterton's compound used in the coil supplied me for experiment, it being of small diameter. I think it is of very doubtful benefit in the manufacture of cables to use Chatterton's compound in large quantities. It has some very valuable qualities which would always, I think, as far as I can see, make me recommend it next the copper; it fills up the interstices between the different strands, copper strands being with justice considered superior to single wires. It also attaches the gutta-percha to the copper, and renders it less probable, when the gutta-percha has been stretched, that the copper should become detached; these I think very valuable qualities indeed. Those interstices would otherwise be filled with water, which would percolate to the wire, through any slight fault, and the water would do a good deal of injury by eating away the copper. But in the way of using alternate layers between the various layers of gutta-percha, I think there is much reason to make further experiments upon Chatterton's compound at low temperatures to see whether it really is a benefit.

2818. Have you tried Professor Hughes's compound?—I have not.

2819. Would not that compound fill the interstices between the strands of the copper wire?—It would; but I think it is too liquid. I have seen a specimen of it, and I think it would hardly act as a gum or varnish attaching the gutta-percha to the copper. Besides, I think Professor Hughes' special patent is for the application of a varnish or semi-fluid matter between the layers of gutta-percha, not next the copper.

2820. Have you made any experiments upon Hughes' compound?—I have not made any experiments whatever upon it.

2821. Have you made experiments upon the insulating properties of india-rubber?—Yes, one or two which I think it would be as well to mention to the committee. I made two experiments upon a knot of india-rubber covered wire which was sent down to Birkenhead for the purpose of being used by the Red Sea Company, whilst it was at the works of Messrs. R. S. Newall and Co. I compared it with a standard knot of gutta-percha covered wire. The insulating qualities of the two materials seemed to be extremely similar. I did not see any marked advantage in the gutta-percha, but it had a slight advantage over the india-rubber. The india-rubber was not so well applied as an insulator as the gutta-percha.

2822. Who covered that knot upon which you experimented with india-rubber?—Messrs. Silver, and the agent of Messrs. Silver has since informed me that they no longer approve of that covering. I believe there was cotton next the copper; but I do not consider the result obtained at all conclusive.

2823. (*Mr. Saward.*) Was the diameter of the gutta-percha and the india-rubber equal?—It was as nearly equal as Messrs. Silver could make it; their covering was then by no means perfect; it was subject to considerable irregularities. The copper was the same size as that of the Red Sea. A peculiarity in that coil of india-rubber covered wire was noticed which would give it a very great advantage over gutta-percha if it were confirmed. I allude to the charge. I made an experiment upon that knot with respect to the discharge, and I found that the charge or the quantity of electricity in that knot was only three-fourths of the charge upon a standard knot of gutta-percha. This is but one experiment, and I should not wish to attach too much importance to it, but if it be confirmed, that the inductive capacity of the two materials is at all in that ratio, it would give the india-rubber a very considerable advantage.

2824. (*Chairman.*) Will you be good enough to give the particulars of that experiment upon india-rubber covered wire?—I will send a Table of the results of that experiment to the committee. (Table No. I.)

2825. Have you considered the influences of the dimensions of the covering on the insulation of submarine cables?—Yes. I have already alluded to the fact of a change of resistance, or extra resistance, as I have called it, of the coating when under the influence of continued electrification. Now in a third coil, which was a single knot of the Red Sea cable, upon which I made similar experiments as to temperature, that

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change was extremely marked, and I believe the increased change of resistance to be due to a change in the dimensions. The diameter of the gutta-percha of the Red Sea core was 0·34 in., and the diameter of the copper 0·105. These dimensions are both larger than those of the experimental coils before described; a larger surface of the copper was exposed to the action of the compound, and also a larger body of gutta-percha was exposed to this influence of continued electrification. The effect was that the change in resistance, due to continued electrification, was enormous; the loss at 75° diminished 40 per cent., and at 60° 30 per cent., as may be seen on these diagrams (Plates V. and VI. in Mr. Jenkin's paper, Appendix No. 14.)

2826. A diminution with the temperature?—Yes; there was a marked diminution at the lower temperatures in this coil, but not in the other coils. In this coil, where there was a greater surface and mass exposed to the action of the electricity, the extra resistance and increase of extra resistance at high temperatures is very marked. You see from these diagrams that it becomes a very important element indeed; the tests at the end of five minutes are perfectly different from the tests at the end of one minute.

2827. For how long were the coils exposed to the effect of each degree of temperature?—From seven to twelve hours.

2828. So that the coil would be thoroughly heated through?—Yes. I found very considerable difficulty in keeping the mass of water at one temperature. I had not the time to put up an apparatus for agitating the water, and it was a little colder at the bottom than at the top, but I took an observation at the bottom of the tub and the top of the tub. I took a mean as the mean temperature of the coil. I should have liked to have had more accurate observations of the temperatures at which the coils were kept, but I think the curves strike a sufficiently good mean between the observations to obviate errors arising from this source. I have also endeavoured to ascertain whether the formula which was furnished to me by Professor Thomson was correct and could be applied. I found that the extra resistance prevented a very strict application of that law after the fifth minute; but that during the first minute it applied very closely indeed.

2829. (*Professor Wheatstone.*) To what law do you allude?—The law that the loss will be inversely proportional to the logarithm of the ratio of the external to the internal diameter of the coating. The proportion stands thus:—

$$L : L' :: \log.\frac{R'}{r'} : \log.\frac{R}{r},$$

where L, R and r, represent respectively the loss, the external, and the internal diameter. For instance, to compare the experiments I made upon the Red Sea coil with the experiments upon the other experimental coils, the logarithm of the ratio of the diameter for the Red Sea core is 0·513, and the logarithm of the diameters of the experimental coils is 0·699. Now, if the loss at 50° from No. 1 experimental coil, after the first minute, be represented by 600, the loss at the same temperature from the Red Sea coil will be represented by 860. These two numbers are in the proportion of 0·513 to 0·735 instead of 0·513 to 0·699, which would have been the exact ratio. The error here is only about 5 per cent., and I think, considering that the two materials were not absolutely identical, and considering the disturbing influence of continued electrification, these results may be considered as a very close verification of the law. The loss is the greatest on the Red Sea coil, and this confirms the observations on No. 2 coil, namely, that Chatterton's compound (which was used in the Red Sea coil, and not in No. 1,) was the cause of a slight falling off in the value of the insulator at low temperatures.

2830. (*Chairman.*) The experiments were not made upon two coils exactly corresponding to each other, were they?—No, they were not. I had not two coils of pure gutta-percha. The most satisfactory experiment would have been upon two coils of pure gutta-percha. In two coils of different dimensions, composed of Chatterton's compound and gutta-percha combined, the change in the relative amount of the two substances employed slightly influences the material, and I have not therefore been able strictly to verify the law. At 60° the difference between the number given by the formula and the result of observation is about 7 per cent. in favour of the Red Sea coil, a result due to Chatterton's compound and the influence of extra resistance on the larger core.

2831. Were these experiments made immediately the current was sent, or was the current allowed to remain a certain time?—I have simply taken the loss after the first minute from the curves of No. 1 coil and the Red Sea coil, for the purpose of showing whether those losses were in proportion to the numbers to which they should be proportional according to Professor Thomson's formula; and I found, assuming the coils to agree in composition, that they agree with the formula within five and seven per cent. at temperatures of 50° and 60° respectively. The losses on the two coils after the fifth minute are very near equal, instead of being nearly in the ratio of five to seven; the theoretical law, therefore, no longer applies when continued electrification and consequent extra resistance occur.

This result of the influence of resistance due to continued electrification is not at all confined to short lengths. As soon as I had observed it in a single knot, I looked for similar results in the longer cables, and for that purpose I made an experiment upon lengths of from 400 to 500 knots of cable.

2832. Have you the result of that experiment?—Yes. (Table No. III.) Upon 512 knots I found from the first observation I could make, that after about 15 seconds the loss measured upon a tangent galvanometer was 51°, after one minute it was 44½°, and after five minutes 36½°. After making the necessary reductions and calculations, I found that the loss diminished from first to last 40 per cent. with a negative current, and that from the first minute to the fifth minute the loss diminished about 25 per cent.; this entirely corroborates the experiments upon the shorter lengths, and shows, moreover, that this change takes place in very much the same time, whatever lengths of cable may be used. The loss with a positive current diminished rather less than with a negative current. In Table No. 3 not only the relative but the absolute specific resistance of the gutta-percha is given.

2833. (*Professor Wheatstone.*) In these experiments were the coils always under water?—In the first experiments the coils were under water, in the second experiment upon the longer cable the coil was not under water; but I have good reason to suppose that, except when the cable is defective, that fact does not very greatly influence the deflections obtained during the insulation test. My reason for believing this is that upon one occasion several of the tanks containing long cables were filled with water, and tests made after they had been so filled; the temperature of the water was very much the same as the temperature of the air, and the whole mass having been allowed to remain for several days in contact, the change of deflection, when all due corrections had been made for the variation of the battery, was extremely small; in fact, I think it was not more than might be due to an error of observation when the coils were good. It is an interesting fact that the addition of the water did not make any difference. On this ground I think I am justified in believing that I may consider the experiments upon the 512 knots to be corroborative of the other experiments. There was also a difference between the tests with the zinc and copper, showing the want of symmetry which I had already observed upon the short length prepared with Chatterton's compound. The test with the positive current was higher, or the loss was greater, than when tested with the negative current.

2834. Might not that arise from the poles of the battery being unequally insulated?—It might, but I do not think such was the case, for each cell of the battery was composed of glass; the glass cells were in wooden boxes which were carefully kept dry; (I found it quite impossible to get accurate results when they were not kept dry); the wooden boxes again stood upon sheets of gutta-percha, and those sheets of gutta-percha were on a wooden table attached to a stone wall; therefore, I think the batteries were very carefully insulated.

2835. (*Chairman.*) Have you considered the various methods of testing the conductors and insulating coverings of submarine cables?—I have. The mode which has been most generally adopted is the mode already alluded to; deflections are taken which are directly proportional, or the tangents or sines of which are proportional to the strength of the current; the resistance of the total circuit is inversely proportional to the strength of the current so measured; and by comparing the resistance of a circuit chiefly composed of a resistance coil, or indeed of the copper conductor of the cable itself, with the resistance of a circuit consisting mainly of the gutta-percha covering, the measurement of the absolute resistance of the gutta-percha can be obtained. Another mode of measuring the resistance of the gutta-percha or of the copper conductor with which I am acquainted offers in some respects considerable advantages. This resistance may be directly measured by means of the connexion known by the name of Wheatstone's bridge, or differential arrangement. I will first allude to some points connected with the first method of testing which I have mentioned. It is a convenient method when frequent observations have to be made by people who are, perhaps, not scientific; there is generally only a key or a switch to turn, and the deflection to observe, which almost any one is capable of doing, and, consequently, daily, and even hourly, tests can be made by people of moderate intelligence; whereas, I think some little greater skill is required in the other method of testing; but on the other hand, in the first, or usual manner of testing, it is necessary to observe and record a greater number of particulars to give the observation any scientific value. Whichever method is adopted, it is necessary that the following points should be recorded, namely, the length of the cable; whether the cable is in water, or whether it is dry; the nature of the earths employed, whether copper or iron; and the time (this is a point I wish to insist upon) which elapses between the first connexion with the battery and the moment at which the observation is noted. I think it is also very desirable that there should be frequent reversals, a subject I have already spoken of. In the first method it is necessary that some measure should be given of the delicacy of the instrument, in order that the results with various instruments may be compared. I have seen accounts of experiments, in which the deflection of galvanometers has been stated: but, being ignorant of the degree of delicacy of the instruments, I have been unable to draw any conclusions from those experiments; in other experiments some attempt has been made to describe the galvanometer, by stating that with a certain number of cells the needle deflected a certain number of degrees, but such a statement forms no guide to the delicacy of the instrument unless the total resistance of the circuit formed by the inherent resistance of the battery, the resistance of the galvanometer, and any added resistance, is also given. The resistance of the instrument itself, the strength of the battery, and the resistance of the battery should in all experiments be noted: With respect to this point, in order to measure the strength of the battery usually employed, I have taken two deflections with two known and different resistances added to that resistance, which I may term the resistance of the short circuit (the short circuit being supposed to comprehend the battery and the galvanometer), for the purpose of determining the inherent resistance of the battery. Having taken those two deflections, there is a very well-known formula* which will give me the resistance of the short circuit, provided those two deflections are taken upon measuring instruments. Having obtained the resistance of the battery and the tangent galvanometer, then adding that resistance to the previous resistance, multiplying the sum by the tangent of the deflection corresponding to that resistance (if it is a tangent of the deflection which is proportional, or the sine, if that is proportional to the strength); the product is a measure of, or at least a number proportional to the electro-motive force of the battery. I can, by this means, compare any two deflections taken at different dates, or taken by different batteries, provided they are taken upon the same instruments. I can compare any of them together. There is generally, however, some little error owing to defective observation, and a considerable number of experiments should always be made, three or four at least, upon the strength and resistance of each battery.

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2836. (*Professor Wheatstone.*) Cannot you ascertain whether the magnetism of the needle of the galvanometer remains constant, by counting the number of its oscillations?—Yes; but in using a tangent galvanometer, I believe constancy of magnetism is not of much importance, since the influence of the coils and of the earth on the needle augment and diminish in the same ratio; the case is different with an astatic galvanometer, and in this case I believe the method to be employed is that mentioned. To return to the measurement of the battery, I have frequently found that there has been a thorough want of understanding of the meaning of the words "intensity" and "quantity" of a battery. Some experimenters imagine that there is some hidden difference in the quality of two currents, and they explain the various results that they have obtained, by attributing them to the "quantity," or to the "intensity" of the current.

2837. Do not you think it would be better to discard those two words, and use the terms employed by Ohm, in his theory, namely, electro-motive force, and resistance?—Yes, it would be very desirable to abandon the words; a quantity current is simply a current in the circuit of which there is a very small inherent resistance. I wish to make one or two suggestions upon the means by which I think independent observations even by unscientific observers might be compared. There should, I think, be some well recognized standard of resistance, or resistance coil. I have myself found a great want of such a recognized standard. I have now adopted Professor W. Thomson's units, which, I believe, are based upon very scientific data; but it would be extremely convenient if there were a generally recognized standard or unit which one could always mention as one does a foot, or any other well-known unit; and I think there should be somewhere, in some public institution, some standard coil with which any resistance might be compared.

2838. In my memoir on voltaic constants, published in the "Philosophical Transactions" for 1843, I suggested that a foot of pure copper wire, weighing 100 grains, should be adopted as a standard unit?—Jacobi's unit is of the same nature; Mr. Siemens, I believe, has given a measure of mercury, and Professor Thomson has a different standard. In consequence of so many different units being proposed, it is really difficult to know in reading a treatise which is alluded to, and what co-efficient is to be adopted for the purpose of reducing the measurements from one unit into another.

2839. I have compared our resistance coil or rheostat with the same unit?—In the same way I began by adopting a certain resistance coil as the unit, and

* $BR = \frac{RT - rt}{t - T}$

where BR = battery resistance, R and r the added resistances, and T and t the respective tangents of the deflections on a tangent galvanometer or sines on a sine galvanometer.

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have since found co-efficients to reduce my measurements into other units. As there is at Kew a standard barometer and a standard thermometer, so I would suggest that there should be a standard resistance coil kept in some public institution, with which any other resistance coil sold by an instrument maker could be compared. If the instrument maker had not science sufficient to enable him to make the resistance coil correct, he could resort to the standard resistance coil, as he would in the case of a barometer or a thermometer, and a co-efficient of correction would be given him. I also think that it would be of very great assistance to independent observers that there should be a standard galvanometer, either a sine or a tangent galvanometer, by means of which the delicacy of any other galvanometer sold by an instrument maker, or in the possession of manufacturers or scientific inquirers, might be measured, and that a certain deflection of that public galvanometer should be taken as the unit; if it were a tangent galvanometer that deflection would be 45°. Other galvanometers might be directly compared with the standard, and a co-efficient having been thus obtained, all deflections might at once be reduced to what would have been observed on the standard galvanometer, a second correction being of course applied for the difference in resistance.

2840. (*Chairman.*) Have you paid attention to the resistance of copper conductors and to the influence of temperature on that resistance?—Yes. Even previously to making the experiments described upon the resistance of gutta-percha, I made a series of experiments upon the influence of temperature upon the copper conductor. In these experiments I also made use of insulated wire immersed in warm water. On this occasion I made a few experiments on the gutta-percha, but they were not so complete as the second series already described. I made use of the differential arrangement to measure the resistance of the copper. I had a couple of knots of the Alexandria and Candia cable, and I compared that with a given unit that I have adopted at Birkenhead at a known temperature. The temperature was altered at will by the use of warm and cold water. I made several experiments at several temperatures on the copper strand alone.

2841. How were the strands warmed?—They were warmed by warm water. The strands were covered with gutta-percha, as in a manufactured cable, but the resistance measured was not the resistance of the gutta-percha, but the resistance of the copper, and instead of being measured by the deflection of the sine galvanometer it was measured by the differential arrangement.

2842. (*Professor Wheatstone.*) Did not you find that the resistance increased with the temperature?—Yes; I was unable to obtain any accurate information, and wishing to be in possession of the facts as to the amount of increase, I made these experiments. I found that the increase was very regular between the temperature of 50° and 90°.

2843. Had you pure copper for that experiment?—No, it was the ordinary commercial copper used by manufacturers. It was the copper adopted in the manufacture of the Alexandria and Candia cable.

2844. (*Chairman.*) Do you think your results were influenced by the gutta-percha?—I think not. The resistance of the gutta-percha was too large in comparison with the resistance of the copper; there were only two knots, and I think the gutta-percha would have no appreciable influence on the result. Moreover the pole of the battery was not in connexion with earth, an arrangement which still further diminishes any slight loss which may have occurred through the gutta-percha. The results of my experiments are pretty accurately embodied in the following formula:—

$$R = r\,(1 + 0{\cdot}00192t),$$

where r = resistance at $a°$ Fahrenheit, R = resistance at any temperature, $t + a$. The above co-efficient, reduced for the centigrade thermometer, gives 0·00346. The co-efficient given by Becquerel is 0·004. Table IV. and Plate IV. in Mr. Jenkin's paper, Appendix No. 14, show graphically the experiments and results.* The resistance of one knot of the Alexandria and Candia cable was 2957×10^6, Thomson's units. The resistance of one knot of the Red Sea was 25×10^6, both measured at 60° Fahrenheit. The resistance of these two knots is very nearly in inverse proportion to their weight, or as 180 to 152, instead of to 150, not so much as one per cent. difference. The resistance of 2,000 yards of copper covered with india-rubber supplied by Messrs. Silver was extremely small. Instead of 25 it was only 23·4. When these different measurements are reduced to the resistance of one foot of wire of the same quality, weighing one grain, the material of the Red Sea gives a specific resistance equal to 8,510,000. Silver's india-rubber covered wire gives 7,970,000, and Professor Thomson gives as the best copper wire he has met with, 7,500,000. Therefore the Red Sea copper is within 12 per cent. as good a conductor as the best wire known. The reciprocal of these numbers, which is in fact the conductivity as it is called, or the conducting power of the copper for the Red Sea cable is 0·000001175; for Messrs. Silver's coil, 0·000001255; and for the best copper 0·000001333. The following formula will give the resistance at 60° Fahrenheit of any length of copper strand of any given weight, provided it is of the very best material:—

$$R = 397 \times 10^6 \frac{L}{W}$$

where R is the resistance of the strand in Thomson's units, L the length in knots, and W the weight in lbs. per knot.

2845. Have you considered in what way the very best copper can be secured?—No; I have had no experience in the manufacture of copper. I think it would not be difficult to set up a standard, and say (as I believe is done by the Gutta-Percha Company) nothing shall pass which is inferior to that standard. It is only on that principle that I can account for the extreme regularity of the copper which Messrs. Newall receive from the gutta-percha works. I never found it differ more than 2 or 3 per cent. from the measurement given, and I have the result of a great many experiments.

2846. Have you any remarks to make upon the charge of submarine cables?—I have not made any connected series of experiments upon the charge, as I have done upon the insulating properties of the gutta-percha. Nevertheless I have made a few experiments for the purpose of verifying the formula given by Professor Thomson, according to which the charge is inversely proportional to the same quantity as I before mentioned, the logarithm of the ratio of the internal and external diameters. I find though apparently that law is also verified in this instance, yet there is some difficulty in accurately measuring the charge. The formula usually applied in measuring the charge is based upon the theory of percussion; by this formula the charge will be proportional to the sine of half the angle of deflection, which is observed upon the charge escaping through the galvanometer. I have verified that formula up to lengths of 100 knots, or perhaps 150 knots of cable. But for longer cables of course that law no longer applies, because the whole charge does not escape instantaneously; it no longer acts as a blow upon the needle, but continues in action for a considerable time; consequently the method of measurement given above no longer applies.

2847. (*Professor Wheatstone.*) My experiments seem to show that the charge is directly as the length and not as the square of the length. Is that the result at which you have arrived?—Yes; certainly not as the square, but simply as the length. I have not been able to verify this law for lengths greater than 150 knots, in consequence of the formula not affording the means of measuring the charge. The charge measured by the half sine of the deflection on two lengths of 312 and 858 knots gave numbers in the proportion of 312 and 500, evidently showing that

the formula was no longer applicable, but the electro-magnetic induction between the coils of the cable may have influenced this result. I have measured the charge on cables of two different diameters, that is to say, on the Alexandria and Candia cable, and on the Dardanelles cable. The logarithms were as follows :—

Alexandria and Candia - -	0·51
Dardanelles - - - -	0·55

In one experiment the lengths were, Alexandria and Candia, 137 ; Dardanelles, 122 knots ; the charges calculated in proportion to the lengths, and inversely in proportion to the above logarithms, were in the proportion of 133 to 110 ; and the observed half sines were 133 and 107. The calculated and observed charge therefore agreed very closely, confirming Professor Thomson's theory. But I think that I am in possession of another means of measuring the charge upon the longer cables, which would give results to be depended upon, which the percussion formula does not in the long cables. I have not yet practised this mode, but I am nearly certain it would succeed. I propose for this purpose to employ Professor Thomson's reflecting galvanometer, the peculiarity of that galvanometer being the very small weight of the moving parts, the advantage of which is very evident. By means of that galvanometer you can observe the strength of a rapidly varying current at any moment during its variation ; that is not possible with a heavy needle galvanometer. I have made experiments which I will further allude to upon the retardation in submarine cables by means of this galvanometer. I find that I can very accurately indeed observe the increase in the current at the far end as the current arrives by means of the reflecting galvanometer, and I conceive that I could measure the gradual decrease of the current rushing in or rushing out of the cable at the near end by that galvanometer also, and thus obtain a curve expressing that decrease ; if I could not obtain it at the very beginning, I could obtain the greatest portion of the curve, and the area included between the base line, and that curve would represent the charge. That is a method, I hope shortly to employ, and I have no doubt that the charge will be found strictly proportional to the length of the cable. In speaking of the charge hitherto, I have always been speaking of the charge when the further end, or when one end of the cable, was insulated ; but there is, of course, a charge which takes place in the cable, and which is very different in amount when the end of the cable is earth. Now, it is rather important to ascertain that charge, as it forms one of the means of distinguishing the position of a fault, as was first suggested to me by Mr. F. C. Webb. If, for instance, there is a bad fault 144 miles from land in a cable of considerable length, if I knew the charge which was due to 144 miles, or to 144 miles with given added resistance, I might determine the position of that fault by that means, or at least obtain valuable data to such determination. I began to make some experiments for the purpose of ascertaining what that charge might be with various resistances, added for the purpose of similating a fault, practically ; the experiments produced results not at all expected, and which showed the experiment to be valueless for the purpose proposed. Instead of obtaining a deflection upon the galvanometer, in the same direction as that which would be caused by the charge when the one end of the cable is insulated, I got a deflection in the opposite direction. At first I was extremely puzzled to account for this, and I thought that I must have committed some error in the observation. I wrote to Professor Thomson upon that subject, but before I received his answer I convinced myself that there was no error in the observation. A little consideration showed me that it must be due to the mutual action of the adjacent coils of the coiled cable, and would not be at all observed in a straight cable.

2848. Have you ever tried to observe that effect in a straight cable?—I have not had an opportunity. In a letter of Mr. Webb's published in August, I saw the fact noticed that he had observed this false discharge as he termed it ; but that he had not observed it in a straight cable ; it was absent in a straight cable. Therefore I presume it may be supposed to be entirely due to the mutual action of the conducting wires in the coil. Professor Thomson read a paper on the subject at the meeting of the British Association.

2849. Have you found constant results, or are they capricious ?—Very constant. There are three in this Table. (Table V.) As the length increases the amount of the false discharge decreases. I think that table will show it very clearly. The false discharge seems to decrease regularly. I have no doubt that the influence to which it is due affects the rate at which the true charge escapes from the cable. Further experiments are very desirable upon straight lengths upon that point ; the charge would no longer be proportional to the length of the cable when the end was to earth ; also experiments upon the influence of temperature and upon specific inductive capacity are very much wanted. The inductive capacity varies materially ; I am not acquainted with any experiments on the subject, but I believe Professor Faraday has given some measures of inductive capacity ; those are all points for investigation.

2850. (*Mr. Saward.*) In the case of the india-rubber covered wire to which you alluded where the copper wire was first coated with some textile substance, do you think it possible that the smaller charge of electricity held by the india-rubber as compared with the gutta-percha might be in any degree due to the interposition of that fabric ?—It might be. I have not made any experiments upon india-rubber without that fibrous material.

2851. Have you read any papers that have been written from time to time by Mr. Hearder, the electrician at Plymouth, who has a theory upon that subject ?—I have not ; I have looked at them occasionally, but as I did not agree with what I saw in them, I discontinued reading them ; I should not like to give a decided opinion, not having read them carefully. If there was any influence at all, it would be from the fact that the specific inductive capacity of this fibrous material is less than that of gutta-percha.

2852. You would not attach any practical value to the use of cotton in the construction of the core ?—I think not ; but experiments upon all these points and upon the specific inductive capacity of all materials, both of that and of other materials that have been proposed, are very desirable. I know of none.

2853. (*Chairman.*) Have you made any experiments upon the retardation of signals through long submarine cables ?—Yes ; I have made a long series of experiments by the aid of Professor Thomson's marine galvanometer. I used that instrument as the receiving instrument at the far end of a considerable length of cable, which was coiled in the establishment of Messrs. R. S. Newall and Co. By the aid of a stop-watch at one time, and by the aid of a second observer at another, I was able to note the strength of the current at the far end of the cable, after various intervals of time from the first connexion of the battery at the near end, and by that means I was enabled to draw what may be called the curve of arrival. I have brought the drawings in which that curve is shown (*producing several diagrams*, Figs. 1, 2, 3, 4). In the long cables the current does not reach its maximum till after 50 or 60 seconds, and the increase is so gradual that the curve can be described from actual experiment over a very great portion of its length. The position of the small circles in the diagrams indicates the strength of current observed at the corresponding line, and the ordinates of the curve consequently represent the strength of the current at various intervals of time, the intervals of time being measured from the commencement of the diagram.

2854. (*Professor Wheatstone.*) Were the observations made at the near end ?—No, the far end. The

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strength of the current is extremely feeble at first, and gradually increases till it reaches its maximum.

2855. (*Chairman.*) What were the intervals of time?—Seconds, the vertical divisions represent seconds.

2856. And the law of increase is what?—The law of increase is given by the curve. I do not know any formula which exactly expresses it. The increase at last is extremely slow.

2857. It attains nearly its maximum very soon?—No, in the longer cables 50 seconds elapse at least before the maximum is nearly attained; 10 seconds after the application of the battery the current is still far from a maximum.

2858. After the current has attained a considerable amount of strength it increases in that strength very slowly?—Very slowly, indeed.

2859. Upon what length of cable was that experiment made?—On from 1,500 to 2,000 knots the lengths are marked on the diagrams.

2860. What was the battery power used in that case?—Seventy-two pairs of plates.

2861. Sand battery?—Daniell's; I ordinarily employ Daniell's battery. I made only one experiment then for the purpose of verifying a fact of which I felt perfectly certain, namely, that the strength of the battery did not in the least influence the law of that curve; the resistance of the battery would slightly influence it, but the electro-motive force does not in the slightest degree influence it; the maximum due to half the battery would be one-half the maximum of the whole battery, and the ordinates of the second curve will be exactly half the ordinates of the first. The result of the experiment is shown in Fig. 1.

2862. A similar curve would be shown, whatever the strength of the battery is?—Exactly, there is no difference caused in the curve by the difference in the strength of the battery. That is one proof that by an increase of the strength of the battery the speed of signalling cannot be increased.

2863. It would depend upon the power of the conductor?—It would depend upon the power of the conductor and its covering.

2864. (*Professor Wheatstone.*) Other things remaining the same, it depends upon the electro-motive force?—That is the absolute strength of the current, but the law of the curve does not depend upon the electro-motive force.

2865. (*Chairman.*) You mean, whatever the electro-motive force may be, the current would not attain its maximum a bit the sooner?—No; the maximum would be attained in exactly the same time; half the maximum in the same time, and a quarter of the maximum in the same time. So that if you were to draw the two curves to different vertical scales proportional to respective maxima, you would produce the same curve.

2866. It follows from that, if you require a certain strength of current to move a needle, that strength of current would be attained with double the electro-motive force, in half the time that it would be attained with half the electro-motive force?—Double the strength would be attained in the same time, and the required strength would be attained in a much shorter time. You can attain one signal in almost an indefinitely small time, through any length of cable, by the employment of sufficiently strong battery power, or rather sufficient electro-motive force, but it is the first signal only.

2867. Why does not that apply to the other signals. I will endeavour to explain. My experiments were immediately directed to this point, having obtained the fact as to the arrival of the current, I then wished to ascertain the law according to which the currents would move upon continued reversals. For that purpose, I got an ordinary Morse key, and a metronome, I moved this key in time with the metronome, so as to give 100, 200, or 300 reversals per minute. I had no mechanism for doing it, but it could be done by hand with sufficient accuracy for my purpose. I observed the oscillations of the spot of light which formed the index in Professor Thomson's marine galvanometer; that spot of light, supposing the cable had not been previously charged, began to rise from zero; if a very strong current was employed, it rose considerably at once, and you would at once get a signal; it rose with oscillations, rising and falling until it reached a point very nearly the middle of the scale between the maximum and minimum. At that point if the reversals were extremely rapid, the spot of light was quite stationary; and that spot of light became practically stationary with the same number of reversals, whatever the strength of the battery might be. Therefore, I cannot get a greater number of signals through a length of submarine cable by increasing the strength of my battery. I increase the amplitude of these oscillations by increasing the strength of the battery, it is true; but those amplitudes decrease in magnitude as their speed is increased, and after a certain speed they decrease so rapidly that even by trebling or quadrupling the battery power no intelligible signal is obtained at those speeds.

2868. (*Mr. Saward.*) Would the case be the same if the needle required to be moved was heavier than Professor Thomson's magnet?—If the needle were heavier, it would apparently become stationary at a lower number of reversals on account of its inertia; and, therefore, a stronger battery would permit a rather greater number of reversals to go through, because you would apparently have reached that zero point to which I allude, at which there is no oscillation perceptible before it was really reached, owing to the inertia tending to render the needle stationary; then, by using a stronger battery power, you would get a slight motion of the needle. When the needle is exceedingly light, and can be moved with a very feeble current, the point where there is an absence of all motion is reached very nearly at the same speed, I might say practically at the same speed with any given power which it is possible to use.

2869. You do not apprehend, I presume, that you would get sufficient motion under the circumstances just mentioned, to set a relay in action?—I do not. I think there is a limit, and that to set a relay in action the variation in the strength of a current must be, say, two or three per cent. of the maximum strength of the current when allowed to flow continuously through the cable. I am certain that that limit is in various lengths of cables as the square of those lengths. The amplitudes I find, when they are reduced to the same strength of maximum current, are in different lengths, precisely as the square of the lengths. The law of squares, which has been for such a long time propounded by Professor Thomson, is completely verified. I have a table of those amplitudes:—(Table VI.) To reduce the observed amplitudes to those due to a constant maximum current, they have been multiplied by the ratio of that constant current to their own maximum. This results from the observation before named, that double the strength of battery will give double the amplitude.

2870. (*Professor Wheatstone.*) When you speak of increasing the strength of the battery, do you mean increasing the electro-motive force or decreasing the resistance?—I always mean the electro-motive force. I should have said by doubling the electro-motive force, provided the resistance of the entire circuit remains the same I shall get double the amplitude; but, owing to the conformation of the curve, as shown at its rise from the origin, the first increase being extremely slow, before the great increase comes, the oscillations will become illegible at the same speed, irrespective of the ellectro-motive force employed. Any oscillation will be doubled by the use of double the electromotive force. But on the other hand, with a given number of reversals, the deflection or amplitude of almost any strength will be so extremely small as not to be of any use. Having made these experiments upon reversals, I made experiments upon long and short signals, such as are used in Morse signalling. Those are represented in this diagram (Figures 2 and 3), (*producing the same*), by which the speed is clearly shown at which signals become legible by means of

relay. The experiments at higher speeds are shown near the origin of the curve, and the experiments at lower speeds are shown towards the opposite end of the curve. The zigzag lines rising and falling equally represent dots, others rising and falling unequally alternate dots and lines. No signals would be legible on a relay unless a line parallel to the base intersected every zigzag, whether representing dots, lines, or alternate dots and lines. At the higher speeds the dots and lines appear on quite a different part of the scale. The speed at which signals become such as could be received by a relay is easily perceived by this mode of experiment and graphic representation. The zigzags have no further connexion with the curves on the same diagrams than this,—both result from experiments on the same cable with the same battery. They are drawn in one figure to save space. These results are very much vitiated by the fact of the cable being coiled. I am certain the mutual influence of the coils, which is shown in the charge experiments, exercises a sensible effect both upon the curves which represent the law of the arrival of the current upon the number of reversals sent, so that it is very desirable that the same experiments should be repeated upon straight cables.

2871. (*Chairman.*) Have you not made the same experiments upon straight cables?—No; these experiments were made in July of the present year, and since then I have had no opportunity. There is some influence upon the law of arrival produced probably by the change in the insulation of the gutta-percha. Professor Thomson published a very beautiful theory in 1855, in a communication to the Royal Society, in which he gave the theory of the arrival of electricity at the farther end; and he gave a set of curves which in general character extremely closely agree with these. That theory included the case of constant imperfect insulation such as was supposed to be produced by gutta-percha; but the change in the gutta-percha to which I have referred, or the extra resistance, renders a further modification of the theory necessary.

2872. You have had so much experience in the mechanical part of paying-out cables that I will ask you a question upon that subject. *What* specific gravity of cable would you prefer for deep water?—It seems clear that the lighter the cable can be for its strength the better, provided the extension produced by any strain to which it is liable be not also increased, and provided the gutta-percha is duly protected. I do not think a cable can be of too light specific gravity, provided it is at all heavier than the water. I do not, however, consider that there *is* any benefit to be derived from the addition of such a material as hemp to an iron or steel cable, for the purpose of diminishing its specific gravity. The available strength of the cable is not increased, though the specific gravity is diminished by the addition of a material of the same specific gravity as water. Though I decrease the specific gravity of that cable, I do not really increase the number of fathoms of its own weight which it will carry. To effect this it would be necessary to add a material which was lighter than water. I think a good many inventors and manufacturers of cables have here been led astray; they have diminished the specific gravity of the cable, but have not really added to its strength. For instance, we will say a given wire cable will support only 6,000 fathoms of its own length in water; suppose I cover that wire cable with hemp (assuming always that hemp *does not* add to the strength of the wire), but is put only for the purpose of diminishing the specific gravity of the cable; that hemp has no power of floatation, being nearly of the same specific gravity as water, and neither takes away from nor adds to the weight which the cable will carry; it does not add to nor diminish the weight of any given length of the cable in water; nor does it diminish the strain required in order to lay the cable without slack; that cable, will, as before, support only 6,000 fathoms of its own length; consequently the diminution of specific gravity has by no means improved the cable.

2873. But the weight which the cable will support in the water depends upon the specific gravity of the cable, does not it?—Not necessarily. A cable gets the appearance of strength from this fact; a man says, my cable is of very light specific gravity; it will support ten tons; your cable is of very great specific gravity; it will support ten tons; apparently he has the advantage, but he has not any real advantage unless the material which he has added for the purpose of diminishing the specific gravity is lighter than water. *F. Jenkin, Esq.* 22 Dec. 1859.

2874. Suppose you have a given length of cable which weighs a given quantity; say a knot of cable which weighs a ton, and you hang that cable to the edge of your ship; if that cable is hung at the end of your ship in the air, a certain quantity of strain is produced upon the point where it hangs, and if you immerse that cable in the water, the strain upon that point *will* be diminished by a certain quantity dependent upon the specific gravity of the cable?—Certainly. In that case I should say that by the addition of the hemp to the cable you are simply adding unnecessary weight to the cable out of water, although you certainly relieve the cable of that weight again when it is put into the water. The dead weight of the cable in air is increased, and its specific gravity diminished. The advantage and disadvantage precisely neutralize one another when the cable is placed in water, if the material added is of the same specific gravity as water. If the material added were heavier than water there would be a loss, and if the material were lighter than water, an advantage in the addition. The cable is not changed as regards its strength for supporting a given length of its own material by the addition of that hemp.

2875. If the amount which a cable will bear when immersed in the water depends upon the specific gravity of the material which is so immersed, that law must hold good whether it is the material of a cable or any other material?—I think I can make my views clear in this way. Suppose a cable is made up of 12 wires which weigh 15 cwt. in the water per knot, and about 17 cwt. in air; and suppose that to that cable you add four cwt. of hemp which upon my supposition does not add to the strength of the cable, but is merely put there for the purpose of diminishing its specific gravity; a knot of that cable will weigh 21 cwt. in air; in water that cable will still weigh 13 cwt. A knot of cable in water weighs precisely the same as if the hemp had never been put there, in spite of the changed specific gravity. The strength of the cable is the strength simply of the iron covered cable without any hemp. Granted that the weight of a knot of the hemp and iron cable in the water is the same as the weight of the iron covered cable without the hemp, then the number of knots which the two cables will sustain in water without breaking will be the same. Hemp has no buoyancy; it will hardly float, much less will it buoy up any other material. There is a slight advantage in favour of a cable that is of light specific gravity and great diameter, the friction of the water will, I think, slightly relieve it, but very slightly.

2876. How do you mean that the friction of the water will relieve it?—I think that the friction of the water will cause a slight diminution of the theoretical strain; each particle of the cable, when laid without slack, follows a line which bisects the angle formed by the inclination of the cable with the horizon. The friction very slightly counteracts the tendency of the cable to slide down in the direction of its length.

2877. The cable in being laid gradually sinks through the water?—Yes; and as the specific gravity diminished the cable would lie at a smaller angle with the horizon, but the strain would not be influenced so long as the absolute weight of the cable in water remained the same.

2878. Of course the strength of a cable will only depend upon the strength of those parts of it which contribute to its strength; that there is no question about?—Yes; and the addition of a material that does not float, will not lighten the cable in water, will not

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add buoyancy, and if it does not add strength the cable remains unchanged. Suppose a cable weighs 13 cwt. per knot in water, and it will carry six knots of its own weight, and you add to the iron a quantity of hemp, which neither diminishes nor adds to its weight in water nor to its strength, that cable will carry still six knots of its own weight, though the specific gravity is very much changed.

2879. It will keep all the strength that the iron will give it?—Yes; and if the hemp, as in some instances of cables projected by Mr. Gisborne, is so applied as to add strength to the cable, then it is an improvement. If the hemp is added so as to give strength, then it completely alters the data, and then, of course, the lighter you can get your cable the better.

2880. I apprehend the only advantage of covering this iron with hemp is that it fills up the interstices between the bars of iron instead of filling up the outer covering with additional pieces of iron; it just increases the diameter of each of these pieces of iron so as to protect the gutta-percha inside; it is not to give it strength?—Unless it did so, it would be useless, in my opinion.

2881. You do not wish to leave the core uncovered?—No, certainly not; but unless the hemp adds strength, I would cover the wire completely with iron or steel. The addition of metal does not weaken the cable relatively to its own weight per knot. Suppose I take a cable in which I put 18 very light wires, and another in which I put 18 very heavy wires, in the one case I have an enormously strong cable and in the other a very weak cable; both carry the same length of their own substance. What Mr. Gisborne has done by his experiment is to produce a cable which will carry a great deal more of itself than can be done by mere steel or iron cable in water.

2882. Is it not a simple question of specific gravity?—As far as I see, it is not a mere question of specific gravity. In Mr. Gisborne's cable the hemp adds its strength to the strength of the iron.

2883. You have not seen the experiments, have you, upon Mr. Gisborne's cable?—I have heard the result of the experiments; there is additional strength by the addition of the hemp.

2884. To a moderate extent?—I have heard that the strength of the cable is nearly equal to the strength of the iron and the hemp added together. In that case the strength of the cable is increased.

2885. However, you prefer a light cable?—Yes, provided it is not elastic, but I have not yet seen any so-called light cable which fulfils all the necessary conditions, unless it be Mr. Gisborne's cable. Usually they are elastic, and the insulating material is insufficiently protected.

2886. Have you seen Mr. Allan's cable?—Yes; but I should prefer not giving evidence upon engineering subjects. Being in the service of Messrs. R. S. Newall and Co., who are at this time all absent from England, I do not feel at liberty to give evidence upon the engineering parts of the question without their consent. Electrically of course Mr. Allan's cable would be very much opposed to my views.

2887. Have you made any experiments electrically upon Mr. Allan's cable?—No; I do not think special experiments are required in order to arrive at some of the effects which would be produced in this cable.

2888. What would be the effect electrically?—The large surface of the conductor would greatly increase the charge of the cable. The charge of the cable is the great electrical difficulty in submarine lines. When the metal employed is a bad conductor, the charge will be greatly increased, either by the increase of surface consequent on increased area of the conductor, or if the conductor is kept small, then by the increase of resistance in that conductor. If to obviate this difficulty the thickness of the insulating cover is increased, the cost will be increased.

2889. (*Professor Wheatstone.*) In constructing lines generally, would you prefer a thin or a thick wire?—I think that is entirely a question of maximum and minimum, to be determined by the cost of the two materials. There will always be a given proportion between gutta-percha and copper, by which a maximum of effect will be attained at a minimum of cost.

2890. Cost being out of the question, would you prefer a thick wire?—I should prefer a thick wire, provided the quantity of gutta-percha is also increased. The thicker the wire the better, and the more gutta-percha the better.

Tables handed in by Mr. Fleeming Jenkin.

Table No. 1.

Experiments made on the 21st September 1859, on the insulation 2,000 yards india-rubber covered wire manufactured by Messrs. Silver for the Red Sea Company.

The coil was completely immersed in water of the temperature of 61¾ Fahrenheit, and during the experiment the pressure in the tank containing the water and coil was maintained at 600 lbs. per square inch.

The experiments were conducted on a Ruhmkort Sine Galvanometer. The zero point = 3481.

	Number observed.	Angle of Deflection.	Angle.	Sine.	Sine corrected for connexion.
Zinc to coil after 1st minute	3409	7°3	7°12	1253	1061
2d "	3411	7·0	7°0	1219	1027
3d "	3413	6·8	6°48	1184	992
4th "	3417	6·4	6°24	1115	923
5th "	3422	5·9	5°54	1028	836
10th "	3427	5·6	5°36	976	784
Copper to coil after 1st minute	3414	6·7	6°42	1167	1062
2d "	3418	6·3	6°18	1097	992
3d "	3418	6·3	6°18	1097	992
4th "	3420	6·1	6°6	1063	958
5th "	3420	6·1	6°6	1063	958
10th "	3420	6·1	6°6	1063	958
Connexions.					
Copper to connexion	3476	0·6	0°36	105	—
Zinc to connexion	3470	1·1	1°6	193	—

S C + 30. 2°.—Experiments on charge of the same coil.

Number of Plates.	Deflection observed on a Tangent Galvanometer.	Sine of half Angle observed.
72	6°	823
96	10·20	915

Table No. 2.

1st.—Experiments made on the 21st September on the insulation 2,028 yards gutta-percha covered wire manufactured for the Red Sea Company.

Temperature of water, 61¾°.
Pressure in tank, 600 lbs.
Zero of sine galvanometer, 3486.

	Number observed.	Angle of Deflection.	Angle and Minutes.	Sine.	Sine corrected for connexions.
Zinc to coil after 1st minute	3425	6° 1	6° 6′	1063	993
2d "	3433	5° 3	5°18′	924	854
3d "	3432	5° 4	5°24′	941	871
4th "	3435	5° 1	5° 6′	889	819
5th "	3435	5° 1	5° 6′	889	819
Copper to coil after 1st minute	3424	6° 2	6°12′	1080	1080
2d "	3433	5° 3	5°18′	924	924
3d "	3436	5° 0	5° 0′	872	872
4th "	3439	4° 7	4°42′	819	819
5th "	3439	4° 7	4°42′	819	819
Connexions.					
Zinc to connexion	3482	0·4	0°24′	70	—
Copper to connexion	3486	0.6	0	0	—

2°.—Experiments on the charge of the same coil.

Number of Plates.	Deflection observed on a Tangent Galvanometer.	Sine of half Angle observed.
72	8°	697
96	14°	1219

FIG. 1.

DIAGRAM SHEWING EFFECT OF ELECTROMOTIVE FORCE ON ARRIVAL CURVE.

Length of Cable 2108 Knots. *Maximum deflexion with 72 p.p. 130*

" Maximum deflexion with 36 p.p. 63.

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FIG. 3.

DIAGRAM SHEWING ARRIVAL CURVE & REVERSALS at VARIOUS SPEEDS.

Length of Cable 2192 Knots.

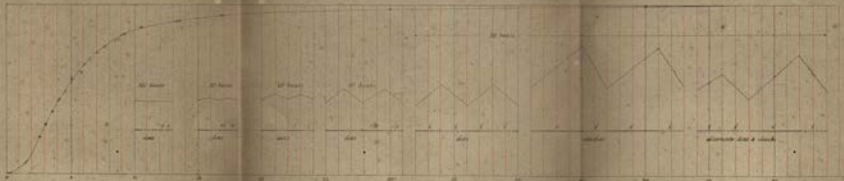

FIG. 3.

DIAGRAM SHEWING ARRIVAL CURVE AND REVERSALS AT VARIOUS SPEEDS.

Length of Cable 1500 Knots.

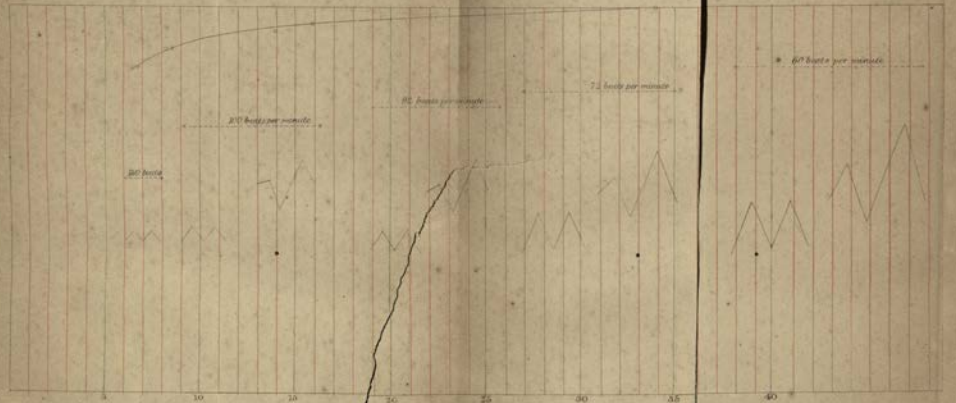

FIG. 4.

DIRECT CURRENT AND EARTH CONTACT.

Common Key.

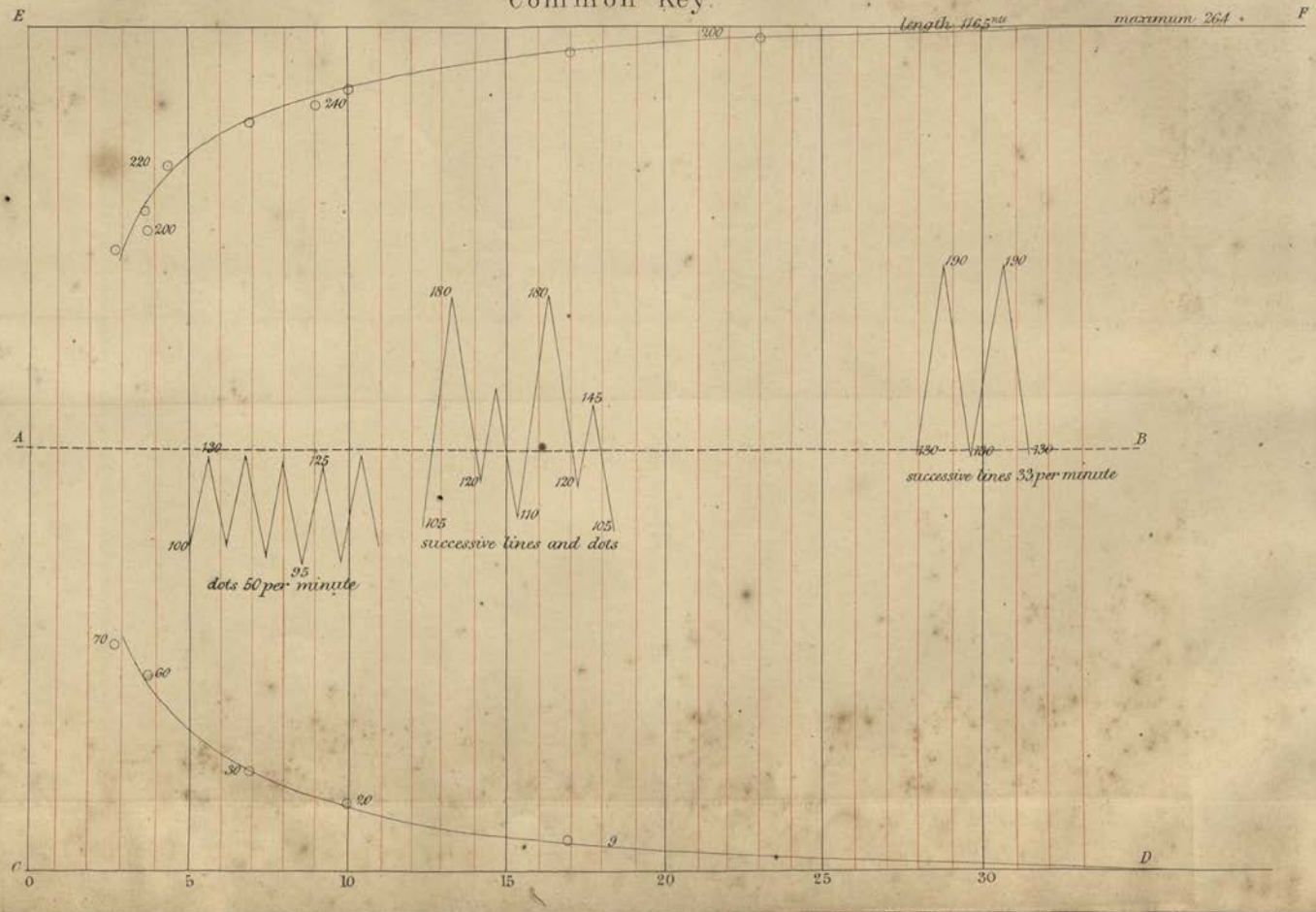

FIG. 5.

REVERSE CURRENTS,

Siemens Key.

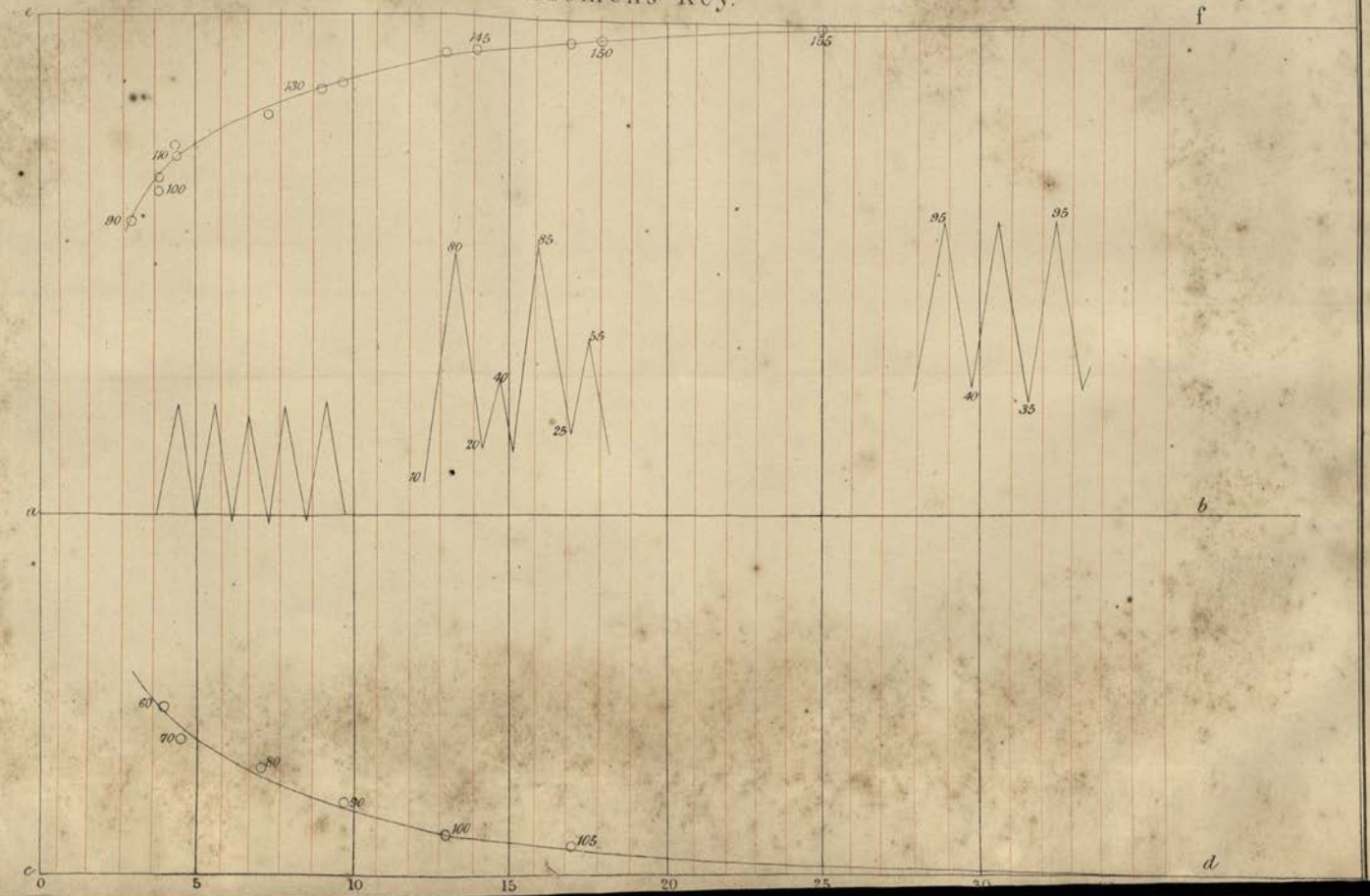

TABLE No. 3.

Insulation test of 512 Knots of Red Sea Cable.

—	Deflection.	Observed.	Specific Resistance calculated from previous Column.	Mean specific Resistance calculated from four Observations.
		°		
Zinc to Cable, Copper to Earth.	1st steady - -	51	1631×10^{17}	
	After 30 seconds	Not taken	——	1627×10^{17}
	1 minute	44½	2071×10^{17}	2078×10^{17}
	2 ,,	41	2355×10^{17}	2365×10^{17}
	3 ,,	38½	2583×10^{17}	2554×10^{17}
	4 ,,	37½	2680×10^{17}	2622×10^{17}
	5 ,,	36½	2781×10^{17}	2698×10^{17}
	1st steady - -	51	1631×10^{17}	1624×10^{17}
	After 30 seconds	48°30	1791×10^{17}	1765×10^{17}
	1 minute	45	2035×10^{17}	1888×10^{17}
	2 ,,	43	2187×10^{17}	2389×10^{17}
	3 ,,	41	2355×10^{17}	2278×10^{17}
	4 ,,	39°30	2488×10^{17}	2368×10^{17}
	5 ,,	39	2533×10^{17}	2397×10^{17}

TABLE No. 4.

One Knot of Alexandria and Candia Cable compared with 178×10^7. T.O.

Temperature.	Readings on scale of Differential Instrument.		Mean 1.	Mean 2.	Resistance in Thomson's Units.
	Position 1.	Position 2.			
88	7420	2305	74170	25830	620×10^6
82½	7440	2385	74275	25725	616×10^6
74½	7460	2361	74495	25505	611×10^6
66½	7402	2332	74809	25200	600×10^6
61½	7520	2507	75065	24935	592×10^6
53½	7559	2505	7527	2473	585×10

TABLE No. 5.

EXPERIMENTS on charge of a coiled Cable with the end free and to earth respectively. April 9th 1859.

Length in Knot.	End to Earth.		End insulated.	
	1st to Right.	2d to Left.	1st to Left.	2d to Right.
137½	37°	18	18½	14½
281½	31°	10	28½	26½
389	21°	8	41½	68°

TABLE No. 6.

Amplitude of oscillation of spot of light in reflecting galvanometer corresponding to variation of strength of current at the receiving end of various lengths for various speeds of reversals.

The amplitudes in 3d column are reduced to what would have been observed had all the maximum currents been alike.

Length of 1,500 knots.

Reversals per Minute.	Amplitude.	Reduced Amplitudes.
112	6	3
100	8	4
92	10	5
72	17	8½
60	25	12½
Length of 1,802 knots.		
132	0	0
112	2	1·5
100	2½	2·1
92	4½	3·7
84	5	3
72	6½	3·9
60	10	6
50	15 to 16	9 to 9½
40	25	15
Length of 2,192 knots.		
100	Less than 1	Less than 1
80	1	0·75
60	3 to 4	2½ to 3
40	10	7·5
36	13	9·3
30	17	12·8

DEAR SIR, Turin, September 28, 1860.

I MUCH regret that I have not been able sooner to comply with your wish that I should send a paper on "Faults in Submarine Cables" to the committee. I had no intention of publishing anything on this subject before making a complete course of experiments. My conversation with you, however, showed me that even the few facts of which I am in possession may be interesting, and may perhaps in some measure restore confidence in the durability of submarine cables, by showing that recent failures probably depend on causes which, if known, can be avoided.

I am myself convinced that a submarine cable, insulated by gutta-percha, which tests well shortly after being laid down, will remain in good condition, if properly used, for an indefinite number of years. On the other hand, ignorance or neglect may in one day, or one hour, destroy an excellent line. I will first draw attention to the meaning of the words, in good condition, and to the proofs which it is in our power to obtain of the good or bad condition of a cable. I have already stated in my evidence before the committee as well as in the paper read to the Royal Society, and published by the committee, that the loss from a sound wire covered cable is not affected by submersion, provided the temperature of the gutta-percha is unaltered. This circumstance affords one simple and valuable means of ascertaining the condition of a cable; I have lately had an opportunity of again verifying this assertion, which I now consider placed beyond all doubt. The specific resistance of the gutta-percha of a cable tested on the 22nd August 1860 before immersion, was, after one minute's electrification with zinc to cable 1745×10^{10} Thomson's units. The specific resistance of the gutta-percha of the same cable, tested the same day under water, was, after one minute's electrification with zinc to cable 1996×10^{10} Thomson's units. The increase of resistance when under water was due to a decrease of temperature. Tests made with copper to cable, and after longer electrification, confirmed this result. The length of this cable was 18 knots. Several other cables gave similar results.

Thus to establish the good condition of a cable, it is necessary that the tests before and after submersion should agree, due correction being made for temperature. Unfortunately, even where the temperature of the cable after submersion can be accurately determined, even this agreement will not suffice to establish that a long cable is quite sound. I have discovered and made faults so extremely minute, that they would not have added one per cent to the loss from a cable of a thousand knots; we have not yet brought our tests to such perfection as would enable us to cavil at a difference of one per cent., or two per cent.

For instance, the resistance of one natural fault which I discovered, was about 14×10^{10} Thomson's units; the resistance of the covering of 1000 knots of the Red Sea cable at 60°, is about 3×10^{12} Thomson's units. This fault would, therefore, have diminished the resistance of the 1000 knots of gutta-percha, by about two per cent. I have artificially made still smaller faults. A discrepancy of tests amounting to only two per cent, would be quite insufficient to establish the existence of a fault, as will be obvious if we consider, 1st, the rapid and great change due to extra resistance, and 2nd, the difficulty of accurately ascertaining the temperature of a long cable. The fault above named, (which I discovered in a short cable) would necessarily have passed unperceived in a length of 500 knots, and probably even in much shorter lengths, yet this very fault, without most careful management, would have proved fatal to a cable, as will presently be shown.

Extra resistance gives another useful test of the condition of a cable. The tests of a fault with a negative current always show a smaller resistance than those made with a positive current, and the extra resistance caused by a positive current is also always greater than that due to a negative current. The permanent passage of both negative and positive currents seems ultimately always to increase the resistance of small faults. All these phenomena are, however, on too small a scale to be perceived in a long cable. They are completely outweighed and obscured by the effects produced in the sound portions of the cable.

The probable difference of quality in gutta-percha used at different dates, to cover different cables, prevents an entire reliance on a comparison with previous measurements of the resistance of gutta-percha; and without that entire reliance very small faults, such as have been described,

F. Jenkin, Esq.

22 Dec. 1859.

cannot be discovered by measuring the absolute resistance of the covering.

It will here be asked, if neither the absolute resistance of the insulating cover, nor the examination of the extra resistance, nor the comparison of tests when dry and wet, will suffice to establish the sound condition of a cable, What hope is there of obtaining this knowledge?

I reluctantly own that no test I have yet made will enable me to be quite certain that a long cable is quite sound.

I have still some tests to try, with statical electricity, but I have no great hope that they will overcome the difficulty, which consists in detecting a small loss due to a defect, in the midst of a large loss due to the normal condition of gutta-percha.

Nevertheless, as I do not consider the existence of faults so minute as those to which I allude necessarily fatal to a cable, I would readily certify that a cable is good if it fulfils the following conditions:—

1st. The specific resistance of the gutta-percha must not fall short by more than ten per cent. of the numbers given in my paper on gutta-percha.

2d. The resistance of the gutta-percha must be the same shortly after immersion as before immersion, due allowance being made for the change of temperature.

3d. The tests with positive and negative poles of the battery must be symmetrical if the wire is covered with pure gutta percha. In any case, the tests must be nearly symmetrical.

4th. The extra resistance caused by positive or by negative currents must be nearly equal after equal periods of electrification.

I will now examine the probable duration of a cable in which these conditions are fulfilled, admitting that even in this cable small faults may exist. I must, of course, set aside the chances of mechanical injury, which are different in every different case, and examine only the dangers caused by the daily use of the line, by testing by lightning, or by any other cause common to all cables. I have no reason to believe that pure gutta-percha undergoes any decomposition or permanent alteration after submersion; I have, indeed, some proof to the contrary. The specific resistance of the gutta-percha of a cable which was made six years ago, and which lay for three years in deep water, is now 1840×10^{9} at 60° with copper or with zinc to cable after one minute's electrification. The test, when the cable was dry, agreed with that made after immersion, and the tests of extra resistance were symmetrical when made with the two opposed currents. I therefore consider the gutta-percha of this cable quite good. This cable had, however, been dry for two years immediately before this test was made, and I am in possession of certain facts which seem to prove that continued immersion does diminish the specific resistance of some gutta-percha. I cannot, however, publish the experiments on this point. I must first thoroughly examine the permanency and extent of the effect produced, which may depend on the absorption of a small quantity of water, which, to some extent, damages the insulation of the cable, although not in such a manner as to endanger communication, nor, as would appear from the experiment last mentioned, permanently to alter the nature of the gutta-percha. This effect may, however, have been due to other causes than immersion; and I only mention the phenomenon that further experiments may be made. I can, however, safely say, that I have never yet seen or heard of any considerable length of gutta-percha which had suffered any deterioration throughout its entire substance after submersion, such as would seriously injure the transmission of signals through the cable. Assuming, therefore, that the sound parts of a cable will remain sound unless mechanically injured, I will now examine what changes may occur in the faults which I have assumed to exist in the cable.

To dispose at once of the simplest case. Lightning will certainly burn these faults, melting the gutta-percha to a considerable distance. I have seen a fault about 18 inches long, which could only be due to this cause. The carelessness shown with regard to lightning dischargers is sometimes very great. I know of two dischargers at the ends of a cable which have not been visited for months; and of which the points were, on the last examination, reported to be melted. Considering these facts, is it astonishing that cables fail? Lightning dischargers are, however, well understood, and I need say nothing further on this point.

I will next consider the action of a battery upon these small faults, and the importance of this point cannot be overrated. First under this head, I will examine the degree in which faults are subjected to the action of the battery according to their position in the cable; whether near the battery or at a distance from the battery. The internal wire of a submerged cable, charged by a battery, may be considered as one source of electricity. The water surrounding the cable as a second source of electricity; the potential of the latter source being zero. The potential of the internal wire of the cable, when perfectly insulated at all points, is equal to the potential of the pole of the battery with which it is in connexion. In a well insulated cable this is sensibly the case, when one end of the wire is connected with the battery and the other end insulated. When, however, the other end of the wire is connected with earth (as when signals are being sent), the potential of the internal wire varies at every point. Close to the battery, the potential is still equal to that of the pole of the battery with which it is connected. Close to the end of the wire which is connected with earth the potential at the wire is zero. At intermediate points the potential diminishes in direct proportion with the distance of each point measured by electrical resistance from the pole of the battery; thus, midway the potential is half that of the pole of the battery, and so on. This law obviously only holds good so long as the fault offers a resistance, large as compared with that of the portion of conducting wire between the fault and the end connected with earth. Some slight divergence from the law described will also occur in a cable which is not perfectly insulated, but which is equally insulated through its entire length, as in the case of a sound gutta-percha cable. It is not now, however, necessary for my purpose to investigate the precise sequence of the potentials at the consecutive points of such a cable, although this may some day become a valuable test. My wish now is only to show to what extent a battery exerts its full power on defective portions of a cable at different points of its length. From what has just been said, it results that if 100 cells are used in signalling through a cable 500 knots long, a fault which exists midway will only be subjected to a statical power equal to that of 50 cells applied directly at the spot where the fault exists; and a fault 400 knots distant from the battery would only be subjected to a statical power equal to that of 20 cells. When, however, signals are sent from the opposite end, this latter fault will be subjected to a power equal to that of 80 cells. When there are several faults in a cable, the the potential at different points of the wire no longer varies in direct proportion with the linear distance from the pole of the battery, as is the case in a sound uniform cable, but diminishes rapidly after each fault; the larger the fault the greater the diminution of potential in the wire beyond it. Each fault, as it were, protects those parts of the cable beyond it from the full action of the battery, and thus it may happen that mending one fault close to shore may cause a fault at a greater distance to deteriorate rapidly, under the action of a battery, which had not before injured it (assuming for the moment that the action of a battery is detrimental). From the above considerations, we see that a fault near one end of a cable may be uninjured by signals sent from the opposite end, but may be much damaged by signals sent with an equal battery in the opposite direction. We also see that a cable is more severely tried in some respects at all points distant from the shores when tested for insulation than when used to transmit signals, the battery remaining the same.

I will now state the reasons which lead me to think that the action of a battery on a fault will generally, if not always, be detrimental. Wherever two sources of electricity at different potentials are in close electrical juxtaposition, there the phenomena of heat and chemical action will appear, in a greater or less degree, according to the difference of potentials of the two sources, and according to the greater or less resistance of the medium separating those sources. If the resistance of the medium separating the two sources be very great, a very small current will pass from one source to the other, and very little heat or chemical action will be developed. On the other hand, if the resistance be small, the current passing from one source to the other will be great, and the chemical action and heat will also be great. The greater the difference of potentials between the two sources, the greater will be the heat and chemical action (by the word heat, I do not mean sensible temperature which depends on the specific heat and dimensions of the medium, but the number of units of heat developed, which are independent of these influences). In a cable we have just shown how the battery, the resistance of the wire, and the position of a fault determine the difference of potential between the internal wire and the surrounding water at the fault. The nature and dimensions of the fault determine the resistance of the medium separating these two potentials.

The heat developed at this point can only be injurious to the cable, but some electricians have been of opinion that the chemical action may positively be beneficial. I incline at present to a contrary opinion so far, at least, as concerns any intermittent chemical action, such as is produced during the transmission of signals. Several experiments have led me to assume that this intermittent action is always detrimental. If this assumption be correct, the action of a powerful battery on a small fault would be as follows:—When the fault is very small, the heat and chemical action developed at such fault will also be small; but assuming that their tendency is to increase the fault, the heat and chemical action will also increase, very slowly at first, but with gradually increasing rapidity, until the time arrives at which the resistance of the fault becomes small enough to alter sensibly the potential of the internal wire at the fault. When this occurs, the deterioration will no longer proceed so rapidly as before, for as the fault becomes larger the potentials of the internal wire and of the water will differ less and less. The effect being as though a smaller and smaller battery were directly applied to the fault, until at last the resistance of the fault will become so small that unless the battery (and by this means the difference of potentials between the wire and water) be increased, the fault will get no worse. The limit at which the fault will no longer increase depends simply on the potentials excited in the wire by the battery; thus, the smaller the battery employed, the sooner will this limit be reached.

The assumption on which this reasoning is based, namely, that any small fault will increase under the intermittent action of a battery capable of producing a potential in the internal wire of a certain intensity, will not be admitted by many electricians, and I myself think it absolutely necessary that many more experiments should be made before this assumption can be said to be either established or refuted. I hope to investigate this question more fully than I have yet done, and I will endeavour to establish in some sort the limit of the harm which can be done by batteries of different strengths.

I believe many electricians think that positive electrification always improves the insulation of a faulty cable, but that negative electrification quickly destroys the improvement. I have however found that the permanent application of either the positive or negative poles of a battery will augment considerably the resistance of a puncture in the gutta-percha covering of a cable immersed in water. In salt water the hole is rapidly stopped up by chloride of copper in the first case, and by soda in the second case. The reversal of the current in either case quickly dissipated the artificial resistance made by these substances, and possibly by bubbles of gas, and showed that the fault had really increased, the resistance after each reversal being at first rather less than the resistance to a similar current at the beginning of its previous application. An effect similar in kind but much smaller in degree, was produced by simply discontinuing the current and again applying it. But it is a matter of further experiment whether, by carefully maintaining a potential in the conducting wire, inferior or superior to that of the surrounding water, varying the amount but not the sign of the potential during the signals, a permanent good effect might not be produced on a fault. Thus in signalling, the first contact being made with the zinc pole of a battery of 20 elements, the second contact might be made with the zinc pole of a battery of two elements. This arrangement would give a signalling power of 18 elements, and would continually maintain the potential of the conducting wire inferior to that of the surrounding water; the connexion with the zinc pole of two elements being continued even when no signals are sent; the object being to prevent the intermittance or reversal of chemical action at the fault. The same experiment might be tried with a positive potential in the wire, although I should much prefer using a negative potential in all cases, as the chloride of copper formed by a positive potential tends gradually to eat away the wire and cause a solution of continuity. These and other very necessary and interesting experiments are unfortunately inconclusive, unless continued for years, and the different nature of different faults makes any conclusion from limited experiments extremely dangerous.

Meanwhile, I beg to urge that no trust be placed in these artificial benefits, and that above all, powerful batteries should never be used to produce them. The beneficial effects which may arise from oxidation of the copper or from a deposit of soda, will be produced even by a weak battery.

The time required for the complete result will only be somewhat lengthened by the small number of elements employed. The dangers to be avoided, on the other hand, namely, melting or burning the gutta-percha by the battery, the destruction of the copper conductor, and possibly the electrolysis of the gutta-percha, are all much increased by any increase in the electro-motive force of the battery used. Sensitive receiving instruments allow of an almost indefinite decrease of the battery power employed to signal without any decrease in the speed of reception.

The disturbing influence of line currents alone limits the diminution of battery power. These line currents will not, I think, exist to any serious extent in a sound cable or in a cable with small faults, unless, perhaps, during electrical storms.

It may however be urged, that the difficulty of signalling will be equal, whether small batteries are used in a cable with small faults, or large batteries for the same cable with large faults; but this is not so. If the resistance of a fault be 99 and the resistance of that part of the wire which separates the faulty part of the cable from earth at the receiving end be 1, then 1 per cent. only of the current will be lost at the fault; whereas if the resistance of the fault is reduced to 1, then half the current is lost at the fault. By still further enlarging the fault, 99 per cent. may be lost; and it is evident that 99 per cent. of a small current arriving at the receiving end of a wire will be much preferable to 1 per cent. or even 50 per cent. of a larger current.

The few experiments I have lately made to determine the number of cells which may be safely used, convince me that much smaller batteries are dangerous than even those commonly used, and that the evil effects are frequently imperceptible for a considerable time after the cable has been exposed to their action. These experiments so far agree with the theory developed; but there are so many kinds of faults, and they are so differently affected by the same circumstances, that general conclusions must be carefully avoided.

I have met with faults of the following various kinds, each of which should form the subject of separate series of experiments.

1st. Small punctures.

2nd. Air bubbles thinly enclosed in gutta-percha.

3rd. Copper wires separated from the water by very thin gutta-percha.

4th. Porous gutta-percha.

5th. A foreign body connecting the copper inside the gutta-percha with the water outside.

Each of these faults has its own pathology, and I must again apologize for the very small number of facts I am able to produce throwing light on their behaviour.

The first experiment to which I would draw attention, was tried on a fault made artificially by simply piercing the gutta-percha of a cable with a pricker. I tested this fault with 200 Daniell elements. The deflection showing the loss on a tangent galvanometer, with copper to cable, was 3°, corresponding to a resistance of about 3×10^{10} Thomson's units; the loss slowly decreased to 2½°. The following Table shows the effect of frequent reversals continued for about an hour.

Pole of Battery connected with Cable.	Loss immediately after connexion with Battery.	Loss some Minutes after connexion with Battery.
Copper	3	2½
Zinc	7	9
Copper	5	1
Zinc	10	8¼
Copper	5½	1½
Zinc	12	11
Copper	6½	1½
Zinc	24	15
Copper	10	1½
Zinc	10	48
Copper	45	2½
Zinc	25	38
Copper	25	10
Zinc	20	35
At this point current reversed quickly, about 50 times.		
Copper	40	17
Zinc	30	35

In this table the gradual increase of the fault is very marked; the loss with a zinc current increasing, instead of diminishing, with continued electrification. Nevertheless, when the zinc current is continued for a considerable time, the loss diminishes under its action. Even at the end of this experiment, the resistance of the fault was very considerable, but I have no reason to believe that the deterioration had ceased.

With 80 elements I could not produce these marked

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effects of deterioration on a similar fault; but these experiments were neither long nor very carefully conducted.

I have made no experiments on faults described as Nos. 3, 4, or 5, in the enumeration of various kinds of faults. Electrolysis may be feared where gutta-percha is thin. Pores may increase in size, and gutta-percha may be melted or decomposed round a foreign body.

Accident put me in possession of a fault caused by an air bubble, which, I think, throws light on many recent failures of cables which tested well when first laid down. I have already had occasion to mention this fault, which had a resistance of about 14×10^{12} Thomson's units, when first discovered. I traced this fault, testing with 80 Daniell's elements until I ascertained its position in the gutta-percha to within one inch. There were signs that the gutta-percha had been heated in the journals during manufacture. Nevertheless, I was quite unable to find any visible flaw in the gutta-percha. A little white speck looked suspicious, but on wiping it away I could see no hole. During the tests (in fresh water) the fault got a little worse, but not much, except perhaps for an instant, on the first admission of a negative current. The current was continually reversed during the tests. I put the fault into a wine glass of salt water and tested it once more (with a negative current). I at once saw a little row of bubbles rise from the spot where the white mark had been. I took the fault out of the water almost immediately, and a little hole could now be distinctly seen. I replaced the fault in the wine-glass, putting the bulb of a thermometer close to the little hole. Bubbles escaped rapidly from the fault, the mercury in the thermometer rose 5°, and in three minutes the fault had lost almost all resistance. A positive current as well as a negative current was used. The hole was now $\frac{1}{4}$ in. long, and $\frac{1}{16}$th broad. A hollow, extending to some distance on each side, indicated the presence of an air-bubble. A second hole had began to form a quarter of an inch off from the same cavity. The copper was visible, well placed in the centre of the gutta-percha. The sudden and complete opening burnt in the gutta-percha in this case was obviously due to the body of water held in the bubble; after the slight fault in the outer skin, (caused probably by a heated journal) had increased to a certain point, this water was so heated by the passage of the current from the internal wire to the water outside, as to melt the surrounding gutta-percha. The transition from fresh to salt water, probably made the action more sudden, but would not, I think, change the sequence of facts. With such a fault as this, 80 cells are, therefore, sufficient to destroy a cable. Nevertheless, the deterioration, had the cable been laid, would not have been immediate. Probably in the two days during which I tested to find the exact position of the fault, it was subjected to as severe a trial as would be caused by a month's signalling, where reverse currents were not used. Nevertheless, after a certain time, in a few minutes, the cable would have been rendered useless.

It seems very probable that the failure of some sections of the Red Sea cable, and of the cables belonging to the Mediterranean Extension Company, all of which tested well and worked well for some time, may have arisen from similar faults, burnt by the use of too powerful a battery. I certainly do not think these failures need be attributed to mysterious or unknown causes. I think these failures are either due to lightning, mechanical injury, or to the effects of powerful batteries on faults too small to be at first detected. These causes of failure are easily avoided.

I do not, however, wish to draw any forced conclusions either from the theories or facts which I have advanced, but I certainly consider it proved that a cable which tests well may contain small faults. That these faults may not injure the working of the line for a considerable time, and that they may be almost suddenly converted into faults of fatal magnitude by the action of a battery of 80 elements.

I can also hardly doubt that in such cables, the use of very small battery power might indefinitely adjourn their deterioration. I will not venture to name any exact limit to the number of plates which may be safely used, until after much more extended experiments.

Meanwhile, I consider it established, that the use of reverse currents is much to be deprecated, especially as no increased speed in signalling is gained by their use.

I am at present inclined to consider the use of negative currents only to be the safest means of preserving a sound cable, on account of the danger of a solution of continuity in the conductor, which may be caused by the use of positive currents.

I would submit, that instead of testing cables when they are manufactured in great lengths, they should be tested in as short lengths as possible. Certainly not more than 100 knots should be tested at once.

I would also suggest that each length should be as nearly alike as possible, and subject to as nearly as possible the same conditions when tested.

A slight difference in the quality of gutta-percha, or in the quantity of Chatterton's compound, may make the specific resistance of different cables differ considerably; but when two equal lengths of the same cable, covered by the same water, test differently, one of the two will probably be found to contain a fault. The test is, moreover, so simple and obvious that it is easily made even by unskilled electricians.

I beg to suggest that all cables over which the Government has any control, be tested by an electrician named by the Government, both before they are laid, immediately after they are laid, and at stated intervals after their submersion, and the results should be published in every case; it would thus soon be known whether cables really deteriorate or not. The Government and the public would then also be able to judge what reliance should be placed on the continued action of the several cables. Directors and possibly shareholders may oppose this publicity, which I believe on the contrary to be the true interest of all. If there are difficulties, let them be known, and they will be overcome; conceal them, and for the sake of a temporary advantage, lasting mistrust, only too well founded, will be the result.

Let every one who has the opportunity, make as many, and as continuous experiments as possible, upon all manner of faults, in all conceivable conditions; it is only by learning all possible causes of failure that we shall learn how to prevent failure.

Lastly, now that there is a strong feeling against the further use of gutta-percha, and in favour of the adoption of india-rubber as an insulator, I wish to call attention to the fact, that we do begin to know something of the defects of gutta-percha, and how to prevent their evil consequences, whereas probably many years will pass before we shall have gained the same knowledge of india-rubber, which doubtless has not some of the defects which we know of in gutta-percha, but probably has others, unfortunately unknown as yet, which may cost as much as has been wasted already in consequence of these unhappy "faults in gutta-percha."

FLEEMING JENKIN.

Captain D. Galton, R.E.,
&c. &c.

Adjourned to Friday, the 30th instant, at Two o'clock.

Thursday, 5th January 1860.

PRESENT:

Captain DOUGLAS GALTON.	Professor WHEATSTONE.
Mr. G. P. BIDDER.	Mr. EDWIN CLARK.
Mr. VARLEY.	Mr. SAWARD.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

C. F. Varley, Esq.

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CROMWELL FLEETWOOD VARLEY, Esq., a Member of the Committee, examined.

2891. (*Chairman.*) You are an electrical engineer, I believe?—I am.

2892. You have devoted considerable attention to the science of electricity?—Yes.

2893. Are you in the employment of the Electric Telegraph Company?—Yes, and have been since its formation, 13 years ago.

2894. Are you also the electrician of the Electric

and International Telegraph Company, and the Atlantic Telegraph Company ?—Yes.

2895. Have not you had considerable experience with the underground wires through the streets of London ?—Yes ; when the Electric Telegraph Company was first formed they had to extend their line wires which terminated at the Nine Elms station on to their office at the Strand and also to the Admiralty. Those wires, for gutta-percha was then unknown, were insulated by means of cotton ; the wires were covered twice with cotton, and nine of them were placed inside a leaden tube. They were then saturated, by boiling in a cauldron, with a mixture of Stockholm tar, bees'-wax, pitch, and a little resin.

2896. In what year was that ?—1846-7 ; those wires were manufactured in lengths of 50 yards, and the lead tubing was split open at every five yards, in order that the mixture might readily penetrate. The wires so prepared were then boiled for various periods, and subsequent experience showed us, that unless the materials were boiled for 24 hours, the insulation was not good. Those pieces which were boiled for 24 hours insulated extremely well ; and with some of them I succeeded in obtaining an inductive charge, that is to say, I was enabled to make contact between the battery and one of the wires, remove the battery, and a quarter of a second afterwards get a very decided discharge from the wire to the earth, by applying my tongue to it, my body forming the connexion between them. When the mixture had been boiled for 12 hours, it was soft, and in that state never insulated. Those wires gave us a great deal of trouble, owing to the greater part of them not insulating perfectly ; and the slits in the tubes, which ought to have been carefully soldered up, were never so well united as to exclude moisture ; after they had been down a certain period, those wires regularly required repair, excepting some few lengths, which stood for several years without giving way.

2897. Were similar wires laid in tunnels ?—Similar wires were laid in tunnels until india-rubber and gutta-percha were introduced.

2898. How did you test those wires ?—The wires in the leaden tubes were never put under water, because very few of the lengths would bear that process. In most instances, the lead tubing was porous, and not sufficiently well made to exclude the water ; consequently we took every care in using those wires to put them again into iron pipes, and to keep them as dry as possible. For that purpose, syphons were placed at the lowest parts of the iron pipes, and the leaden cables themselves were wrapped round with coir yarn, which was pitched over, so that any moisture getting into the iron should not readily get to the lead.

2899. What was the thickness of the lead ?—It was ordinary half-inch lead tube ; the lead itself was about the sixteenth of an inch. After the wires had been inserted into the lead tubing it was passed through a die, so as to compress it on to the wires and leave as little space as possible.

2900. Did you observe any peculiarities with those wires ?—They were very liable to get out of order. Scarcely a mile length could be put down, but one of the wires would fail from a little piece of the solder running through the thin insulating coating formed of two layers of cotton to it, while joining the lead tube. Those faults gave us a great deal of trouble. After laying down the first length which connected the office at the Strand with the Admiralty, I went on to the London and North Western Railway, and there ran the wires from Euston Square Station up to the middle of the Primrose Hill tunnel ; and when I had got there, I was recalled to attend to the wires which were being extended from the Strand office to the new office at Lothbury. The office at Lothbury had to be opened by Christmas; and it was very clear, at the rate at which the men were then getting on with the work, that they would not have it completed in sufficient time. This arose from their having employed a plumber, instead of a tinman, who made plumber's joints, which were porous and admitted the water so fast that he never could get more than a mile and a quarter from the Strand station before his wires were bad, and he had to come back to cut out the faults. The wires became defective so frequently that I was led to endeavour to discover some means whereby the localities of the faults might be ascertained without resorting to the expensive and tedious process of successive subdivision until the fault was traced out ; after a little experimentation, I contrived 9 times out of 10 to cut out the identical joint containing the fault ; the instruments used being a battery, a galvanometer whose resistance I had ascertained, and the street wires, of which we had 27. I regularly used that system of testing, and got the work by that means completed in a much shorter period than would otherwise have been the case; in fact, I do not think we should ever have got to the new office, had not some such means been adopted to obviate the necessity of destroying so much work as we were obliged to do every time a fault appeared. There was one peculiarity I should mention in those wires, which is important, inasmuch as it throws a good deal of light upon the causes of defects in cables. One of the wires in this underground system, from the Lothbury station to the Euston Office, and in connexion with Manchester, was used for a Baines printing circuit, in which the currents were all sent in one direction, positive currents being used; the other wires were all used with the needle instrument, in which the currents were reversed. This one wire always got better from being worked on, and the others always got worse; the wires that were not in use very frequently remaining for months stationary. I tried experiments by shifting the Baines printing circuit on to different wires, and invariably got the same result: the wire improved. I traced this improvement of insulation by positive currents to the following cause :—Positive currents, when the leakage or defect is caused by moisture, have a powerful tendency to expel the damp ; negative currents attract it, and thus, independently of the oxidation of wire by being made (+) plus to the earth, the positive currents remove the moisture which causes the loss of electricity ; this effect only takes place where the moisture is in very small quantity. However, comparatively speaking, a very short contact with the negative pole of the battery would bring back the wire to its original state of defective insulation.

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2901. In what year did you introduce gutta-percha or india-rubber covered wire?—Gutta-percha covered wire was first used in the streets of London in 1849, and we immediately found an immense improvement in our wires. The wires that were then introduced were mostly iron wires covered with gutta-percha. In many cases, there were three wires placed in a solid cylinder of gutta-percha. This was very objectionable, because, when we uncoiled them, one of the three iron wires would almost invariably knuckle out through the gutta-percha, and ruin it. When copper wire was used, this casualty did not take place to so great an extent; but subsequently it was found necessary in all cases to use a single wire in each rod of gutta-percha. Those gutta-percha cylinders containing three wires were made into coils a yard in diameter. The conducting wires were of No. 16 wire gauge.

2902. Were the three wires three separate conductors ?—Yes, inside one rod of gutta-percha, about half an inch in diameter.

2903. Were they separate wires insulated in one gutta-percha tube ?—The gutta-percha was run out from the machine simultaneously with the three wires, which were thus insulated from each other.

2904. Have you observed any peculiarities in the gutta-percha wires ?—Gutta-percha wires exhibit a great many peculiarities. One of the first things to be noticed in gutta-percha is, that it is very liable to crack when it is exposed either to light or to heat. At the mouths of tunnels it is almost impossible to preserve the wires, even by covering them with Stockholm tar

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and tape, a process introduced by Mr. Edwin Clark, and which is one of the best preservatives as yet found for gutta-percha; even, in spite of this preparation, we find it very difficult to preserve the wires at the ends of tunnels, where they are exposed to wet and dry alternately, and to light. In such places where the wire is bare it cracks all over, and if the wire be uncovered from the gutta-percha, opposite each crack will be found a little indentation etched in by the electric current. I have seen pieces with from 200 to 300 such etched marks in a foot, where the gutta-percha has been much cracked; this was particularly observed at the mouth of the Standage tunnel, between Diggle and Huddersfield, in some wires erected by the late Mr. Highton. The gutta-percha was not protected from the weather, and at certain seasons of the year the wires were constantly wet, at other seasons dry. I should add that these wires were some of the earliest manufactured, and were erected before the action of wet and dry and of light had been discovered. I have here a specimen very recently taken from one of our continental circuits. It is a piece of ordinary gutta-percha covered wire, which has been injured by the earthen tubes in which it was contained, being crushed on it. The water which occasionally got to this fault formed a connexion between the wire and the earth, but not of such magnitude as to suspend our working; in fact, it was not noticed for a long while, until the copper wire was eaten in half. If you examine this fault you will find that the two ends of the copper wire are at least a quarter of an inch asunder, and one day the line suddenly stopped its communication; it had been working badly for a few days, and to test for this fault, I had a positive current put on from 120 cells at Amsterdam. I found that the current came very irregularly at first, and ultimately almost entirely ceased; I then put on powerful negative currents at the London end, when the positive current from Amsterdam almost immediately reappeared for a short time, about three or four seconds, and then disappeared again. The positive current repelled the moisture from the fault and the negative current attracted it. I tried the following experiment, and found it successful. I had the currents reversed at Amsterdam, so that we produced the marks with the negative current instead of the positive, using a weak positive current to discharge the wire; in that way, having a predominance of the negative current, sufficient moisture was attracted into the fault to bridge over the interval between the ends of the wire, and thus re-establish the circuit. In this way the line was worked with almost no interruption for many days, until the line was repaired. I have adopted this expedient on several occasions with success. It can only be used where the fault is damp, but not flooded with moisture. We have now a similar fault on one of the wires, and have adopted the same means; we have been working through it for the last three or four days, but have not yet traced out the locality of the fault. I have seen many instances in which gutta-percha covered wire has been eaten through. The first case I noticed was in the London street work in 1848 or 1849, but its cause was not then traced out; but the true nature of these faults was made apparent in 1851, in the Kilsby tunnel, where a very similar fault was found in a defective joint. The electric current rendering the exposed wire positive to the earth, electrolytic decomposition of the water, the salts it contains, and the wire takes place, carbonate and oxide of copper being produced until the wire is eaten entirely away. In the majority of instances these faults are at the joints.

2905. In the two cases of subterranean wires that you have mentioned, was the copper entirely destroyed?—Yes; one fault was taken out between Woodbridge and Melton; the one that I found in 1851 was taken out of the Kilsby tunnel, and there is a fault we have now, whose locality is not yet discovered, and which presents all the indications that that fault did, and there is no doubt whatever that it is a fault of that kind.

2906. (*Mr. Bidder.*) What circuit is it?—It is one of the wires connected with the Zandvoord cable, somewhere between Colchester and Ipswich.

2907. (*Chairman.*) You have devoted considerable attention to the working of long submarine and subterranean lines, have you not?—Yes, I have arranged an apparatus by which the speed of working through those lines is rendered much more constant and regular than is possible with the ordinary Morse instrument. The peculiarity of my apparatus is chiefly that the currents into the cable, instead of being a continual succession of currents of the same name in one direction only, as in the Morse and the Baines systems, are alternated to produce the signal. The effect of induction in the cable is to produce great irregularity in the signals received at the distant end, as has been well explained already before the Committee. If at one end of a long submarine line a succession of positive currents be sent in, at the distant end will be received an almost continuous current; but if the currents be reversed by using the battery to discharge the cable instead of leaving the charge in the cable to discharge itself, more rapid signals can be produced at the distant end. A second peculiarity is this: in the Morse instrument and in instruments of that character a relay is used which consists of an electro-magnet attracting an armature of soft iron, this armature being retained back by means of a spring or a permanent magnet; therefore when the contact with the battery is made at the sending end this armature does not move until the current has reached a sufficient strength to overcome that force. Another peculiarity of my system is that the line is put to earth after each signal, and before the reversal of the current; this gives a much higher speed. My apparatus are less affected by variation of power, because the current itself regulates the relay. The currents are never continuous, the batteries are continually changing and the state of the insulation of the circuit is continually varying.

2908. (*Professor Wheatstone.*) They are never constant?—No.

In my apparatus the negative current is used instead of a spring; and consequently if the insulation of the line vary, or if the strength of the battery vary, so does the regulating force of the relay (which is the negative electric current) vary, therefore the machine is self-regulating. On the Continent the Morse system is regularly used, and in England on the lines of the Electric Telegraph Company my system is almost invariably used. If you go into one of the Continental offices you will find the manipulators have one hand continually regulating the relay spring. The iron core of the relay magnet is another and a considerable source of irregularity, when the currents all flow in one direction. The permanent or residual magnetism of the iron core does not reach its maximum with the first current, but gradually augments during the working, and then requires a greater force to cause the armature to retire when the line current ceases to flow; this requires a different set of the regulating screw, in order that the marks shall appear correctly on the Morse instrument; that is done with great skill by the manipulators, as those slips you have show, in which some thirteen or more such instruments were in circuit. One slip was written in London, in the Electric and International Telegraph Company's office, by a clerk in Odessa, the current being renewed by the translators not less than 13 times between Odessa and London. It was sent round from Odessa through Moscow, St. Petersburg, Riga, Königsburg, Berlin, Hanover, and Amsterdam to London. The highest speed that we have actually attained between Odessa and London is between five and six words per minute—we have taken off some twelve or fifteen messages

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from Odessa direct in London, and have sent rather more from London to Odessa. I may mention a little conversation which took place a few weeks ago between London and Odessa. London first called Berlin, inquired about the weather, and Berlin said, "It is very cold, have you had any frost in London?" We said, "No." We then asked for St. Petersburg. In about three minutes we were told to call. St. Petersburg answered, and we put the same question to him. He replied that it was very cold, and the snow was so deep that they were using the sledges. We then stated that we had messages for Odessa, and asked for direct communication. He put us through to Odessa, we sent our messages, and he immediately afterwards asked us, "What weather have you in London?" We informed him, and put the same question to him, he stated that they had had no frost in Odessa, and that the flowers were in full bloom. Thus we had spoken through Berlin where, it was freezing hard, through St. Petersburg where it was colder still, back again into a warm climate in one continuous uninterrupted chain. I should add that these relays constantly require the attention of the clerk, it is one of the chief things that he has to learn. Two years ago the Prussian Government sent over a commissioner to examine our system of working; he was himself a practical telegraphist, and could manipulate the apparatus very well. He went to Edinburgh and examined the working direct from Edinburgh to London, and wishing to satisfy himself that he was speaking direct to London, he asked for one of the German clerks to be brought to the instrument to speak to him in his own language. A German clerk in London was brought to the instrument, and he communicated with him. Having finished his communication, he inquired for the relay, to regulate it while receiving the reply. The relay was pointed out to him on a shelf some three yards distant from the apparatus, there being no room in that limited office to place it nearer, he was surprised to find that it was not necessary to regulate it to work with London. He asked the instrument clerk how frequently he had to adjust these relays, who stated that it was adjusted once in the morning about ten o'clock and seldom ever again throughout the day.

Where the lines are shorter, and not more than 200 miles in length, the apparatus so nearly compensate themselves that we seldom have to adjust them even for weeks together; by this means we carry on our communications at a much higher rate, because we can work nearer to the limit of speed.

2909. Do you mean by "the limit of speed" what the manipulators can send?—No; I mean the limit of the apparatus itself. The limit of speed in a telegraph line depends upon the rapidity with which the wave flows through that line, and also upon the apparatus itself. In an over-ground circuit, such as that from London to Edinburgh, in which there is but a small amount of gutta-percha covered wire, the wave travels so rapidly that it may be almost entirely neglected; but apparatus such as will work without giving too much trouble must be rather heavy, to possess sufficient force to close and open the local circuit with precision. Such apparatus have not a very high rate of speed, they will not exceed 35 words per minute through a line of 200 miles of No. 8 iron wire unless exorbitant battery power be used. In practice we are obliged to allow a very large margin, because you never can find telegraphists who are, in the first instance, sufficiently expert to keep the apparatus adjusted to the highest pitch while receiving and writing a communication; we are, therefore, obliged to allow a very large margin, in order that the instrument shall work throughout the day without any material interruption. In practice we seldom work on the English lines at a higher rate than 22 words per minute. An entire hour's work will seldom show a higher speed than 12 or 15 words per minute. The average length of words is $4\frac{1}{2}$ to 5 letters, but the public, when telegraphing, often suppress the "a's," the "of's," the "the's," the "in's," &c., and thus telegraphic words average of a greater length, and very often exceed 5 letters, especially when such words as the following occur in the message, "Dampfschifffahrtgesellschaft," which, according to telegraphic law, is a single word all over Europe. In working through a line in which you are limited by the rapidity of the wave; for instance, in an underground wire of considerable length, you ought to take not more than one-third of the attainable speed as the available speed for communication. There are very many sources of interruption to telegraph lines; first of all, the batteries at times fail; the apparatus get out of order from wear or breakage; the wires connected with the apparatus break; the apparatus of the receiving station require re-adjustment; and where Morse instruments are used the burning of the contact points in the relay makes them rough and liable to adhere, when the contact parts (made of gold or platinum) must have their surfaces renewed by being filed or cut with a knife. There are many other small sources of irregularity, which, when put together, make a rather large total. I am not now speaking of the interruptions that arise from the currents leaking from one line to another, or from defective insulation; I am speaking simply of interruptions that are peculiar to the apparatus themselves. Another source of interruption, which is a serious one, is that the clerks at the instruments sometimes get out of temper. I have noticed that telegraph working generally causes great nervous irritation, and the clerks are very prone to quarrel. This is particularly the case with those circuits which are difficult to work, from the above numerated causes. The quarrelling is a thing we cannot entirely check, and it is a source of reduction in the available speed of telegraphic lines. If a clerk be thoughtless, and do not "key" very accurately, and so cause one or two words in a message to come indistinctly, the clerk at the distant end, after this has been repeated two or three times, will frequently become so exeited as to refuse to work; quarrelling commences, which ends frequently in serious delay to the working of the line. We do all we can to check this, but cannot entirely prevent it. These are some of the elements which enter into the calculation as to the availability of a line for commercial purposes, and taking them, together with the other sources of interruption, you ought not to allow more than one-third or one-fourth of the attainable speed through a cable for its practical capacity, and to maintain this throughout the 24 hours would be a hard task for the most expert hands.

2910. Either subterranean or submarine?—Yes, either. In long submarine lines we are very much bothered at certain periods by the earth currents. There are currents continually flowing about the earth either in one direction or the other throughout the day. These currents seem to reach a maximum somewhere about 2.40 p.m. They are evidently connected with the rotation of the earth on its axis, and I will draw attention to a curious coincidence or analogy between them and the tides. The moon goes round the earth once in about 29 days, the earth rotates once in 24 hours; the tide reaches its maximum about three days after new moon or full moon; these currents reach their maximum (not three hours) but two hours forty minutes after noon and after midnight; they are rather stronger, I think, at noon than at midnight. They agree with the magnetical variations recorded at Greenwich, and are doubtless in intimate connexion with them.

2911. Do you think the temperature has anything to do with earth currents?—I can offer at present no very distinct clue as to their cause, but I expect it has to do with the temperature, and is connected, I imagine, with the expansion of the atmosphere, removing as it were by expansion the oxygen from the surface of the earth, so disturbing the magnetism in the earth. I imagine that that may be the cause, but this is little more than conjecture; the sun probably radiates more forces than are at present known to us,

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2912. (*Professor Wheatstone.*) Have you kept any accurate register of the observations of earth currents, or reduced them to curves at any time?—No: it is impossible to do it with telegraphic lines; the currents on such lines are so weak, and the amount of current that we get by induction and leakage from one line to another is so great in proportion to the daily currents, that it is impossible to record them accurately, added to that, our lines are seldom free enough, or those who could take the observations free enough to take them. The only way to take the earth currents, is by establishing, at the different observatories, lines on purpose. I have communicated with Professor Airey on the subject, and pointed out to him the means by which, on wires a mile or even half-a-mile in length, distinct records of earth currents could be obtained. The second source of disturbance is a very serious one, that is, from what are sometimes termed magnetic storms. When magnetic storms are observed at Greenwich, or the aurora borealis is about, we invariably have very powerful currents, flowing sometimes in one direction and sometimes in another through the wires. Generally speaking, these currents change from positive to negative very rapidly, sometimes in the space of a small fraction of a second, but more generally in half a minute or a minute, and sometimes longer.

2913. (*Mr. Saward.*) When you say flowing through wires, do you mean a cable?—Through conductors of any description, whether over-ground or under-ground. I should mention that we are never able to devote very much time to the observation of these "magnetic storms," owing to having to exert ourselves to the utmost to get the Company's business through when the storm temporarily ceases, and to making such arrangements as will neutralize as much as possible their mischievous effects. The plan which I adopt, wherever we are able to do so, is to take off the earth from one line and substitute a return wire for the earth, when the currents which are generated in one wire neutralize those generated in the other, and we can work them without any material obstruction. But as the insulation of no two wires is exactly alike, we still have currents, only to a much less extent. I have collected a great many facts connected with these currents, more with respect to the direction of them in different parts of England, and find that there is no general line across England for them. The line from London to Ipswich is very powerfully affected; the lines from Hull to Leeds and Leeds to Manchester are lines powerfully affected; the line from London to King's Lynn is a line that is very frequently almost neutral; while going still further north, take the line from London to Newcastle, the currents flow in the same direction as those through the London and Ipswich circuit, which is on the other side or south of the London to King's Lynn line, which is nearly neutral. This shows that the neutral line, or the line of equal electric tension, is a curved one. I have traced out some of these currents, and they seem to be influenced by the configuration of the island. It would seem that the soil is a less good conductor than the ocean, and that the currents that are flowing through the ocean upon coming to the land meet with a check; and the very powerful currents observed on the short lines—London and Ipswich, Liverpool and Leeds, Liverpool and Hull, and Manchester and Leeds,—are partly due to the indented form of the coast at Ipswich, Liverpool, and Hull.

2914. (*Chairman.*) Is the line from London to Bristol affected by earth currents?—The line from London to Bristol is very frequently powerfully affected, but it is also frequently a neutral line, showing almost no disturbance. The line from Portsmouth to Southampton is very frequently neutral. I will give some diagrams showing the direction of the currents at different times, which will do more than any verbal explanation I can give. These earth currents or "magnetic storms", caused serious inconvenience last year, and on one day caused considerable excitement at the Stock Exchange, the telegraph communication being almost suspended. During the last autumn I succeeded in measuring some of the currents between London and Ipswich, and had to introduce an opposing battery of 140 cells of Daniell's battery to balance the current, showing therefore in the earth at Ipswich, compared with the earth in London, a difference of electric tension, amounting to 140 cells of Daniell's battery. Taking this line, roughly speaking, at 60 miles, and assuming that the same state of tension existed all the way across the Atlantic for a cable of 2,000 miles, you would have a current of very great power flowing through that line, equal to from 4,000 to 5,000 cells of Daniell's battery, which force would be extremely dangerous and might destroy the insulation of such a line; but on comparing the currents received in London from Ipswich with those received from Amsterdam, *viâ* Ipswich, I found that the latter were, comparatively speaking, but little stronger than those received from Ipswich, and that a return wire between London and Ipswich was nearly sufficient to neutralize the effects of the "magnetic storm." I therefore think that in the ocean itself the tension is very much lower than what we experience on land, and on that account the fears that I first entertained as to the durability of an Atlantic cable, on account of these earth currents, are almost entirely allayed.

2915. You have always advocated the use of large conductors as the only practical means of obtaining rapid and certain communication through long lines, I believe; will you state your views on that subject?—When the Electric Telegraph Company was first established the insulation adopted was very imperfect; firstly, from the form of insulator used; and secondly, from the quality. The result was that whenever a shower of rain came on we were unable to work through our long lines, and seldom could get through more than 100 miles. Upon consideration it soon became apparent that, whatever were the dimensions of the conductor, the size and quality of the insulator might remain very nearly the same; but by increasing the size of the conductor, the amount of current leaving the original station from a given electro-motive force is increased in nearly like proportion, and it is easy to show that in that way the amount of current subtracted by defective insulation will decrease in proportion to the current supplied, in a very rapid ratio, when the amount of loss is considerable. For instance, in a theoretical line of, say, 100 miles in length, with five supports or insulators, each having a leakage equal to 100 miles of line in resistance, the amount of current passing through that line would be reduced from 7 to 1; but were a wire of twice the diameter used, nearly four times as much electricity would leave the battery, and at the distant end the current received would be eight instead of one. From this consideration it was immediately apparent that great gain would arise from the use of wires of larger diameter in overcoming all those obstacles. With regard to underground wires, it is very clearly evident that the loss of speed in transmission is due simply to the increased induction to which those wires are subjected by being placed so near the earth. In an aerial line the wire is from ten to 14 feet from the earth, but in an underground line the gutta-percha covered wire is only a small fraction of an inch from it; gutta-percha has a much higher inductive capacity than air, consequently the induction round that wire is very much greater than in an air line; and it is to this induction alone that the decrease of speed can be attributed, because, if a coil of gutta-percha wire be suspended in the air, it will be immediately found to have almost no induction; but the moment it is put into water that induction increases. From these and many other considerations it was easily shown that the speed decreases as the inductive absorption increases; and, therefore, so long as the conducting power, and the inductive power, if I may use that term, maintained the same ratio, the speed of a given line will be almost the

same; but vary either of these, and the speed varies in like proportion. I have frequently proved that the conducting power of a wire is exactly in proportion to its area. Increase the conducting power of the line you increase the speed—decrease the conducting power of the line and you decrease the speed, the inductive absorption remaining the same. By increasing the diameter of the wire and the thickness of the gutta-percha you increase the conducting power as the square of the diameter of the wire; that is, as the area of the wire; the surface is simply increased as the diameter. If now this wire be coated with insulating material to such a thickness as shall give the same induction only as the former wire, you will have four times the speed. I tried a number of experiments in 1854, and obtained the following law. I will read a short article which I wrote with a view to publication:—

LAW of INDUCTION *in* TELEGRAPH CONDUCTORS.

In 1854 I commenced a series of experiments to determine what effect thickness of insulating material had upon the inductive action of cylindrical conductors, covered with a dielectric (or insulating) medium, such as telegraph wires coated with gutta-percha.

For this purpose glass tubes, filled with mercury and coated externally with tin foil, one end of each of the tubes being hermetically sealed; steel wires placed inside brass tubes, and the latter filled with lac or rosin; also plates of dielectric substance and sheets of tin-foil were used. Having first by experiment confirmed the statement that between equal surfaces inductive "absorption" was inversely as the distance between the inductive surfaces; *i.e.*, if with like tension of electric force, and like surfaces placed at a distance, *d*, from each other, the amount of electricity absorbed by the one plate (and driven out of the other to an equal amount) by induction be represented by *e*, then,—

A distance between flat equal surfaces	d	gives electricity absorbed	e	
„	$2d$	„	$\frac{e}{2}$	
„	$3d$	„	$\frac{e}{3}$	
and „	nd	„	$\frac{e}{n}$	

In the case of cylindrical conductors, such as telegraphic wires, the outer surface of the dielectric is greater than the inner surface, the surface of the conductor; and experiment showed, as expected, that with a distance, *d*, between the inner and outer surfaces, the electric tension and the inner surface being the same as in the preceding experiment, the amount of electricity absorbed by induction was more than *e*, and did not decrease in the inverse ratio of the distance between the surfaces, but in accordance with the following law:—

Let the external surface of the conducting wire be - *C*,
the „ gutta-percha, or other insulator be - - - - - - *G*,
and the distance between the inner and outer surfaces of the gutta-percha be - - - - *D*;

then,

If with a distance, *D*, the electric absorption be *E*, to halve this amount to $\frac{E}{2}$ another layer of dielectric must be added to the former, whose thickness is as much greater than *D*, as *G* is to *C*; *i.e.* to reduce the inductive capacity of a wire from *E* to $\frac{E}{2}$ you must have a thickness of dielectric $= D + D\frac{G}{C}$, and so on;

or,

If a thickness, D, give electricity absorbed by induction E

„ $D + D\frac{G}{C}$ „ „ - $\frac{E}{2}$

„ $D + D\frac{G}{C} + D\frac{G^2}{C^2}$ „ „ - $\frac{E}{3}$

„ $D + D\frac{G}{C} + D\frac{G^2}{C^2} + D\frac{G}{C}$ - $\frac{E}{4}$

or,

Representing the thickness of the first layer by unity, we get:—

If a thickness 1 give electricity absorbed by induction E

„ $1 + \frac{G}{C}$ „ „ - $\frac{E}{2}$

„ $1 + \frac{G}{C} + \left(\frac{G}{C}\right)$ „ „ - $\frac{E}{3}$

„ $1 + \frac{G}{C} + \left(\frac{G}{C}\right)^2 + \left(\frac{G}{C}\right)^3$ „ - $\frac{E}{4}$

and so on to $\left(\frac{G}{C}\right)^n$

it being necessary to greatly increase the thickness of the outer layers to diminish materially the inductive effect. From this formula I calculated the attached table, in which the conductor's semi-diameter is 100, and the first layer of insulating material is 1.

I have carried on the table to include 350 layers, each of such progressive thickness, that each shall give a like amount of inductive capacity. These layers of equal induction are found in column No. 2, and their sum in column No. 3.

For ease of expression, I have supposed that the layer whose depth is $\frac{1}{100}$ of the diameter of the conductor offers a certain amount of resistance to the absorption of electricity by induction, when the conductor is coated with two layers of equal induction there will be twice as much resistance offered to the absorption of electricity by induction and so on.

By this table it will be seen that so long as the diameter of the conductor and the insulator preserve the same ratio the one to the other, the inductive capacity of the covered wire will remain the same independently of the magnitudes of *C* and *G*.

The first column indicates the number of layers of equal inductive capacity or resistance.

The second column shows the thickness of each layer.

The third column shows the sum of these, that is, the depth of insulating material, the diameter of the conductor being 200, the radius = 100.

The same table applies to the insulating power of different depths of insulator, here the first column indicates the No. of layers of equal resistance, the second column the thickness of each layer of equal resistance, the third column gives the sum of these, *i.e.* the thickness necessary to give a given amount of resistance or insulation, the diameter of the conductor being 200.

Here is the table used in my calculations based upon the foregoing law:—

The semi-diameter of the conductor or radius is divided into 100 parts; the first layer of "equal induction" is = 1; consequently the interior surface of this layer is to: the outer surface of it as 100 to 101; being so nearly alike, the inductive power of this layer may, without any appreciable error, be considered as equal to a flat plate whose surfaces is = to 100·5. I have verified this law with gutta-percha covered wires of various dimensions.

Column No. 1. (1) is the inductive resistance (*i*).

Column No. 2. represents the thicknesses of the gutta-percha layers, to increase the inductive resistance (reduce the inductive capacity) an equal amount = (layers of equal induction).

Column No. 3. the sum of those layers; the semi-diameter or radius of conductor being 100.

Thus, if a wire whose radius is 100 have a covering of gutta-percha whose thickness is equal to 302·7, its inductive resistance, and also insulation, will be twice as great as a similar wire covered to only 100·7.

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VARLEY'S INDUCTION AND INSULATION TABLE.

1.	2.	3.	1.	2.	3.
1	1·000	1·000000	92	2·437	149·785
2	1·010	2·010000	93	2·498	152·283
3	1·020	3·030100	94	2·523	154·806
4	1·030	4·060401	95	2·548	157·354
5	1·041	5·101005	96	2·573	159·927
6	1·051	6·152015	97	2·599	162·526
7	1·061	7·213535	98	2·625	165·152
8	1·072	8·285670	99	2·651	167·803
9	1·083	9·368526	100	2.678	170·481
10	1·094	10·462211	101	2·705	173·186
11	1·105	11·566833	102	2·732	175·918
12	1·116	12·682501	103	2·759	178·677
13	1·127	13·809326	104	2·787	181·464
14	1·138	14·947419	105	2·815	184·279
15	1·149	16·096893	106	2·843	187·121
16	1·161	17·257862	107	2·871	189·123
17	1·172	18·430441	108	2·900	192·892
18	1·184	19·614746	109	2·929	195·821
19	1·196	20·810894	110	2·958	198·780
20	1·208	22·019003	111	2·988	201·767
21	1·220	23·23919	112	3·018	204·785
22	1·232	24·47158	113	3·048	207·833
23	1·245	25·71630	114	3·078	210·911
24	1·257	26·97346	115	3·109	214·020
25	1·270	28·24320	116	3·140	217·161
26	1·282	29·52563	117	3·172	220·332
27	1·295	30·82089	118	3·203	223·536
28	1·308	32·12909	119	3·235	226·771
29	1·321	33·45038	120	3·268	230·039
30	1·334	34·78489	121	3·300	233·339
31	1·348	36·13274	122	3·333	236·672
32	1·361	37·49407	123	3·366	240·039
33	1·375	38·86901	124	3·400	243·439
34	1·389	40·25769	125	3·434	246·874
35	1·403	41·66027	126	3·468	250·343
36	1·417	43·07687	127	3·503	253·846
37	1·431	44·50764	128	3·538	257·384
38	1·445	45·95272	129	3·573	260·958
39	1·459	47·41224	130	3·609	264·568
40	1·474	48·88637	131	3·645	268·213
41	1·489	50·3752	132	3·682	271·895
42	1·504	51·8789	133	3·718	275·615
43	1·519	53·3977	134	3·756	279·371
44	1·534	54·9317	135	3·793	283·165
45	1·580	56·4810	136	3·831	286·996
46	1·565	58·0459	137	3·869	290·866
47	1·580	59·6263	138	3·908	294·775
48	1·596	61·2226	139	3·947	298·723
49	1·612	62·8348	140	3·987	302·710
50	1·628	64·4632	141	4·027	306·737
51	1·645	66·1078	142	4·067	310·804
52	1·661	67·7689	143	4·108	314·912
53	1·678	69·4466	144	4·149	319·061
54	1·694	71·1410	145	4·190	323·352
55	1·711	72·8524	146	4·232	327·485
56	1·729	74·5810	147	4·274	331·759
57	1·746	76·3268	148	4·317	336·077
58	1·763	78·0900	149	4·360	340·438
59	1·781	79·8710	150	4·404	344·843
60	1·799	81·6697	151	4·448	349·291
61	1·817	83·4863	152	4·493	353·783
62	1·835	85·3212	153	4·538	358·321
63	1·853	87·1744	154	4·583	362·904
64	1·872	89·0461	155	4·629	367·534
65	1·890	90·9366	156	4·676	372·209
66	1·909	92·8460	157	4·722	376·931
67	1·928	94·7744	158	4·769	381·700
68	1·948	96·7222	159	4·817	386·517
69	1·967	98·6894	160	4·862	391·382
70	1·987	100·676	161	4·914	396·296
71	2·007	102·683	162	4·963	401·259
72	2·027	104·710	163	5·012	406·272
73	2·047	106·757	164	5·063	411·335
74	2·067	108·825	165	5·113	416·448
75	2·088	110·913	166	5·164	421·612
76	2·109	113·022	167	5·216	426·828
77	2·130	115·152	168	5·268	432·097
78	2·151	117·304	169	5·321	437·418
79	2·173	119·477	170	5·374	442·792
80	2·195	121·671	171	5·428	448·220
81	2·216	123·888	172	5·482	453·702
82	2·238	126·127	173	5·537	459·239
83	2·261	128·388	174	5·592	464·831
84	2·283	130·672	175	5·648	470·480
85	2·306	132·979	176	5·705	476·185
86	2·329	135·309	177	5·762	481·946
87	2·353	137·662	178	5·819	487·765
88	2·376	140·038	179	5·878	493·643
89	2·400	142·439	180	5·936	499·580
90	2·424	144·863	181	5·996	505·576
91	2·449	147·312	182	6·056	511·631

1.	2.	3.	1.	2.	3.
183	6·116	517·748	267	14·11	1324·973
184	6·177	523·925	268	14·25	1339·222
185	6·239	530·164	269	14·39	1353·615
186	6·302	536·466	270	14·54	1368·151
187	6·365	542·831	271	14·68	1382·832
188	6·428	549·259	272	14·83	1397·661
189	6·492	555·752	273	14·98	1412·637
190	6·557	562·309	274	15·13	1427·764
191	6·623	568·938	275	15·28	1443·041
192	6·689	575·622	276	15·43	1458·472
193	6·756	582·378	277	15·58	1474·056
194	6·824	589·202	278	15·74	1489·797
195	6·892	596·094	279	15·90	1505·695
196	6·961	603·055	280	16·06	1521·752
197	7·030	610·085	281	16·22	1537·969
198	7·101	617·186	282	16·38	1554·349
199	7·172	624·358	283	16·54	1570·893
200	7·243	631·601	284	16·71	1587·601
201	7·316	638·917	285	16·88	1604·477
202	7·389	646·307	286	17·04	1621·522
203	7·463	653·770	287	17·21	1638·737
204	7·538	661·307	288	17·39	1656·125
205	7·613	668·920	289	17·56	1673·686
206	7·689	676·610	290	17·74	1691·422
207	7·766	684·376	291	17·91	1709·337
208	7·844	692·219	292	18·09	1727·431
209	7·922	700·142	293	18·27	1745·705
210	8·001	708·143	294	18·46	1764·162
211	8·081	716·224	295	18·64	1782·804
212	8·162	724·387	296	18·83	1801·632
213	8·244	732·631	297	19·02	1820·648
214	8·326	740·957	298	19·21	1839·854
215	8·409	749·366	299	19·40	1859·253
216	8·494	757·860	300	19·59	1878·845
217	8·579	766·439	301	19·79	1898·634
218	8·664	775·103	302	19·99	1918·620
219	8·751	783·854	303	20·19	1938·806
220	8·838	792·693	304	20·39	1959·194
221	8·927	801·620	305	20·59	1979·786
222	9·016	810·636	306	20·80	2000·584
223	9·106	819·742	307	21·00	2021·590
224	9·197	828·940	308	21·21	2042·806
225	9·289	838·229	309	21·43	2064·234
226	9·382	847·611	310	21·64	2085·876
227	9·476	857·087	311	21·86	2107·735
228	9·571	866·658	312	22·08	2129·812
229	9·666	876·325	313	22·30	2152·110
230	9·763	886·088	314	22·52	2174·631
231	9·861	895·949	315	22·75	2197·377
232	9·959	905·908	316	22·97	2220·351
233	10·06	915·968	317	23·20	2243·554
234	10·16	926·127	318	23·43	2266·990
235	10·26	936·389	319	23·67	2290·660
236	10·36	946·752	320	23·91	2314·566
237	10·47	957·220	321	24·14	2338·711
238	10·57	967·792	322	24·39	2363·097
239	10·68	978·470	323	24·63	2387·727
240	10·78	989·255	324	24·88	2412·604
241	10·89	1000·147	325	25·12	2437·729
242	11·00	1011·149	326	25·38	2463·106
243	11·11	1022·260	327	25·63	2488·787
244	11·22	1033·483	328	25·89	2514·724
245	11·33	1044·818	329	26·15	2540·770
246	11·45	1056·266	330	26·41	2567·177
247	11·56	1067·829	331	26·67	2593·848
248	11·68	1079·507	332	26·94	2620·786
249	11·79	1091·302	333	27·21	2647·993
250	11·91	1103·215	334	27·48	2675·473
251	12·03	1115·247	335	27·75	2703·227
252	12·15	1127·399	336	28·03	2731·259
253	12·27	1139·673	337	28·31	2759·571
254	12·40	1152·070	338	28·59	2788·166
255	12·52	1164·591	339	28·88	2817·047
256	12·64	1177·237	340	29·17	2846·217
257	12·77	1190·009	341	29·46	2875·67
258	12·90	1202·909	342	29·76	2905·42
259	13·03	1215·938	343	30·05	2935·47
260	13·16	1229·098	344	30·36	2965·82
261	13·29	1242·389	345	30·66	2996·48
262	13·42	1255·813	346	30·97	3027·44
263	13·46	1269·371	347	31·28	3058·71
264	13·69	1283·064	348	31·59	3090·30
265	13·83	1296·895	349	31·91	3122·20
266	13·97	1310·064	350	32·22	3154·42

Tables may be constructed by logarithms thus:—

let radius of copper - - = 1

" gutta-percha - - = g

and thickness of gutta-percha - - = $g-1$;

then, if S = thickness sought to produce any given insulation or inductive resistance, $S = gn-1$, and $(\log. g) \times$ = log. $(S + 1)$.

I have verified this table in numerous ways, both for induction and insulation, by experimenting on gutta-percha covered wires, and also upon wires covered with such materials as shellac and sealing wax; but while experimenting on this subject I found that some materials did not strictly follow this law as regards insulation, and one of those materials was sealing wax. Upon making the insulators of various diameters, but of proportionate lengths, so as to give a like amount of induction, in accordance with the table; they were then insulated internally and externally, the two interiors were connected together and charged. They were then removed and reversed, and if they each were equal in inductive capacity, the negative charge of the exterior of the one exactly neutralized the positive charge of the interior of the other. With sealing wax, especially, I obtained the following phenomenon, when I had turned them over and made the connexion; first of all, I got an indication of electricity, either positive or negative, as the case might be, but gradually it became neutral, and after the lapse of a few seconds I had signs of negative electricity. I found that the jar, if I may use the term, of equal inductive capacity which was the larger in dimension always showed a larger amount of electricity taken in. This was chiefly observed with sealing wax which had been very much heated and boiled before being put on to the line; the difference from the law is, however, very small, not more than three or four per cent. This difference from the law of induction (which holds good with such insulators as atmospheric air, and with some kinds of glass) is termed "electric absorption;" and this electric absorption will, to a certain extent, interfere with rapidity of transmission through a cable. More especially does it interfere with working to more than the second wave. If any attempt be made to work to greater nicety than the second wave, this absorption almost annihilates the currents.

2916. You mean that the insulating material absorbs a certain quantity of electricity into its pores, which it does not give out again for some time?—It gives it out slowly. Green glass absorbs a large quantity of electricity, and gives it out slowly; sealing wax, especially when it has been much heated, and allowed to cool, absorbs a very large quantity.

2917. If it is warm at the time?—No; I mean when it is cold after being heated. All the resins seem to absorb more or less electricity to a considerable extent; in an electrophorus you can very often see it penetrating the substance itself. There are two phenomena of great importance that I may mention, raising the temperature of the sealing wax greatly increases its inductive capacity; a difference of temperature of 40° Fahrenheit will increase its inductive capacity 30 or 40 per cent.; and it is quite within the range of possibility that the discrepancy between the observed inductive capacity and that indicated by theory may have been due to a difference of temperature in the materials tested. The second phenomenon is this: if, when one of the test wires have been charged positively, it be discharged for 1 second, then charged negatively for 2 seconds, and then discharged half a second and applied to an electroscope, first negative electricity comes out of the dielectric, then this dies away and out comes the original positive charge, and which continues for a very long period.

2918 (*Mr. E. Clark.*) When you speak of heated sealing-wax, is not all sealing-wax heated?—Yes, but not so much as to cause the more volatile parts to boil.

2919. Do you mean more than the ordinary melting temperature?—Yes, so as to make it boil and bubble.

I have, at the end of the table, a few observations upon the Atlantic cable:—"Assuming that the "Atlantic cable gave 1 word per minute, and that "the external diameter of the core were $\frac{1}{3}$ of an "inch, and of the conductor $\frac{1}{15}$ of an inch, and "weight 93 lbs. per statute mile, to make the same "conductor give a speed 7 times as great, *i.e.* of "7 words per minute, the gutta-percha must have "been increased in diameter from $\frac{1}{3}$ inch to 180 feet, "while, on the other hand, had their present ratio "been preserved, and both copper and gutta-percha "been simultaneously increased until the copper wire "were $\frac{1}{2}$, and the gutta-percha $\frac{2}{3}$, of an inch in diameter instead of 180 feet, 7 words per minute would "have been obtained. These, however, are not the "most economical proportions; for were the conductor increased in diameter from $\frac{1}{3}$ to $\frac{3}{10}$ inches, "the gutta-percha remaining $\frac{2}{3}$ of an inch as before, "the speed would be increased from 7 to 11 words "per minute. By increasing the diameter of the "copper until it is $\frac{3}{4}$ of the diameter of the gutta-percha, the maximum speed is nearly approached, "but this depth of insulator is insufficient to bear "the ill-usage a cable meets with. Were the copper "made half an inch in diameter, the gutta-percha "remaining as before $\frac{2}{3}$, the speed would be about 16 "words per minute."

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2920. (*Mr. Bidder.*) Is not your table based, as far as the conductor is concerned, upon the square of the diameter?—Yes; that I have verified very carefully.

2921. I suppose it has struck you, as it must everybody, that a great analogy seems to hold between conductors as regards electricity and pipes applied to hydraulic purposes?—The analogy seems to hold in a great many respects, but totally fails as regards intensity. You cannot increase the speed of the wave in a cable by increasing the tension, but only its height; it is a more perfect fluid.

2922. Being the more perfect fluid, ought not the intensity to create velocity?—Intensity increases the velocity of electricity (*i.e.* the quantity of electricity transmitted) in the ratio of the increase of tension; this has nothing to do with the rate of the waves, whose height will be increased with the tension, but not augmented in speed. To make twice the quantity of water flow out of a tube in a given time, the "tread" (or intensity) must be increased nearly fourfold, or as the square, and so on; not so with electricity, the quantity flowing out increasing in the same ratio as the tension. There is no inertia in electricity unless magnetic metals be in the neighbourhood, by the presence of iron all the effects of momentum can be produced in an electric current. I have frequently used water flowing through pipes to convey to my mind an idea of the action going on with respect to electricity. If you were to suppose a tube filled with sand as the conductor of *water*, a tube of a given diameter would permit a given quantity of water through it, and a tube of twice that diameter would permit twice that quantity of water through it. So with electricity; a copper wire of a given diameter gives a given quantity of electricity through it in a given time, and twice the area of the material gives exactly twice the amount.

I may add, with regard to that table, that quite independently of myself, and by another process of reasoning altogether, Professor Thomson has arrived at the same results with regard to the law of induction in telegraphic conductors, and as regards the depth of insulating material necessary to cause a variation in the inductive power of a cable, his figures agree with my table.

2923. (*Professor Wheatstone.*) Does the formula $\log. \frac{D}{D'}$ express your experience?—I believe it does, because you state it is Professor Thomson's formula; the logarithmic numbers express the inductive capacity of a wire when radius of copper wire is equal to unity. I have not seen the formula in question, and do not understand $\frac{D}{D'}$, without further explanation; a wire of a given length conducts a given amount of electricity. If we consider this as the conductor, two wires offer exactly double the resistance, and allow exactly half the current to flow through; but if you put two of these wires together you get the same result; that is to say, with four

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wires, two in length and two in thickness, as with a single one, you can carry on the same reasoning, and show that this holds good also for any form; that is to say, supposing this to be the diameter, not coming to a point, because that comes to perfect insulation, but anywhere short of a point, another one twice that size and twice the width will conduct exactly the same amount of electricity; that is to say, it will offer the same amount of resistance. This table applies equally to the insulating power of gutta-percha, looking upon it as a conductor, as it does to the induction of the gutta-percha, excepting the fact that there is a great variation in the quality of the material, and the leakages are due to special faults of extreme minuteness and hence discrepancy; but there is no doubt that this is the law of any uniform dielectric. Wray's material follows this table exactly. These layers of equal induction also show the layers of equal resistance to electric conduction; therefore the wire that will give half the amount of induction will insulate twice as well if the insulating material be uniform throughout. I made some experiments on the Zandvoord cable, which has lately been laid by the Electric and International Company, of which these are the results.

SEPTEMBER 1858.

TESTS of the DUNWICH ZANDVOORD CABLE at Greenwich.—Temperature 62½ Fahr.

139½ miles of double gutta-percha, covered wire, gave	-	-	with 180 cells of Daniell's
139½ miles of 2 coats of Chatterton's compound and gutta-percha, gave		35°	" "
A circuit whose resistance (galvanometer included) equalled 85 miles of the cable		48°	1 cell "
A circuit whose resistance (galvanometer included) equalled 158 miles of the cable		35°	1 cell "

JANUARY 7th, 1859.

Cable had been under the ocean since September 1858.

129 miles of plain double covered wire, gave	-	-	19° 96 cells "
129 miles of 2 coats Chatterton's, as before		14½°	" "
A circuit whose resistance (galvanometer included) equalled 395 miles of cable		19°	1 cell "
A circuit whose resistance (galvanometer included) equalled 565 miles of cable		14½°	" "

Diameter of copper wire = ·0865 inches
" gutta-percha = ·375 "
Inductive resistance in my table = ·133½ "
whence,

a = 2 coats of plain gutta-percha, considered as a conductor
b = 2 " Chatterton's compound and gutta-percha, considered as a conductor.

At a temperature of 62½° Fahrenheit before submerging.

a. As gutta-percha : copper :: 1 : 7 × 10^{18} (7 trillions)
b. As C.'s comp. : copper :: 1 : 15·5 × 10^{18} (15·5 trillions).

Under the ocean, in January, 1859.

a. As gutta-percha : copper :: 1 : 14 × 10^{18} (14 trillions)
b. Chat.'s comp. : copper :: 1 : 23 × 10^{18} (23 trillions).

These amounts are only approximate, but near enough.

2924. (*Mr. Edwin Clark.*) Have you repeated the experiments at high and low temperature two or three times, or have you only the results of two experiments, one made in warm weather and the other made in cold weather?—I have frequently, with many others, observed the rapid increase in the conducting power of gutta-percha as it is warmed. I have a great many tests of the wires in question, and have selected these as good illustrations. There are four wires in the cable, and the two covered with Chatterton's compound gave each, within half a degree, the same result.

In 1853 or 1854 I went over with Mr. Edwin Clark, when the third cable between Orfordness and Scheveningen was submerged, and tested it during the operation of paying out, we there noticed a continual decrease of the loss by defective insulation as the cable was put under water, that is always observed in the paying out of cables. When the Zandvoord cable was laid, the insulation improved as the cable was submerged.

2925. (*Chairman.*) Have you made any experiments to determine the effect which the conductibility of the wire has upon the speed?—The speed varies in direct proportion as the conductibility varies.

2926. (*Mr. Edwin Clark.*) Have you made any experiments to ascertain that that is the case, or do you merely speak upon assumption?—I have made experiments in the following way and got some very curious results at first, which puzzled me considerably, but they were soon explained away. The plan I adopted was this: with a given wire I applied at each end resistance coils so as to increase the resistance of the whole circuit. By theory I had ascertained that the addition of resistance to either end of the line would decrease the speed in the like proportion. When I experimented upon the underground wires between London and Liverpool I found that adding resistance to the sending end decreased the speed very rapidly, while resistance added to the receiving end made but very little difference in the speed of transmission. I used a resistance, first of all, equal to that of the line itself; secondly, a resistance equal to double that of the line; and, in one case, a resistance four times that of the line. The results puzzled me. I found afterwards that it arose from the defective insulation of the line. I should here state that where a line is defective in insulation (and we will for argument's sake assume that the amount of defective insulation or resistance be constant), the speed of the wave is increased thereby. As an example, take two lines of, say, 100 miles in length; those when joined together would give a speed in the inverse ratio of the square of the distance; the speed would decrease exactly as the square of the distance. Those two wires alone would each of them have a speed four times as rapid as when united into one circuit. If there be a leak at the point of junction between these two lines, and the resistance of the leak be so small in comparison with that of the line that it may be made = O nearly, then any current applied to one end would charge the circuit. The tension of the current will be very small indeed at the leak; this weak current will, however, cause a wave in the second half of the circuit. Now, were the wave measured from the battery over the first 100 miles to where the leak is situated, you would have a speed due to that length, because the resistance of the leak by assumption is too small to be considered of any consequence, and from the leak to the distant end, another 100 miles, you would have a speed due to that length. The result will be, that for such a wire of 200 miles in length, with a leak equal to O nearly in the middle, the speed through it will be just one-half what it is through 100 miles, instead of being one-quarter; therefore carrying this on, if you assume that along your line you have a series of leaks throughout, and if all of them offer no appreciable resistance, an infinitesimal current will reach the distant end, but the speed of transmission through that line will be almost indefinitely great. In order to determine this, I had first to ascertain that Professor Thomson's law of the squares of distances was correct; and, secondly, that increasing the tension of the battery does not in any way increase the rapidity of the transmission of the wave through the line. For that purpose I constructed a machine which should leave no ambiguity in its results. I am rather at a loss how to explain its action clearly; but before attempting to do so I will show the reason why many experimenters have been misled in their calculations. People have generally experimented with a single current, and have noted how soon the electric current, as they termed it, reached the other end. What they did was simply to ascertain how long it was before a sufficient current arrived at the other end, or rather, how long it was before the wave at the other end rose to a sufficient tension to actuate their instruments. The mode which I adopted is one which gives comparable and constant results. I made a machine whose speed

could be governed at pleasure, and to which was attached a key, similar to that invented by myself, for working through submarine lines for reversing the current into the line. I rotated this key, so that at every revolution the line was connected with the positive pole of the battery, the negative pole being connected with earth during one half of the revolution, and during the other half of the revolution the connexions were reversed. The current that was received at the distant end was made to come back through a similar commutator on the same axle, so that as often as the currents were reversed at the sending end they were reversed at the receiving end. Those currents were then, after passing through this commutator, sent through an astatic galvanometer whose needle was weighted, so that it should slowly oscillate. When this wheel was rotated very slowly, as often as the currents were reversed at the sending end, the connexions of the galvanometer were reversed at the distant end, and consequently the same indication was shown, viz., the needle was deflected to one side. As the electric wave requires a considerable period to reach a given elevation, I was enabled, by increasing the speed of rotation, to reach the point where the second commutator exactly bisected the wave, and consequently sent one half the current round the galvanometer in one direction and the other half round in the other direction, and so brought the needle to zero, showing when the waves that were alternating in the line reached a given elevation. This machine was made to rotate at such a rate that the second commutator should exactly bisect the wave. As this is about the only way of rationally expressing the comparative speeds of telegraph circuits, I recommend it for general adoption. In this way I found, when I doubled the length of the line, that the speed of the machine had to be reduced to one quarter, in order that the second wheel should bisect the wave; and when the line was increased in length to three times, the speed of the machine had to be reduced nine times.

2927. (*Mr. Edwin Clark.*) As the square exactly?—Yes. The next thing I had to do, to see whether my theory was correct, that double the amount of the resistance of the line applied at either end would reduce its speed one-half; and, secondly, that it was immaterial at which end of the line that resistance was applied so to reduce the speed; I will give you the results as given by the machine, and the results as given by theory; 2·7 was the result given by experiment, and 2·4 the result given by theory, and considering the nature of the experiment that was an amount of error quite possible, as will be seen from the variation of different results. The mean of five results, with a line of 154 miles, gave 15·16 reversals per second; with double that length of line, the mean of two results gave 3·78; 3·79 was the amount deduced by theory, that is as the square of the distance. A mean of eight results, with a line of 462 miles, gave 1·727, and theory gave 1·685, showing that the law of squares held good, or at all events very nearly so, the machine being difficult to govern at this slow speed. I tried also another experiment, to determine whether, as had been stated at the Institute of Civil Engineers, when discussing the Atlantic Telegraph Cable, the iron covering of the cable produced any material retardation. By some engineers the retardation was supposed to be almost entirely due to the magnetic induction of the iron covering. I tried two experiments to determine this with the 462 miles, and got a mean speed of 1·75 reversals per second. I tested this on the cable in the East India Docks, by first passing the currents in alternate directions; and secondly, by making them flow through the wires in one direction. In the first case, the magnetic induction obviously neutralizes its effects; in the second case, it was in full force on all wires, but this source of retardation is so small that I could not discover any difference. In the first instance the currents were passed up the first wire, down the second, up the third, down the fourth, up the fifth, and down the sixth. In the second case, when the magnetization of the iron was opposing, the result was 1·75, and in the case where we had the magnetism neutralized we had 1·727. These experiments, however, only show that the amount of the effect of the iron is so minute, that it certainly does not produce more than 3 per cent. of the whole retardation, and probably not 1 per cent.

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2928. What was the length of the cable?—Seventy-seven miles, with six wires in it. These experiments show one thing, that is, that the system adopted by the German instrument-makers of using a very fine wire on their relays, offering a very great resistance, frequently equal to 50 or 80 miles of the cable used, reduces the speed very materially; for if the resistance in the receiving instrument be twice that of the cable itself, it reduces the speed to one half. This is an item of importance; the resistance of the receiving instrument should be reduced as low as possible, without sacrificing too much mechanical power for working.

2929. (*Chairman.*) Were you not of opinion that the Atlantic cable would not transmit at the rate mentioned in the Atlantic Telegraph Company's pamphlet?—Yes. In the Atlantic Company's pamphlet, Professor Morse wrote to Cyrus Field, stating that from experiments upon the underground wires belonging to the Magnetic Company, he anticipated a speed of certainly not less than than nine or ten words per minute. I have tried experiments on the underground wires belonging to the Electric Telegraph Company, and found that the wave, when worked up to that pitch, about the third or fourth wave was so uncertain and so vague, that it could not possibly be used; and from careful consideration of those results I came to the conclusion that not more than one message per hour during the 24 hours could be transmitted by the Atlantic cable, assuming that each message contained 25 words. I called on one of the leading Directors of the Atlantic Telegraph Company and told him what were my views upon the subject; but the Atlantic Company seemed perfectly well satisfied with the experiments they had tried, and were confident of the results.

2930. (*Mr. Saward.*) You have modified your views somewhat as to the amount of messages that could be got through the Atlantic cable since then, have you not?—My estimate of the speed of the present Atlantic cable, assuming that you used relays, is, that your maximum speed will not exceed one and a quarter words per minute. I think 1·1 words the maximum. I have seen some imperfect slips giving 1·2 words per minute.

2931. With the existing cable?—Yes, and with relays; but by using such an instrument as the reflecting galvanometer, in which you are not obliged to come to zero before you can produce a new signal, you may get a speed of two and a half words per minute. In fact, I have seen a correspondence that took place between two clerks of the Atlantic Telegraph Company (I have seen the slips and read them through), in which a speed of nearly four words per minute was obtained, abbreviations being used, and half of the words being guessed at only, in many instances the letter L could not be distinguished from the letter B or the letter H. Such a speed could not possibly have been used in ordinary despatches from the public.

2932. (*Chairman.*) The speed that you estimated before the cable was laid at 25 words an hour was rather more than one-third of a word per minute?—Yes; I anticipated that one word per minute would be got through the cable; and that you must only take one-third of that as the available speed during the 24 hours; that I think will be found in all cases to hold good as the available capacity of a circuit.

2933. (*Mr. Saward.*) Your remark as to a speed of even three words per minute only applies to the past cable, and not to such a cable as could be constructed, if I understand you?—Certainly. I have shown in my table that any speed you like can be obtained by increasing the dimensions of your cable.

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2934. I believe you have quite satisfied yourself as to the fact that an increase in the size of the conductor will result in a practical increase of speed?—These experiments as well as theory show it most decidedly, and I am most firmly of opinion that such will be the result, that increasing the size of the conductor will increase the speed, as indicated by that table. The Zandvoord cable, which has a larger wire than the Scheveningen cables, proves this beyond all doubt.

2935. (*Chairman.*) What is your opinion respecting the proper size for the conductor of an Atlantic cable?—That depends upon the speed you are content to work with. There are many things to consider in the construction of a line; first of all, it is not simply how fast you can get your waves through the cable, but you must also consider how much interruption you will receive from other causes. You must have a conductor of such magnitude as shall give a good strong current through the cable, in order that the apparatus shall work without derangement for a sufficiently long period to make the cable pay. You must also have a sufficiently powerful current to overcome the daily disturbance that arises from the causes previously mentioned, viz., earth-currents, and so forth. You must also have a sufficient amount of current to allow a certain amount of variation in your battery; for instance, the contacts at the sending end will frequently not be perfect, a little dirt gets in, or they get dry, and the moment the oil between the contacts gets dry you get imperfect contact; instead of a continuous rub, you get a rapid succession of momentary contacts. Although oil itself is a non-conductor of electricity, or nearly so, you never can make a perfect contact between rubbing surfaces without oil, and the moment the oil gets away, as it will do in practice in the millions of alternations of contact that have to be made during the year, those contacts will get out of order, and unless you have sufficient latitude in the working of your cable to overcome those disturbing causes the speed will be very slow. Added to that the clerks become disgusted if the cable work indifferently, and they are glad to make any excuse to get away; but if the line works pretty well, and they are able to take the messages off without much trouble, they give their whole heart to it, work away satisfactorily, and get off a much greater amount of business than when the cable does not go on steadily and uniformly. For these reasons I should not like to recommend a cable whose maximum was less than 8 or 9 words or more per minute. A copper wire one quarter of an inch in diameter of high conducting power, and covered to a depth of one quarter of an inch with gutta-percha, would give 7 or 8 words about, but if covered with indian-rubber or Wray's compound (No. 2), about 12 or 13 words might be obtained; total diameter being three quarters of an inch.

The following formula will give the approximate maximum speed of telegraph cables covered with gutta-percha, the conductor being copper, and the gutta-percha and copper being of average quality.

$$\frac{A\ i\ c}{l^2} = S = \begin{cases} \text{speed in words per minute, each} \\ \text{word having five average letters.} \end{cases}$$

With ordinary copper and ordinary gutta-percha $A = \frac{1}{23 \cdot 3}$ or $\frac{1}{24}$ (to ascertain the commercial value of a cable, A should be taken at $\frac{1}{100}$).

A, a constant for gutta-percha and copper $= \frac{1}{23 \cdot 3}$.

With Wray's material I should make $A = \frac{1}{14}$ or $\frac{1}{13}$.

With indian-rubber $A = \frac{1}{11}$, $i =$ value in my table of inductive resistance.

$c =$ lbs. of copper to the statute mile.

$l =$ length of line, the unit being 100 miles (statute).

S = speed in words per minute, *i.e.* words of five letters average.

The value of A depends to a great extent upon the instrument used.

The quickest relay instruments are those which have no residuary magnetism, *i.e.* my galvanometer relays, which are always used with long subterranean circuits.

As A may have to be modified as we improve our modes of working, I had better give the data on which I got its value.

N.B.—Instruments which work with a "shifting zero," like Professor Thomson's reflecting galvanometer, will give more rapid results; their action at present is uncertain.

1st. Underground wire from London to Liverpool and back, *viâ* Colwick, Macclesfield, and Manchester, $= 220 \times 2 = 440$ statute miles.

2nd case. Atlantic cable = 2,357 statute miles.

The slips obtained were fortunately timed by clock-work automatically. I have seen some in which the speed by relay was recorded at the rate of even 1·2, 1·0, 1·1 words per minute.

I think 1·1 the highest we should take as the maximum (none of the slips were quite perfect). These speeds agree so nearly with the above value of A, that I think this formula may be considered accurate enough for calculation.

$$\text{as } \frac{A\ i\ c}{l^2} = S$$

$$\therefore A = \frac{l^2\ S}{i\ c}$$

Dimensions of the circuits.

Underground wire to Liverpool and back.

Copper = ·062 inch in diameter, and 61 lbs. per mile.
Gutta-percha = ·255 inch " "
And $i = 142$ in my table.

Length of circuit $= 220 \times 2 = 440$, *i.e.* London to Colwick, Macclesfield, Manchester, and Liverpool and back. Maximum speed obtained = 19·7 to 20 words per minute, say 20.

Atlantic cable—
Copper = ·083 or ·0825 = 93 lbs. per mile.
Gutta percha = ·383.
$i = 154$.

Maximum speed, say, 1·1 words of five average letters.

Liverpool wire—

$$A = \frac{l^2\ S}{i\ c} = \frac{(440)^2 \times 20}{142 \times 61} = \frac{387 \cdot 2}{8662} = \frac{1}{23 \cdot 3}$$

Atlantic cable—

$$A = \frac{(23 \cdot 57)^2 \times 1 \cdot 1}{154 \times 93} = \frac{611 \cdot 05}{14322} = \frac{1}{23 \cdot 4}$$

Or say $\frac{1}{24}$, which is near enough for all practical purposes where the cable's maximum does not exceed 20 words per minute.

2936. (*Mr. Saward.*) With regard to your difficulty as to battery power; could you not get rid of that difficulty by using a large magneto instrument worked by a small steam-engine to reverse the poles?—I had better answer that question by giving my views upon the effect of induction coils, magneto instruments and batteries. When you use a continuous flow in from a battery until the signal is produced, and there is a continuous flow in of a current in the opposite direction to reverse that signal; where you use induction coils whose flow in may be considered as instantaneous you increase the speed of the cable considerably, perhaps in the proportion of from one to two or one to two and a half. But if the period for contact with the battery be taken at one second, and the flow in from the induction coil be considered as $\frac{1}{100}$ of a second, (it certainly is not much more in many cases, I am speaking of high tension charge from induction coils) then you must have a tension 100 times that which is necessary if you use a battery with a contact of one second. Such a tension as that is highly dangerous to the cable; the induction coil gives you the moment you break the contact with the primary a very sudden current of very high tension; which very rapidly falls to a low tension, ever decreasing, never entirely ceasing, but flowing, I suppose, until eternity. It falls (practically speaking) from its maximum to nearly nothing in a very short space of time. Where you use the magneto

machine the current is produced by the separation of the armature from the magnet, and reversing its position and polarity, this operation occupies a very much longer period of time compared with the induction coil. In this machine the same quantity of electricity is generated over a comparatively much longer period of time, and the same effect is produced in the long cable without exposing it to very high tension. The magneto machine if worked by a donkey engine, so as to get over the great difficulty in the way of using it where the clerks have to work it by hand, which gives rise to great fatigue, and consequently low speed, would be found to be one of the best sources of electricity for working long cables.

2937. (*Chairman.*) As being the most rapid?—Yes, excepting you worked by battery through induction plates which you charge to a given amount and discharge into the cable, the latter being put to earth immediately afterwards, by this means you get even a more rapid result through your cable than by using the magneto machine. I mean something like a very extensive Leyden jar charged to a very low degree. For this purpose a series of gold, platinum, or lead plates in dilute sulphuric acid, arranged like a voltaic battery, forms the best induction plates, owing to their great inductive capacity.

2938. (*Mr. E. Clark.*) Have induction plates been practically used?—They have never been practically used. I have made induction plates of a considerable size, but they have to be so bulky that they would be very expensive for ordinary lines. The fluid ones answer very well and take up no great room.

2939. (*Professor Wheatstone.*) Do you expect a better result from induction plates than by using Rhumkorff coils?—Not a much more rapid result, but you may diminish the tension and not weaken signals.

2940. You may diminish the tension by Rhumkorff coils to any degree you please?—It is extremely difficult to diminish the tension; if you diminish the tension you must elongate the flow of the current in order to get the same result. If you diminish the tension from 1 to 0½, the current must be made to flow a period of twice that time in order to get the same result. Whitehouse used induction coils which gave a spark nearly a quarter of an inch in air; this would give a tension equal to some 10,000 or 15,000 cells of Daniell's battery; but 100 cells of Daniell's battery gave a very much stronger signal through the Atlantic cable with a contact of two seconds duration, consequently it shows how short is the flow of electricity from these coils. The only way in which you can reduce the speed of the flow, that is to say, make the same electricity flow out from a Rhumkorff coil during a longer period, is by increasing the diameter of the iron coil, and this you would have to do to such an enormous extent that I am afraid you could not use it. If, however, you could increase the dimensions of the Rhumkorff coil until it gave the same results as the magneto machine, you could use it equally well. There is a great difficulty in working these induction coils, owing to the combustion of the contact springs, which open and close the primary circuit; these springs burn and splash about so rapidly that you ultimately get a succession of contacts instead of a single one, which very materially interferes with the regularity of the currents from them. In the magneto machine you would be free from those difficulties, and therefore I anticipate you would get a higher speed in practice from a magneto machine than you would from induction coils.

2941. (*Mr. Edwin Clark.*) Have you used a magnetic machine on the Atlantic cable?—I have not. One was taken there after the cable had ceased to signal.

2942. (*Chairman.*) You went to Valentia to test the cable after it failed?—Yes.

2943. Can you give the Committee the result of your experiments?—The result of my experiments was this. A fault existed at a considerable distance from Valentia; but as no standard of the conductivity of the strand of the Atlantic cable was at hand, I could get no very correct measurement of the distance. There was one piece of cable measuring, I should think, less than a mile at Valentia, which I used for my standard, and as that piece was very short indeed it was very difficult to compare it with my resistance coils. I took that as one measurement, and I found a bobbin of brass wire, which Professor Thomson tells me he had found nearly equal to 100 miles of Atlantic cable. I used that as a second, and I then got a third. I forget what the third was now, but comparing all these three together I came to the conclusion that the fault must be somewhere between 245 and 300 statute miles from Valentia, that is as the cable lies. Had the resistance of the wire been measured as manufactured into cable, the distance of the fault would have been known to within ten miles. Professor Thomson also, when I asked him if he had any data showing the state of the cable before it was submerged, gave me some important facts which, since I have made my report, have been confirmed by tests, to which I will shortly allude. Those data were: "Testing of coils on board "the 'Agamemnon,' consisting of about 1,200 statute "miles of cable, 1st, when the upper end was dis-"connected, the current entering the cable from a "battery was 8·5 parts. 2nd, when upper end was "put to earth, current entering the cable was 10·5. "3rd, current going out of upper end of cable to the "earth" (that is, when the battery was on at the lower end) "5 parts. 4th, when the lower end was discon-"nected the current entering the cable was 8·5. "5th, when lower end to earth 10·5. 6th, current "going out of upper end of cable to earth 4·5 parts," showing with these figures 4·5 instead of 5, that the cable was very nearly uniform at both ends. When 200 miles had been removed from one end of the coil, I presume the upper end, the results were,—current entering the one end of the cable when the other end was disconnected, 7·5 parts; but when the same battery was applied to the other end 8·5 parts entered. When the distant end was put to earth and the battery applied to one end 10·25 parts entered, the other end 11·5 parts. Current flowing out at the end when the battery was applied at one end was in each case uniform, 6·5 parts. Thus, then, it was clear that after 200 miles had been removed from one end the state of the cable had considerably changed. Calculation showed that there was a fault about 560 miles from one, and 440 miles from the other. The way in which this was calculated was this:—If the current flows along the line and arrives at a fault, the distant end being to earth, the current that flows out there will be diminished, owing to part of the current escaping through the fault. Electricity, when it meets two channels, divides in the inverse proportion of the resistance, that is to say, if the resistance of the portion between the fault and the distant end be 1, and the resistance of the fault be 2, two-thirds of the electricity arriving at the fault will go on, and one-third escape through it. Secondly, by putting on currents and measuring them as they come out, you are enabled to eliminate the resistance of the fault, and show its locality. This showed a fault whose resistance was somewhere about 800 miles. In a case of this kind, where the galvanometer offers very little resistance, the following formula will give the position of the fault:—

S, length of cable.

x, cable between 1st end and the fault.

y, „ „ fault and distant end.

e^1, current flowing out of distant end to earth, battery being on 1st end.

e, current entering 1st end.

f^1, current from 1st end to earth, distant end now having a battery attached to it.

f, current entering distant end from the battery.

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$e - e^1$; difference of currents in first case $= d$.
$f - f^1$; ditto, second case $= \delta$.
Then $x : y :: \delta . e^1 : d . f^1$.

$$x = \frac{S}{1 + \frac{d . f^1}{\delta . e^1}}$$

Allowance has to be made for leakage of gutta-percha in such a cable as this, whose conductor is so small for its length. The effect of a general leakage through the gutta-percha is to make the fault appear nearer to the centre than it really is when tested for in this manner. When the experiments were made upon the two lengths of cable put together on board the "Agamemnon" battery at "Agamemnon" end, the current entering the cable the "Niagara" end being disconnected was 45 parts. Second "Niagara" end to earth 49⅝, and third current flowing out at "Niagara" end to earth 15⅝ parts. "Battery at the 'Niagara' end: "Current entering cable 'Agamemnon' end being "disconnected 35⅜ parts," instead of 45, and when the "Agamemnon" end was to earth, the current entering was 37 parts instead of 49; and the current flowing out at "Agamemnon" end to earth 14 instead of 15 parts, all showing that there was much more escape on board the "Agamemnon" than on board the "Niagara." I will put in a copy of my report to the Atlantic Telegraph Company.

The Report was handed in, and was as follows:

REPORT ON THE STATE OF THE ATLANTIC TELEGRAPH CABLE.

I arrived at Valentia on the evening of the 5th instant, when I found that no *words* had for many days been received through the cable from Newfoundland.

On the 6th, 7th, 8th, 9th, and 10th I tested the cable at intervals, in four different ways, to ascertain its condition. The following are the results:—

Firstly. There is a fault of great magnitude at a distance of between 245 and 300 statute miles from Valentia, but the locality cannot be more accurately ascertained until a portion of the cable, 20 or 30 miles in length, has been tested against my standard of resistance, and until the log has been consulted, to ascertain the amount of slack paid out. (I would suggest that the piece of cable at Greenwich be carefully measured, and tested against my standard, in order to obtain the most correct estimate of the distance of the fault; assuming, however, that it is 270 miles, and allowing 22 per cent. for slack, it is possible that the chief defect is in shallow water, 410 fathoms.)

Secondly. The copper wire at the fault place above alluded to does not touch the iron covering of the cable as is proved by its forming a voltaic element, which gives rise to a continuous positive current, from the copper wire varying very little in tension.

Thirdly. The insulation of the wire between Valentia and the fault is perfect, or at least contains no defect of sufficient importance to be perceptible or to materially influence the working, were the cable otherwise perfect.

Fourthly. The copper wire is continuous, and consequently the cable has not parted. Faint signals or reversals are still received from Newfoundland, but the power used will shortly eat away the exposed copper wire in the faulty place by electrolytic decomposition.

The actual resistance of the fault appears to be at least equal to 10 miles of the cable, but is most probably greater.

Taking it at its lowest resistance, viz., 10 miles, and assuming that Newfoundland is only using 180 cells of "Daniell's" battery, the strongest current received thence, during my stay, was only 1·24th part of the force that it should be, were there but this one fault. When it is, however, borne in mind that on the other side they are probably using more power, and also that the defect first alluded to probably offers more resistance than that assumed, viz., 10 miles, it is evident that there is another and more distant fault, the approximate locality of which I could not pretend to estimate at this end, without being able to speak to Newfoundland.

From authentic data, shown to me at Valentia, I am of opinion that there was a fault on board the "Agamemnon" before the cable was submerged, at a distance of about 560 miles from one end and 640 from the other.

The following are the data in question, but on what occasion they were obtained, I am unable to state; they were, however, probably taken when the ships were at Queenstown:—

Testing of coils on board the "Agamemnon," consisting of about 1,200 statute miles of cable:

1st.	When the upper end was disconnected the current entering the cable from a battery was	8·5 parts.
2nd.	When upper end was put to earth, current entering the cable was	10·5 „
3rd.	Current going out of upper end of cable to the earth	5 „
4th.	When the lower end was disconnected the current entering the cable was	8·5 „
5th.	When lower end to earth	10·5 „
6th.	Current going out of upper end of cable to earth	4·5 „

Showing that if there were a fault it was nearer to the upper end, but not far from the middle of the coil.

When 200 miles had been removed from one end of the coil (but from which end I am not at present aware), leaving 1,000 miles, the amounts were:—

1st	7·5 parts.
2nd	10·25 „
3rd	6·5 „
4th	8·5 „
5th	11·5 „
6th	6·5 „

Indicating that there was a fault, by rough calculation, at about 560 miles from one end, and 440 from the other.

With the 200 miles of cable the amounts were:—

1st	2 parts.
2nd	40 „
3rd	39·5 „
4th	„
5th	40·5 „
6th	39·5 „

Test of the entire cable on board the "Agamemnon" and "Niagara," viz., 2,500 miles.

Battery at "Agamemnon" end.

1st. Current, entering the cable, the "Niagara" end being disconnected	45 parts.
2nd. "Niagara" end to earth	49⅝ „
3rd. Current flowing out at "Niagara" and to earth	15⅝ „

Battery at "Niagara" end.

4th. Current entering cable, "Agamemnon" end being disconnected	35⅜ „
5th. "Agamemnon" end to earth	37 „
6th. Current flowing out at "Agamemnon" end to earth	14 „

indicating considerable leakage on board the "Agamemnon."

I am also informed that the currents through the cable, even immediately after it was submerged, were so weak that relays were useless, and that not one perfect message was recorded by them; everything that was received being read from the deflections of a galvanometer.

By comparing the above data with those of the new cable now making by Messrs. Glass and Elliott for the Electric and International Company, the amount of current which entered the 1,000 miles of cable, when disconnected at one end, should not have exceeded 2 or 2·5 parts, instead or 7·5 or 8·5 parts.

The inference by rough calculation, therefore, is that there was a fault, offering a resistance equal to 1,000 or 1,200 miles of cable, situated at a distance about 560 miles from one end of the 1,200 miles coil on board the "Agamemnon."

This, however, cannot be the fault first alluded to—situated at about 270 miles from Valencia—but may have been the one which caused such alarm when the ships were 500 miles from Ireland, and when the signals ceased altogether, and never certainly recovered.

It is not at all improbable that the powerful currents from the large induction coils have impaired the insulation, and that had more moderate power been used the cable would still have been capable of transmitting messages.

To satisfy myself on this point I attached to the cable a piece of gutta-percha covered wire, having first made a slight incision in the gutta-percha to let the water reach the wire, the wire was then bent so as to close up the defect.

The defective wire was then placed in a jug of sea water, and the latter connected with the "earth."

After a few signals had been sent from the induction coils into the cable, and consequently into the test wire, the electricity burnt through the incision; rapidly burning a hole nearly 1-10th of an inch in diameter.

When the full force of the coils was brought to bear on the test wire by removing them from the cable, and allow-

ing the electricity only one channel, viz., that of the test wire, the discharges, as might be expected, burnt a hole in the gutta-percha under the water half an inch in length, and the burnt gutta-percha came floating up to the surface.

The foregoing experiments prove that when there are imperfections in the insulating covering there is very great danger arising from using such intense currents.

The size of the present conducting strand is too small to have worked satisfactorily, even had the insulation been sound.

With a strand of larger dimensions less intense currents would be required, and both speed and certainty increased.

It is not, however, altogether impossible that some intelligible signals may yet be received through the cable, as stated in my previous communication.

(Signed) C. F. VARLEY,
Electrician of the Electric and International Telegraph Company.

To the Chairman and Directors of the Atlantic Telegraph Company.

At Valentia, Professor Thomson asked me to give him a set of resistance coils. They were sent over to Newfoundland with full instructions how to use them to test the line. Mr. Lundy was entrusted with them; and the chief of the station at Newfoundland, Mr. De Sauty, carried out the experiments very carefully and very creditably, considering that they were new to him. The results of those experiments I have lately received, and they show that this first estimate of the fault is probably very nearly correct. I have here the engineer's log on board the "Agamemnon," and I find that early on the morning of the 1st of August, or midnight July 31st, when 360 miles of cable had been paid out by the "Agamemnon," the current suddenly failed. The "Niagara" had paid out 1,116 miles of cable; those added to the 360, give 1,376 miles as the distance of the fault from Newfoundland to this spot. The tests made against my resistance coils in Newfoundland, showed that the fault from Newfoundland was situated, not at 1,370 miles, but at 1,590 miles, there being a difference of 200 miles. When we look at the results of the electrical log, which I believe you have, you find that this fault, which almost entirely took away the signals from the "Niagara," recovered to a great extent; and therefore, that the fault offered a very considerable amount of resistance. From these results, I should think it probable that the fault itself offered a resistance at the time of from 300 to 400 miles; and as there was a defect 300 miles from Valentia making dead earth or nearly, there would be a distance between that fault and the second fault of about 400 miles. If, then, the fault itself gave a resistance of 400 miles, which the electrical signals received from Newfoundland seemed to indicate must be the case, then the results obtained at Newfoundland will agree with those deducible by calculation; therefore, finding that at this period the signals were observed to fail, from the data taken before the ships went out, that fault clearly existed at about that distance; and considering that the experiments made at Newfoundland all point to a fault at that spot, I think there is no doubt that a fault offering a resistance of 300 to 400 miles exists at a distance of about 650 miles from the Valentia end, in addition to the one at about 300 miles. I should add that, from recent tests of 7 miles of Atlantic cable, the resistance of the conductor is found to vary very much, and that it is quite possible that the fault is not more than 200 or 190 nauts from Valentia, slack being taken into account. No cable should be made without first ascertaining its exact resistance in short lengths, and the log should show when and where each length was paid out.

2943. (*Mr. Saward.*) If it were possible to place a well-insulated cable between that fault at 300 miles and Valentia, should you expect that we might still use the cable to some extent?—I should think that you might perhaps use the cable with care for perhaps 12 or 18 hours during the 24 hours, assuming that the cable be repaired on the other side also, for it is defective near Newfoundland.

2944. That is to say, we should be able to work messages through it for that time?—Yes.

2945. (*Chairman.*) Why do you limit the number of hours?—Because such a fault as that will frequently cause so much disturbance by its resistance varying as to destroy the communication for a time. If the fault remained constant you could work through the cable at a higher speed than without it, but as it varies in resistance very materially as the currents that go in are positive or negative, you would have it for a long portion of time rendered idle by the disturbance being too great to get intelligible signals through it. I had a very good illustration of this a short time ago, on one of the Orfordness and Scheveningen wires; one of the wires had been destroyed, as subsequent examination showed, by lightning, which had punctured the gutta-percha some distance out to sea from the Dutch coast. The current escaped, and this fault gradually got worse and worse, until it became what we technically term dead earth; that is, so much of the current flowed into the ocean that we could get no intelligible signals through. The cable having failed to be of use, I had 400 cells of Daniell's battery applied at one end, and all I could get, 250 cells, at the other end in Amsterdam, and this was kept on for half an hour, sending a positive current into the cable. At the end of the half-hour we tried to speak through, and worked for a short time, about 10 or 15 minutes, but the cable failed again. I then put the battery power on again at each end, and kept it on all night, and sent instructions to work through the following day with two batteries, the one battery having a power three times as great as the other, so that the negative current used to discharge the cable should not be on sufficiently long to break down the insulation if we succeeded in obtaining it. Upon taking off the battery power we found the cable apparently quite good, and we kept on this arrangement of having three times the amount of positive current that we had of negative, and worked the cable for more than twelve months in that state, until it got broken by a ship's anchor, when it was taken up and repaired, and the fault traced out. It frequently broke down again during that period, but then we had recourse to powerful batteries, and sealed up the fault again, and so went on working. Wherever the cable or wire is in mud this sealing up is much more readily produced than it is when the cable is in sand; in the latter case it often cannot be sealed up at all; it is very often difficult to produce it unless the cable be in mud. Now at the bottom of the Atlantic we have every reason to suppose that the cable is lying in a great deal of mud, and will embed itself deeper and deeper in the mud; and if it is perfectly quiescent, it is possible that that second fault in the Atlantic cable might be so filled up in time as not to cause any very serious obstacle to the working. I have frequently observed this peculiarity of the positive current sealing up a fault; it acts in different cases in different ways. In sea water it oxidizes the copper wire, and also produces a sub-chloride of copper, which renders the surface non-metallic, and to a certain extent insulating. Wherever the cable lies in mud, I think that the clay enters into combination with this material so formed, because wherever alum is used in sand batteries you find that the zinc plate becomes coated with a thick material, and ultimately you get a hard incrustation over the plate, which you cannot remove without great difficulty, even with a hammer and chisel. A somewhat analogous action takes place between the clay in the mud and the oxide or sub-chloride of copper that is produced by a solution of the copper wire by the electrolytic action of the positive current. Some years since, a wire between the Admiralty and our Strand office was three times reported by our man who repairs the street wires to be good, and as often reported bad by myself. I made an appointment with him to test the wire; I found that he had tested with the positive current, and I had tested with the negative. That wire was a peculiar instance of the effect which very frequently occurs, but it was very rapid; so rapid that the moment the positive current

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was applied the wire instantly insulated, and the moment the negative was applied it instantly gave way, and became dead earth. As we reversed the current the wire was either insulating or giving earth. Ordinary gutta-percha covered wire often shows to a small extent the same action; its insulation always improves slightly under the influence of a powerful positive current, and, generally speaking, becomes bad again to a certain extent with the negative current. I am very much inclined to attribute this phenomenon to the existence of water in the pores of the gutta-percha, for gutta-percha, as at present manufactured, is not free from moisture; wherever you get this variation, it is, in my opinion, an indication of electrolytic action going on.

2945a. Would not that action tend to destroy the gutta-percha in time?—In time it would, but almost to an infinitesimal extent. When a leakage by water is comparatively speaking large, for instance in a fault that offers a resistance of 10 or 12 miles of No. 16 copper wire, if a battery current of about 700 cells of Daniell's be applied, the effect of the current passing through the water at that fault, will be to render the surface red hot. It is a very pretty experiment to moisten a piece of gutta-percha, place two wires upon it, and upon applying the battery of 700 cells the moist surface immediately becomes heated, catches fire, and the streaks of fire wind in circles round the wire in all directions until it has completely charred and melted the surface. With such battery power as that you would not only char the surface of the gutta-percha and increase the conducting power, but you would increase the fault, melt the gutta-percha, and allow the wire to drop out of the centre. That I have done repeatedly where we have had faults which were too defective to be used and difficult to trace out, from the defect not being sufficiently large for coarse instruments to indicate; we have frequently put on all the spare battery power we could collect in the office, amounting to from 500 to 800 cells, and in that way enlarged the fault; by that means we have much more easily traced out the fault and more quickly repaired it. I have put a Rhumkorff's coil on to a piece of ordinary gutta-percha covered wire, viz., No. 16 copper covered to No. 3 wire gauge; this, after an hour or two's action, is almost, if not quite, unchanged as regards insulation. The power used might be estimated at 30,000 cells Daniell's battery.

2946. With regard to the resistance coils that you made for the Atlantic cable—you made them, I believe, entirely after the cable was laid—ought not such coils to be made whilst the cable is being manufactured, in order to have a perfectly accurate measurement of the resistance of the cable?—Those resistance coils I had made for my own use a long while previously and I had used them frequently for testing defects in submarine and subterranean lines, they had no reference to the Atlantic cable, which I never had the opportunity of testing or doing anything with before it was submerged. My coils were made to suit the No. 16 copper wire which was generally used by the Electric Company, and gave results which were marked off to correspond with miles of that wire for ease of use. Since then the copper wire supplied by the Gutta-percha Company has been of a higher conducting power, and though I still retain the same standard of resistance, I always reduce it to the copper that is being used. If the conductivity of the copper wire that is used be known and the weight of the wire known, it is the simplest thing in the world to reduce the cable to such a standard. But it appears that the copper wire in the Atlantic cable is of very different qualities, varying as much as 30 per cent. I believe. Consequently as it is not known where the good conducting wire is and the bad, it is impossible to say with any precision whereabouts the fault is situated, excepting within certain limits. But during the manufacture of a cable, every mile of that cable ought to be tested carefully with resistance coils, and the results accurately noted down; then after the cable is laid you may know where the good wire and where the indifferent is. In the Zandvoord cable, which was so tested, the copper wire was very uniform indeed in its quality, in fact 140 miles of the worst conducting wire equalled 141¼ miles of the better conducting wire, showing how very uniform the wire was throughout that cable. Such was the result of my tests on different lengths of the cable as manufactured, and it shows that copper wire can be supplied of very uniform quality; that the wire supplied for that cable is very uniform and also of a high conducting power. It is perfectly immaterial what resistance units be used. Weber's units might be used.

2947. Are you of opinion that the pressure which the gutta-percha in the Atlantic cable is subject to at the bottom of the ocean would injure it as an insulator, by causing the water to penetrate it?—I am of opinion that pressure produces no effect whatever, except to improve the insulation, and the tests obtained from Newfoundland, I think, distinctly answer that question. Had the water penetrated the gutta-percha, the resistance of the cable would have been rendered less by the escape of the current where the water entered. The resistance was greater after submersion.

2948. (*Mr. Saward.*) When you speak of results obtained from Newfoundland, do you mean the more recent ones, or the first accounts that were obtained?—Those obtained when my resistance coils reached there before the fault showed itself.

2949. Not the results of experiments tried in the month of October?—They gave no results; they have not yet reached the fault in Trinity bay; it was found that the cable offered a resistance of 1,590 miles. Now, had the water penetrated, the current would have escaped, and the resistance must have been much less. There is room for so little variation here, that it is very clear, from the time of the submersion of the cable up to this period, which was the 17th of October, that the insulation was more perfect than when the cable was first submerged, except at the fault, because the temperature was lower. Had it materially been impaired by the water penetrating, those results would not have been obtained. I think those results show most distinctly and most satisfactorily that pressure does no harm.

2950. (*Mr. Edwin Clark.*) Would not the actual condensation of the bulk of the gutta-percha, by allowing the water to come nearer, have any effect?—That would increase its inductive capacity perhaps, but this is very doubtful, and it would probably improve its insulation. I do not see how water can penetrate a soft substance under pressure; we do not find, from the great pressure we are subjected to, that the air penetrates us to any great extent. For the same reason we ought to be penetrated by air; we have a certain amount which all fluid absorbs, but we do not find that we have any more in us.

2951. (*Chairman.*) If gutta-percha contains water, and you subject it to a very considerable pressure, will not the particles of water be forced into more immediate contact with each other, and therefore is not the action of electrolysis more likely to take place? I think all the forces balance themselves; for instance, if you send down a bladder of india-rubber filled with water to the bottom of the ocean, the water is slightly compressible, and the bladder will yield to that compression, but there is nothing to cause the water to force itself through that bladder, because the power to do that is measured by the elasticity of the bladder itself, and not by the depth; the power on each side of the india-rubber will be equal, and you may consider the water in the pores of the gutta-percha to be water in a small bladder of gutta-percha, in a bladder of soft substance which can yield as the water compresses. As the gutta-percha contains air it will compress; this ought to increase its insulation, and slightly the inductive capacity; the water will not, in my opinion, enter the gutta-percha, which being elastic, will yield and close its own pores.

2952. When a cube of gutta-percha is placed in

cylinder filled with water and exposed to very high pressure, does not the fact of a certain amount of weight being acquired by the gutta-percha, tend to show that some water has been entering into it?—I think that is only at the surfaces; gutta-percha is porous, and as it is manufactured it contains water and air.

2953. If it is at the surfaces, are not any particles of water which exist in the gutta-percha at the time it is manufactured, more likely by pressure in the ocean to be forced closely together, so as to give rise to the subsequent effect of electrolysis?—I cannot see why it should be so. I can understand why the pores near the surface containing air should be filled with water by the water running in, and the air being absorbed by the interior of the gutta-percha; the air will be compressed, it is soft and will yield. While your experiments are going on, it would be well worth while to take a cube of gutta-percha, submit it to great pressure in water, and then cut off the exterior to ascertain the specific gravity of the interior. I believe you will find that the specific gravity of the interior is not changed.

2954. (*Mr. Saward.*) Would a cube be a fair comparison with the gutta-percha in a cable, seeing that from the arch of the cable there would be more likely to be an equal pressure all round?—It is the same thing whether the gutta-percha be a cube or a cylinder; it is too soft for shape to affect it much.

2955. (*Chairman.*) Mr. Macintosh mentioned having exposed gutta-percha, in the interior of which was potassium to very high pressure in water, and that the decomposition of the potassium was caused after a certain number of hours' exposure to the pressure?—Gutta-percha so compressed containing potassium would be compressed close on to the potassium and would touch it consequently uniformly all over. Gutta-percha as manufactured contains water and wherever any moisture comes in contact with potassium it will decompose it; in fact I almost expect that the potassium itself ought to some extent to attack the gutta-percha.

2956. The time which the potassium resisted the decomposition was dependent upon the thickness of the gutta-percha in the different specimens, as if the water took a certain time to penetrate?—I have not seen that experiment and cannot understand how such a result can be obtained; but in the case of a cable you have also to bear in mind that the exterior is covered with tar; that will be the first thing to penetrate the gutta-percha. You have the fact that at the bottom of the Mediterranean at a depth of 1,500 fathoms, one of the four wires of the Bono and Cagliari cable remains good in a very thin coating of gutta-percha and has worked well for years; that indicates that the gutta-percha has not been materially, if at all, impaired in insulating power. The experience with the Atlantic cable itself seems also to indicate that no such result has taken place. I do not myself see why water should penetrate gutta-percha as it is a soft squeezable substance, capable, under great pressure, of filling up any pores that exist in itself.

2957. Have you had any experience with india-rubber covered wire?—We have a great many miles of that wire on our lines. The first we put up was in 1847 on the South Devon Railway; on that line we had to work Wheatstone's dial instruments by means of magneto machines, for starting and regulating the atmospheric trains between Newton, Teignmouth, and Exeter. The open line wire insulated so badly, owing to the sea washing over it, that at times we were unable to work through from Newton to Exeter. The first thing I did was to reduce the resistance of the coils of the instruments. The length of the line was 20 miles, and the resistance of the coils of the instruments themselves each offered a resistance equal to 30 miles, and there were eight instruments in circuit. I had a thicker wire put on to the coils, which reduced their resistance to that of a mile; so that the eight instruments gave a resistance of about eight miles in a line of 20 miles. The magneto machines were also wound with a thicker wire so as to give a current of lower tension but greater volume to suit the instruments. We were immediately enabled to work a great deal better, but still at times we could not get through with certainty, owing to the sea washing over the insulators. An india-rubber covered wire was then erected over the 10 miles exposed to the sea, it being suspended from one of the iron wires by means of little bands of gutta-percha; when it was first put up it insulated very well and we worked very satisfactorily, but as soon as the summer's sun began to act upon the india-rubber it fell off and hung in festoons. The india-rubber changed in appearance, had swollen considerably and lost its elastic character to a great extent, and become much more gummy. During the first two months of a hot summer nearly all the india-rubber was found lying on the ground. The wire, however, in the tunnels remained good, and when I came away from that line in 1848, when the atmospheric system was discontinued, I brought some short pieces of wire to London and buried them in a garden, and some of those wires lost their insulation altogether while others retained it. In some specimens the india-rubber decomposed near the wire, but not to the extent that the more modern india-rubber decomposes; this was chiefly at the joints where cotton was not used. In the early use of india-rubber the wire was covered first with cotton, the cotton carefully varnished with shellac and the india-rubber put on. In those instances the india-rubber never seemed inclined to decompose in the interior, excepting at the joints where the cotton and insulating material were not so carefully applied. Shellac seems to prevent the decomposition of india-rubber. A great deal of india-rubber covered wire was used in the tunnels on the Lancashire and Yorkshire line and also on many other lines, but nearly all of it failed from the wrappings not being properly united. The wire after it had been in the tunnels for a period of a year or so, where it was alternately damp and dry, seemed to lose the adhesion between the coats, and you could very frequently unwrap the wire that before could not be unwrapped. Those wires, as far as my memory serves me, in no instance insulated so well as gutta-percha covered wires, but this I attribute entirely to the want of union between the coats. From subsequent experience with india-rubber, and experiments I have made on wire supplied by Messrs. Silver, and testing sheets of india-rubber, I have found that india-rubber insulates from 45 to 70 times as well as gutta-percha, in some specimens the insulation is very much greater, its softness, I am afraid, will considerably interfere with its being used as an insulator for telegraphic cables, as it has not sufficient power to resist the ill-treatment it undergoes, unless some means are devised of bringing over the india-rubber in its natural state, and coating the wires with it. In that case an insulator would be obtained far superior to gutta-percha, very tough, sound, and not liable to decomposition.

2958. You would insulate the wire with india-rubber before it had arrived at the bottle condition of hardness?—Yes, as it comes from the trees; that rubber is very firm and not liable to decomposition, and it insulates admirably. With a wire covered in that way by successive layers, you would have a great many thin coats which would insure very perfect insulation, added to which the inductive capacity of the india-rubber is much lower than that of the gutta-percha, and consequently the speed of transmission through an india-rubber covered wire would be much higher than that through a gutta-percha covered wire.

2959. You have tested Mr. Wray's material, have you not?—I have tested a great many specimens of his material.

2960. Is it not composed of india-rubber, shellac, and powdered flint or india-rubber, shellac, powdered flint and a little gutta-percha?—Those are the compounds he prefers; he has brought me com-

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pounds with various materials, some with lime which does not do at all, some with chalk, French chalk and different things. I have tested them for him at different times; and some of his specimens tested remarkably well, so much so that after I had charged the lengths he gave me, which were very small, they retained their charge so long that I could not say that I had succeeded in discovering any leakage through them. One piece which I charged retained the charge in it a fortnight. The material composed of flint, shellac, and indian-rubber insulates in an extraordinary manner, 6,000 or 7,000 times better than ordinary gutta-percha. His material 15 X, which contains a little gutta-percha, insulates only as well as indian-rubber, or 300 or 400 times as well as ordinary gutta-percha.

2961. How is that as compared with gutta-percha?—The method that I use for testing the insulation of a material is to put a charge into it of a certain tension and note how long it takes for that charge to run down to half its tension.

After you once charge a wire and connect its end with earth it is never discharged entirely, mathematically speaking, therefore you must take a certain point; the point I take is from a given tension down to half that amount. Ordinary gutta-percha, such as the Atlantic strand, runs down to that amount in about one to one and a half minutes. Wray's best material, I should think, stood a week at that pitch and would have stood longer if the ends could have been perfectly insulated.

2962. How did you insulate the ends to prevent the electricity passing off into the air?—I covered the insulating material (not the wire itself) with a mixture of shellac, resin, and a little turpentine.

2963. Are the ends of the wire exposed?—Yes, dry air does not conduct at all.

2964. You kept it in a dry room?—Yes; these experiments can only be made when the weather is very fine. When I got these results I had not a good arrangement for making the experiments; the wires after being charged were dipped into water, having dried them and stood them upon a table the question was how long will it take for the disturbed electricity, that is the extra quantity in the interior and the less quantity on the exterior, to unite. These lengths stood in a room exposed to the south during the hottest weather of the summer; the temperature in that room sometimes rises over 100°. I am judging now from the temperature of the room, not from the thermometer. I know it is insufferably hot and these lengths were exposed to that temperature in which I got these remarkable results; but I had often had charges in them which had not fallen to half their original tension in six and seven days, the wires being in water. The materials I mention, shellac, resin, and a little turpentine, seem to repel moisture most successfully, and cause the ends to insulate very well while the composition is new, and by frequently renewing it you are enabled to keep the charge in without any sensible loss. The way I detect loss over the ends is this, I use a comparative test. If I have a piece six yards in length I charge also a piece a foot in length the ends are all insulated and if the ends leak at all that amount of leakage will produce a greater impression upon the charge in a short piece of one foot, which has the same facilities for leaking. The result would be as 1 to 18. Consequently if the tension of the charge of one foot is nominally the same as the charge in the six yards I assume that no leakage has taken place over the ends, and that all the leakage found is due to the material itself.

2965. (*Professor Wheatstone.*) You have stated the comparative results between experiments made with Wray's compound and gutta-percha; have you made a similar comparison with india-rubber?—Yes; with wire sent me by Messrs. Silver and Co.; among them a piece containing 100 joints, and which insulates not less than 70 times as well as gutta-percha probably. I also tried Chatterton's double-covered wire, such as we have in the Zandvoord cable; with that wire I get an insulation about twice as good; that is to say, it takes a period of about two minutes, or rather more, to run down from any tension to half that tension.

2966. (*Chairman.*) How does Silver's india-rubber covered wire test?—Some specimens I have tried have shown a difference, compared with the Atlantic strand, of 1 to 70, and others 1 to 45, and some higher still.

2967. Have you tested the 20 layer gutta-percha covered wire?—Not yet, for any accurate results; but I think it is four times as good as the two coats of Chatterton's compound—merely speaking from memory, and eight times as good as the Atlantic strand.

2968. Then it does not increase in excellence, in proportion to the number of coats?—No; it improves with the number of coats, but not in the ratio of the number of coats.

2969. Have you formed any opinion upon Herder's method of insulation?—Herder's cable is constructed upon a wrong principle, because he covers the conducting wire with a material that is a semi-conductor, namely, cotton; and the inductive charge is increased by the increased internal diameter of the gutta-percha, the copper conductor remaining of less size than the interior of gutta-percha. Now, had he increased the copper wire to the full size, and done away with the cotton altogether, he would have obtained no more inductive absorption, but a much higher conducting power; the increased dimension of the copper conductor gives a greater speed. Herder has been led into error by using the instantaneous discharges from a Leyden jar, and comparing them with the discharges of a telegraphic cable; in the case to which he chiefly alludes, that of a plate of glass, a plate of semi-conductor, and a plate of glass again, with a coating on the two exterior sides of the plates of glass, he states that the discharge from the Leyden jar was very much reduced by the interposition of this material, that is to say, he retarded the discharge; but, had he used the means of measuring the amount of electricity absorbed, he would have found that the amount of electricity absorbed, when a short space of time was given, was the same as when he had merely the thickness of glass, and that the increased thickness due to the interposition of his semi-conductor was of no use. When, however, you come to a cylindrical conductor, of which a like depth on the exterior of the cable does not produce a like diminution of effect, it is clear that he is using his gutta-percha to very great disadvantage. Added to that, leaving out the inductive question, it is very objectionable to have a porous medium between the layers of gutta-percha, because, should the water penetrate through a hole in any part of the exterior covering, it will gradually run along this semi-conducting porous material, and very likely come across some fault in the inner coating, and thus allow the electricity to escape. The only use of a number of coats on a conductor is to fill up those holes and pores; the more coats you have, the more you fill up those holes and pores; but if you had a material at the onset solid and sound, you would gain nothing by numerous coats.

2970. You do not think that there is any effect produced by the employment of numerous coats beyond the improvement of the insulation, by preventing flaws in the non-conductor?—No, only by filling up the pores. In the case of Chatterton's compound, the compound penetrates the thinner layers to a much greater depth, and then, in place of water, in some of the pores, you have Chatterton's compound, which is a non-conductor, consequently you fill up the pores of the gutta-percha with a non-conductor, I believe, of lower specific inductive capacity, and consequently you improve your cable in that respect; that is to say, you improve your cable, not only as regards its inductive capacity, but as regards its insulating power.

2971. Do you consider that Wray's material will be durable?—The materials used, shellac, india-rubber, and powdered flint are not likely to act upon each other. The pieces of wire which were hung up on the

South Devon line, to which I alluded, in which the india-rubber was placed over cotton, the cotton being coated with shellac, showed almost in no instance any sign of decay where not exposed to sunshine; consequently I think that the shellac and india-rubber are not likely to exert any injurious action upon each other; on the other hand, shellac beyond all doubt is a great preserver of india-rubber. The third material, powdered flint, when that is used in the compound, is also a material that is not likely to act upon either of those substances. I have had some specimens of Wray's wire some 12 or 13 months, and they do not seem to have lost their insulating power in any way, nor to be affected by light or heat to any visible extent. I shall not be able to test those specimens satisfactorily, owing to their great insulating power, till the summer arrives, because it is impossible to maintain the insulation with sufficient perfection to detect with certainty any increase of leakage through them. Some of the specimens Wray has given me, do not insulate well; but four or five of them, and one particularly, in which there is india-rubber, shellac, and powdered flint, insulate extremely well, several thousand times better than gutta-percha; one in which there is French chalk, insulates very well; one in which chalk was used I think insulated only about four times as well as gutta-percha; and some of the other compounds that he gave me, for he gave me a great many, did not insulate well, compared with his high-class specimens. The compound of shellac, india-rubber, and powdered flint, was the best.

2972. (*Mr. E. Clark.*) Was not the principal cause of the failure of india-rubber owing to the separation of parts of the band, so that it became unstuck and unwound?—In tunnels that was the cause.

2973. You have stated that where there was cotton, the india-rubber lasted longer than where there was no cotton; I cannot see how the cotton affected the question?—It did not affect the question of the want of union between the layers; I was speaking of the decomposition which modern wires undergo so much, which shellac prevents in a great measure.

2974. (*Professor Wheatstone.*) Not all specimens?—No, but many of them. I do not think india-rubber will be safe until it is used in the way I have stated; that is, before it has been through the mill. In going through the mill its nature is entirely changed, and I do not believe that it is any longer the same substance that it was originally.

2975. (*Chairman.*) You would have india-rubber imported in its natural state direct from the trees, and then applied to the copper wire, much in the same way that gutta-percha is applied, namely through dies?—No, not in that way. India-rubber has frequently been imported into England as it comes from the trees in a liquid state; and I would recommend running the wire through it, so as to paint it coat after coat, until it had attained the desired thickness, exactly in the same way that the bottles of india-rubber are made by the natives abroad. In that way I believe that india-rubber would be found a most valuable insulator.

2976. Has india-rubber ever been applied in that way yet?—Never to telegraph wires. Professor Wheatstone informs me that he has dipped his hands into the juice, and let it dry, when he has taken off his hand a delicate glove of pure "bottle" rubber; this fluid was imported in a cask.

2977. You have doubtless observed the effects of lightning on underground and other wires; what has been the result of your observations?—Wherever underground-wires are used in connexion with forming part of overground circuits, they are sure to be destroyed by lightning in time. In the case of a line in which there are many wires, the lightning strikes the top-wire, and then in numerous sparks strikes all the other wires. It then, to a great extent, makes its escape at the poles, splitting them, and sometimes knocking out pieces as if they had been hit by a pickaxe; at other times splintering the poles, and throwing the splinters to a distance of even 200 yards; and, generally speaking, a portion of the current traverses the line to the distant end, burning up such instruments as it meets in its way; or should it meet with a tunnel in which there is a gutta-percha covered wire used, or should the wires go underground covered with gutta-percha, it almost invariably punctures them. I cut out, two years ago, a fault from the Kilsby tunnel, in which there was a minute hole right through the gutta-percha, and through Mr. Edwin Clark's covering of tarred and sanded tape. The material was charred, and an electric connexion established between the conductor and the roofing of zinc which covered the grooved boarding in which the gutta-percha line was located. That made a very bad fault; and upon removing the tape, there was a hole, about the twentieth of an inch in diameter, through the gutta-percha, presenting all the appearance of having been burst by lightning. It did not present the appearance of that piece of wire which lies upon the table. That I attribute chiefly to the fact that the wire was covered tightly round with tape, which held the gutta-percha in position to prevent it from bulging out by the explosion.

2978. You said that you anticipated that any subterranean or submarine wire connected with a subaerial wire, would be subject to destruction by lightning. Do you think that no arrangement can be made at the junction of the two wires, by lightning conductors or otherwise to prevent that destruction?—In 1847, I made a lightning conductor, in which the wires are brought very close together in an exhausted chamber; and upon testing those, I found that two feet of the exhausted chamber offered the same resistance as $\frac{1}{8}$ inch of atmospheric air; and yet this offered sufficient resistance not to allow a current from 400 cells of Daniell's battery to pass across the wires in the exhausted chamber, being very close about $\frac{1}{50}$ inch apart. Consequently you get by this means, I think, the nearest approach to a perfect lightning conductor. Some of these lightning conductors have been in use upon our lines; and in one case, at Rugby, owing to not anticipating the result I am about to describe, the machine was improperly constructed. Ten wires were led up into the exhausted glass through a slate tablet; those were all cemented in and the glass was made tight by filling round it with cement, and covering over the tap that had been used for exhausting the glass with the same soft cement which had been carefully deprived of moisture. A little vacuum gauge was placed inside the glass to show that the vacuum was maintained. A heavy flash of lightning struck the wires on the Trent Valley line, and was discharged through the lightning conductor in the Rugby office; the wires were heated by the discharge, and the heated wires melted the cement which was violently projected inwards by the pressure of the atmosphere coating the glass and filling the vacuum gauge. None of the instruments were injured, and as far as that experience goes, which is the only experience we have had of a heavy discharge going through the instrument, it answers perfectly. In the case of a submarine cable, I should always recommend the introduction of at least three or four miles of underground wire between the cable and the land line, the discharge would be most likely to take place in it to the earth and save the cable. This is not, however, always the case, for in sandy or rocky districts the ground is sometimes struck, and the ground being dry offers so much resistance to the discharge that the lightning bursts into the gutta-percha, and comes out again at some more distant point. As underground wires are sometimes destroyed by lightning, so underground wires connected with a cable might be destroyed by lightning; but the safest means of protecting a submarine cable is to solder a thick wire to every individual wire of the exterior covering of the cable, and to use those wires as the earth connexion of your lightning conductor which should be a vacuum one such as I have just described. Then if the ground itself be to a

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certain extent an insulator, and lightning flash into your wire, it will go to the exterior of the cable at once, because that is the best connexion with the earth at that point; and having charged the exterior of the cable for a moment, which it will do, at that very instant which is the dangerous moment, the interior of the cable has no power to take in electricity, as Faraday has shown by an insulated jar, in which, when you place a charged ball, and let that ball touch the interior of the vessel, the electricity immediately escapes to the exterior; and upon taking that ball out, there is not a trace of electricity there. Therefore, the exterior metal covering of the cable is the next earth connexion that can be selected for preserving the cable. I would, however, recommend that, at the end of the length of underground wire of five miles, where it joins the land line, you should also have another lightning conductor. There is then a double chance, and that is very important in submarine cables. However, should the submarine cable be destroyed by lightning, it will probably take place within a distance of 15 or 20 miles at most, at which repairs are easily performed.

2979. It has been frequently stated that a cable when coiled up shows different results as to speed to when it is stretched out in a straight line, are you of the same opinion?—No; theoretically that is correct, practically I believe that the amount is too slight to be of importance. The experiment which I mentioned to-day proves that the only way in which it could produce an action would be by the magnetism generated by the interior wire magnetising the exterior iron coating. When coiled the magneto-electric current is greater than when not coiled. I have frequently noticed this current in short lengths of cable; it is in the opposite direction to the "discharge or return current." Its tension is so feeble that it is unable to produce any material effect compared with the retardation due to electrostatic induction; this is proved by my experiments already quoted. As regards the electrostatic induction there is no difference whatever, whether the cable be coiled up or in a straight line.

2980. Do you think that the proposed shore end of the Gibraltar cable is stout enough?—No.

2981. What size would you give it?—The size I would give it would be not less than ten tons to the mile.

2982. For what depths?—For depths under 60 fathoms.

2983. From 60 to 100 fathoms what weight would you recommend?—There it matters but little. I should prefer carrying it on to 100 fathoms with not less than six tons to the mile. For this reason: the Dover and Calais cable and the Dover and Ostend cable have been broken four or five times by ships' anchors, and the more especially because since they have been down they have been to a certain extent eaten away by rust which has reduced their weight. A cable might be dragged for by an enemy, and it ought to be heavy enough to make it difficult to break. Formerly, when the Dover cables were new ships, hung on to them for a long period without deranging them at all; and therefore the cable ought to have iron enough, so that after a period of eight or nine years rusting in the sea it shall not be reduced in strength to a less amount than that of those cables. I should therefore not recommend a shore end of less strength than ten tons to the mile, the more the better; and this does not add materially to the cost of the cable, as the shore ends bear a small proportion to the whole of such a line.

2984. You say that up to 60 fathoms depth you would make a cable ten tons weight and up to 100 fathoms you would make it six tons; would you not have great difficulty in picking up a cable of that weight?—I imagine not; a cable of that weight in the Mediterranean, with proper machinery, has been recovered from greater depths. I do not mean Newall's slender one, but Brett's stout one.

2985. Not of that weight?—Yes, a greater weight; it was Mr. Brett's cable; they reached 80 fathoms in crossing from Port Patrick to Donaghadee and they picked up that cable. Newall lost it and recovered it two years afterwards. It is merely a question of steam power. If a cable weighed 100 tons a mile, with sufficient engine power you could bring it up. In fact the heavier the cable the less liable is it to break.

I should mention with regard to the action of the layers of a cable one upon another that I have, in conjunction with my late brother, invented an apparatus for finding out the position of a fault in a cable without cutting it, by means of the magnetic rays emanating from the wire when currents are alternated through it. When currents are made to alternate in the wire, they produce at right angles to the current magnetic action, and I have made horse-shoes of iron which are covered with wire to go over the exterior of the cable. The wires on these iron horse-shoes are wound parallel to the conductor in the cable. By this means when a current passes through the wire it magnetises these iron horse-shoes; and these iron horse-shoes induce currents in these coils of wire, which currents, by means of delicate galvanometers, are rendered sensible without using a very excessive power at the end of the cable. I have tried experiments upon short pieces of cable, in which I have used 20 cells of Daniell's battery on a cable with a resistance equal to 20 miles, and upon making an artificial fault by driving a nail into the cable the currents were sent in from one wire and escaped at that defect.

2986. (*Professor Wheatstone.*) Escaped into the water?—No; the iron ends of the cable were both connected with earth. Upon applying these horse-shoes or "probes" on the exterior of the cable between the battery and the fault the indications were shown very distinctly. On having the cable pulled through the probe we immediately saw on the galvanometer when the fault passed, and so picked out the very spot at which the fault existed. But these horse-shoes are of more use in the manufacture of cables than in the repair of cables, because it very frequently happens that a defect takes place in the manufacture of a cable, and they are obliged to stop the whole of the machinery, as in the case of the Zandvoord cable, which had to be cut in eight or ten pieces; then there were splices to be made, each splice consuming 50 feet or more of cable; the wasting many fathoms of cable which were in the short piece to which the fault had been reduced. If I had had such an apparatus in existence, we might have tested, first of all, for the locality of the fault, and then applied it to flake after flake till we came to the right one, and then till we came to the exact spot. We could then have cut out that one spot, instead of making, perhaps, a dozen cuts.

2987. (*Professor Wheatstone.*) Have you practically applied that process?—I have not employed it on a cable, but I have made use of it for discovering defects on land lines.

2988. (*Chairman.*) You have only applied it out of water?—I have not had an opportunity of applying it out at sea, but I have made arrangements by which to endeavour to do so. I have arranged a table that shall stand very steadily at sea, so as to be very little influenced by the motion of the ship.

The accompaning extracts of our specification will explain:—

"Now know ye, that I, the said Cromwell Fleetwood Varley, on behalf of myself and of the said Cornelius John Varley now deceased, do hereby declare the nature of our said Invention, and in what manner the same is to be performed to be particularly described and ascertained in and by the following statement and accompanying Drawings, that is to say:—

"The chief object of our Invention is to find out the locality of defects in the insulation of a conductor, or imperfect continuity of the latter, without removing the

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covering or the insulator, or cutting the conductor for testing. We generally first ascertain approximately the locality of the fault by the following method:—For example (vide Fig. 1), suppose the defective conductor to be a cable in the maker's stores, and under water; we pass the current from a voltaic battery C, Z, through the two wires of a differential galvanometer *g* in opposite directions, one pole of the battery being connected to the earth or water, and the two ends of the wires from the differential galvanometer *g* connected with the two ends of the conductor at S, S, whose insulation is defective. The current from the battery divides and goes round the galvanometer in opposite directions, and, entering both ends of the cable escapes at the defect in the insulator. If the defect is equidistant from the two ends, *i. e.* at B, the resistance will be equal, the currents round the galvanometer will be equal, and the needle will stand at zero. If the fault be not in the middle of the cable, but located at *l*, the resistance will be unequal, the shorter rout *y* will allow the passage to more electricity than the other *x*, and the needle will be deflected. We now introduce a rheostat *r* into the shorter circuit *y*, and add so much resistance as shall make the two routes indicated by the arrows equal in resistance. Having noted this, and the resistance of the whole conductor, the following formula will show very nearly the exact locality of the defect:—

Resistance of the whole conductor	$=S$	then	
„ of longer portion	$=x$	$x+y=S$	
„ of shorter portion	$=y$	$x=y+r$	
„ of rheostat	$=r$	$S-r$	
		2	$=y$

"The peculiarity of this mode of testing is, that the ever varying resistance of a damp fault does not affect result. The arrows indicate the supposed direction of a negative current, for ease of explanation, N.B. as a negative current, both attracts moisture to the fault, and keeps the metallic surface in an unoxidised state. It is nearly always the best current to use for testing. A positive current causes the resistance at *l* frequently to vary very much, owing to its oxydising action. This operation is not always used, especially when the cable is short.

"Having ascertained the approximate locality, the actual spot is found by our second improvement, which we call probes, vide Figs. 2, 3, and 4. Fig. 2 is a side view of one of our forms of probes, containing a portion of a cable under test; Figs. 3 and 4 are end views and sections. *a* represents a portion of cable containing five wires, the five conducting wires being covered with gutta-percha, hemp, and iron, as usual; *b*, a saddle-shaped piece of soft iron wrapper with insulated copper wire *c*; *d*, Figs. 2 and 3, flat bars or plates of iron fitting against the flat surfaces of *b* at *e*, *e*; these should be fitted accurately. *f* and *g*, the two ends of the wire coil *c*; *h*, Figs. 3 and 4, shows a protecting lining for the wire *c*. Fig. 4 shows another form of probe, in which two saddles of iron *b*, *b*, are used, fitting against each other, united by hinges, the pin at one side being withdrawn, to open or to close them securely when re-inserted. When a current of electricity is flowing through a conductor, it produces magnetic influence or rays throughout the length and outside of the conductor, we apply outside the cable *a*, *a*, our probes, consisting of coils of wire, iron bars, or horse-shoes wrapped with wire, as above described, to be acted on by the magnetic power of the current in the conductor. Every time a current runs through the conductor, or ceases to flow or is reversed, the magnetism in the probe is disturbed, generating weak electric currents in the coils *c* of the probes. These weak currents are rendered visible by a delicate galvanometer, which is included in the circuit. We generally employ a reversing apparatus similar in principle to that first invented and patented by Cromwell Fleetwood Varley in 1854, for working printing telegraphs. We cause alternating or intermitting currents to enter one end of the cable, and escape at the leak, or we cause currents to enter both ends, but at different rates of alternation or intermission; in the latter case, on applying our probes outside the conductor, we see immediately by the speed of the deflections on which side of the fault the probe is. The probe or probes are then moved along the cable towards the leak until, by the varying speed of the deflections, we discern that the fault has been passed. When the intermittent or alternating currents are applied to one end only of the cable the other end is insulated; the currents then run along the conductor to the fault, and there escape. Our probes then show, by the existence or non-existence of currents in their coils, on which side of the fault they are. Having traced out the locality of the fault, a Spanish windlass is made, or the cable is cut at the spot, repaired, and spliced as usual; thus, by a single cut or splice, the fault is removed. Hitherto, to find such a fault, it has been necessary to cut and splice

Fig. 3.

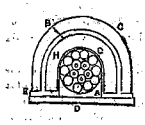


Fig. 2.

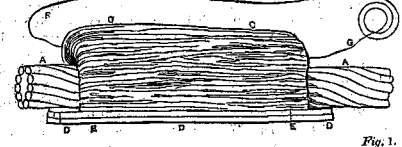


Fig. 4.

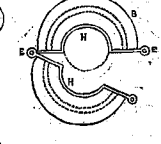
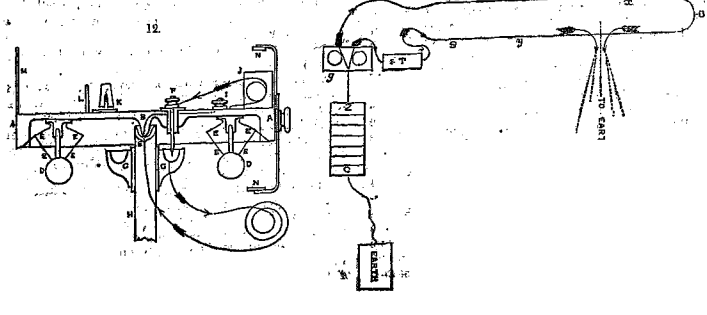


Fig. 1.



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the cable in many places before the locality of the fault was traced out. A splice takes from 2 to 10 hours to make, and frequently wastes from 20 to 30 or more feet of cable, besides causing great delay, expense, and injury. When the currents are feeble we apply many probes, extending over 12 to 20 feet of the cable; their combined action gives greater deflection to the galvanometer. One foot length of probe is generally sufficient with moderate battery power, but when the cable exceeds 10 or 20 miles in length we use greater length of probe. Many other forms of probe may be used to effect this purpose. It is not absolutely necessary to complete the iron circuit by the piece *d*, Figs. 2 and 3, but so doing increases the power considerably. We use a reflecting galvanometer for showing the currents generated in the probe coils *c*, and we have made the following improvement in the galvanometer:—We make the reflector of hardened steel magnetised; this reduces the weight of it, which is considerable when a silvered glass is used with a magnet attached to it."

"When a conductor is broken in the insulating envelope, and we wish to ascertain the approximate distance of the break without burning open the insulator, or where this cannot be done, we measure the distance by the amount of induction in the following way:—A rheostat or bobbins of resistance are prepared free from magnetic induction or retardation; this we effect by winding the rheostat with two or more wires, or a doubled wire, and cause the currents to flow in opposite directions round them an equal number of times. This removes the momentary irregularity of their resistance arising from magnetic induction. We prepare also a series of induction plates, whose action is similar to that of extensive Leyden jars of known and suitable dimensions; these induction plates are added to the coils of the rheostat, so as to give them induction corresponding in amount to that of an equal length of cable. The following example will explain their use.

"Suppose a wire broken in the gutta-percha, and thereby insulating perfectly, by passing the current of a battery round the wires of a differential galvanometer as much resistance and corresponding induction surface are added as shall produce equilibrium. This measures with tolerable precision the amount of induction or charge and discharge of the cable, and consequently the distance of the leak in the conductor. This can also be used to test the locality of a defect, which gives great resistance, but not complete insulation as before assumed. As these induction plates are bulky, we prefer winding our differential galvanometer with many wires, say 11. We then connect the cable to one wire, but the rheostat to 10, in one continuous circuit; the resistance of the latter is increased in proportion, *i. e.*, 10 times, while the surface of the induction plates is reduced to one-tenth, and thus rendered less cumbersome and expensive. On making and breaking contact, if the inductive absorption of the cable tested be 10 times as great as that in our induction plates, while the resistance of the induction plate circuit is 10 times as great as the cable wire, the electricity rushing in to charge the cable will be 10 times as great as that into the induction plates, but as the latter circuit is of 10 times the resistance of the cable, the time of the chargings will be equal. Lastly, as the smaller charge or current circulates 10 times as often round the differential galvanometer coil as does the cable charge or current, but in an opposite direction, they exactly neutralise each other, and the needle remains at zero. Thus, the inductive charge is measured, and the distance of the defect shown.

"In testing for a defect in a cable with the probes, when out at sea, great difficulty is experienced from the motion of the ship and the feebleness of the currents, we obviate much of this by supporting the galvanometer or indicator on our sea table of the following construction. Fig. 12 explains the principle on which they are constructed. *a, a*, the table, having at its centre of gravity a point *b*, by which it stands in a cup *c*; this table when truly balanced will neither be twisted nor thrown out of its horizontal position by any motion of the ship. The table is retained in the horizontal position by means of short quickly oscillating pendula *d, d*, of which there are 3, 4, or more. The pendula *d, d*, are suspended from an imaginary plane or circle coinciding with and equidistant from the centre of gravity *b* of the table; these pendula hang somewhat like scale pans below their beam. From the pendula *d, d*, are springing connections *e, e, e, e*; the pendula being free to move in any direction. These springs bring and keep the table in the horizontal position; any motion sets them oscillating rapidly, and as their springs are feeble compared with the inertia of the table, no motion of the latter is perceptible. Thus we get a steady table at sea. The pendula are as near as possible alike in time of oscillation, and are connected by a light frame (not shown in the Drawing, but easily understood), which merely secures their simultaneous action. 'It must be borne in mind that cables are never repaired in rough seas, when such a table could not be used.'

"We propose, where necessary, to attach gyrascopes to the table, which may be kept in motion by a train of wheels. *f* is a terminal with platina wire insulated from the table, and dipping into the ring cup *g*, which is insulated from the post *h*, on top of which is the cup *c*; these cups *c* and *g* are partially filled with mercury. The currents from our probe travel as shown by arrows, viz., up the post *h*, through the cup *c*, the point *b*, and the terminal *i* to the galvanometer *j*; back again through the terminal and platina wire *f* to the circular trough *g*, and thence to the other end of the probe coil. *k*, a lamp, with a shade *l* to hide it from the screen *m*; its light after entering the galvanometer is thrown back by the reflector to the screen *m*, on which it forms an image or spot of light.

"We prefer using two galvanometers wound in opposite directions and throwing two images near each other on the screen, and thus when a current is passing, their oscillations, due to the current, cause the little reflected images to approach or recede; thus the oscillations caused by the electric currents are readily distinguished from those caused by the motion of the vessel. *n, n*, adjusting magnets to neutralize the earth's action, and so make the galvanometers much more sensitive. When we do not use reflecting galvanometers we use an ordinary astatic one, but as one impulse is seldom sufficient we reverse the connections of the galvanometer with the probe as often as we reverse the battery connections with the cable, and thus cause the alternate currents of the probe to circulate round the galvanometer in one direction, their combined action giving a larger deflection. These reversals are produced by any of the well-known commutators. When using such an instrument at sea, we propose to station a clock, beating half-seconds, on shore at that end of the cable to make the reversals, and also a clock on board beating not quite half-seconds; and thus there will be a given number of currents sent round the galvanometer in one direction, then a pause, followed by a reversal in the direction of the currents, then the slow oscillation of the needle from side to side will show the existence of currents.

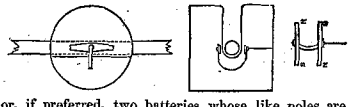
"The modus operandi at sea.—Suppose the cable broken, we first, by testing, get an approximation to the locality. Clocks with the reversing apparatus are then attached to one or both ends of the cable, we prefer the latter; in this case the clocks are constructed to send reverse or alternantig currents from their batteries to the cable at unequal intervals, say, the one reverses the current at every half-second, and the other at every second.

"The ship is sent as near as possible to the estimated spot to grapple for the cable, when this is found and brought up it is often very taut. The sea table, set if lifted on to its post and adjusted, the probes are then applied outside the cable, when, by the speed of the oscillations of its needle, it is seen on which side of the break the ship is. The cable, with buoy-

attached, is then lowered, and the ship proceeds in the direction of the fault, grapples again, and applies the probes; this operation is repeated till the fault has been passed, which is shown by the oscillations coinciding with the other clock. The value of this will be apparent, since to splice a heavy cable takes from 8 to 12 hours, and consumes much cable.

"When the cable contains a defect, but has not parted, it may be under-run, and the probes applied throughout this operation, when the passage of the fault through them will be instantly indicated. These probes are also useful in tunnels on lines, and elsewhere, to indicate the position of faults without cutting the conductor. When there is no iron covering, nor iron near the conductor, an astatic needle, in a case, is applied to the outside of it as shown in Fig. 13 a side view, and 14 a top view, 15 the astatic needle separate; and thus by the deflection of the needle the passage of a current is indicated;

Fig. 14. *Fig.* 13. *Fig.* 15.



or, if preferred, two batteries whose like poles are connected with the earth, are attached to each end of the defective wire. These cause currents to flow from each end towards the defect, and there escape. On applying the needle, or rather on placing the wire into the grove in the case the needle is deflected, and indicates the direction of the current. On passing the fault the direction of the current will have changed, and the needle will be deflected in the opposite direction. This apparatus enables us to pick

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visible to the eye in the usual way, connect the line at each end to a battery so that similar currents, (say, positive ones), enter the line and escape at the leak as shown Fig. 17, on applying the galvanometer the direction of the current will be indicated. This will show on which side of the fault the galvanometer has been applied. In this way the exact spot can be readily traced out. The strength of the current traversing the galvanometer is shown by the formula,—

$$i' = \frac{R}{R+r}\, i$$

i current traversing the line before the galvanometer is applied, R resistance of the line included between the terminals of the galvanometer wire a and b Fig. 16, r resistance of galvanometer, i' current traversing the galvanometer (or loop) circuit."

I have worked out many formulæ for ascertaining the position of faults, as this is a matter of great importance in the case of submarine lines. The general mode of proceeding in such cases will be explained by the following formula, which, with others, was published several years since for private circulation amongst the Electric Telegraph Company's officers:—

When the insulating material of a telegraph cable becomes defective in any spot so as to admit the water, this fact is ascertained by passing negative currents into the cable, and then positive; if the former give less resistance than the latter, it is a sure sign that the conductor is in contact with moisture at the defect; in such a case, if the exposed part of the conductor be small, the resistance will be great; should the cable contain more than one conductor, proceed as already described, by joining at the distant end a good wire to the defective one, forming a loop, this is by far the best plan, at present known, for accuracy.

Fig. 16.

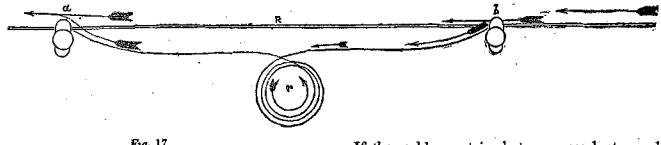
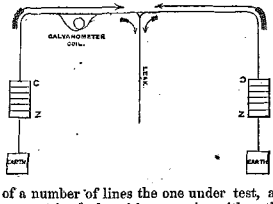


Fig. 17.



out of a number of lines the one under test, and then to trace out its fault, without cutting either the insulating covering or the conductor. This is of much service in tunnels or street work where the copper wire is not covered with iron.

"Where the conductor to be tested is not covered with any insulating medium, for example, a telegraph wire suspended in the air, I adopt the following plan of tracing out what in telegraphic phraseology is called an earth or a contact, when the same cannot be readily discovered in the ordinary manner by the eye. An astatic galvanometer is wound with thick copper wire, about No. 14 gunge, the two ends of this galvanometer wire are connected to the line wire to be tested, as shown in Fig. 16, from 10 to 60 yards apart; any current passing along the line divides one part of it traversing the galvanometer, the remainder continuing through the line. In this way the direction of the passing currents are readily detected. If now, a line have a leak (make earth), and this be not

If the cable contain but one conductor, whose resistance is known, the following plan will eliminate the resistance of the fault:—

1st. Have the wire disconnected at the distant end and measure the resistance, this resistance will be that of the portion of cable included between you and the fault (x), and the resistance of the fault (l). Call this measured resistance $R = x + l$.

2nd. Have the distant end put to earth and again measure the resistance, this will be less than the previous one. Call this r. Call the resistance of the line when perfect S, and the distance of the fault from your end x and from the fault to the distant end y, S therefore equals $x+y$.

In the second case the electricity on reaching the fault (whose resistance is l) divides part going through y to the end of the cable part escaping through the fault l, it divides inversely as their resistances, and consequently these two channels together offer less resistance than either separately; they equal $\frac{ly}{l+y}$; consequently,

$$r = x + \frac{ly}{l+y}$$

$$\text{and } x = r - \sqrt{(S-r)\times(R-r)}$$

in practice for $S-r$ put D, and for $R-r$ put d, and then $x = r - \sqrt{D\,d}$

When the cable is broken in two, measure the resistance offered by the cable, and then with resistance coils and pieces of wire in sea water make up an artificial fault that behaves with various currents of different powers as nearly as possible like the cable.

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This done, measure its resistance, deduct it from that given by the cable, and the remainder is the distance of the leak. In addition to these, there are induction and numerous other tests which an experienced hand can use to find out the locality of any fault of any kind.

When a fault offers a great resistance, and you are testing with the end disconnected, you have the whole of a powerful battery nearly thrown upon the fault, and the electrolytic action changes the resistance of the fault very frequently from 1,000 to 2,000 miles in a second of time on some occasions. In that case you can get no positive result by a test like $x = r - \sqrt{Dd}$. I hit upon a plan which in some cases makes the variation of the resistance of fault of almost no consequence. Supposing this to be the wire you are testing, and you have two wires in your cable, join the two together to get a loop. Measure against your standard the resistance of the whole loop first of all carefully, by applying one pole of the battery to one end, and the other pole to the other end, through a differential galvanometer; then, by putting in resistance exactly equal to the line the needle stands exactly at zero. Having very carefully noted the resistance of the whole circuit, the next thing is to connect one pole of your battery to earth, to divide the current from the other pole through the differential galvanometer, through both ends of the cable, but into that side in which the fault is located; put in resistance coils until the resistances between the fault and the battery are equal by each route. In that case it matters little how much the fault varies in resistance. The question is simply how much resistance you must put in to make the two equal, and from that you can quickly deduce the actual distance of the fault. In one case where the application of my formula $x = r - \sqrt{Dd}$ failed from the rapid variation of l, I got a variation of 40 miles in the estimated distances of the fault. That was the case I alluded to, in which the resistance varied in half a second from 1,000 to 2,000. I could not get within a distance of 40 miles of the true spot, and that led me to the loop test $\left(\frac{S-r}{2}=y\right)$ which, in the difficult case just mentioned gave results whose error could not exceed two miles.

2989. There you require two wires?—Yes.

2990. You must have two wires if the cable is laid?—Yes, wherever you have two wires, it is the best means of all to use.

2991. The two wires need not be in the same cable necessarily?—Not necessarily. During the paying out of the Zandvoord cable one wire failed; when it was repaired the contractors, Messrs. Glass, Elliott, and Company, sent out two ships to pick up the cable, and I was stationed at the shore end to test for them. They ran out to the spot I had indicated as nearly as they could, after considering carefully by the log how much slack had been paid out.

I knew by the tests that the greatest amount of error in that fault was under a quarter of a mile. They ran out to the spot as nearly as they could, fished for the cable, picked it up, and cut it. When they cut it they spoke to me; I had the two wires joined together tested, and told them that they were two and one third miles from the fault. They then spliced up, ran four miles, and cut the cable again; we had told them to run two and a half miles, but as they had got cable enough on board they ran four miles to make quite certain. They cut the cable, and on testing again I found they were nearly two miles past the fault. I telegraphed them to that effect, when they replied; "This agrees exactly with the "test; we have run just four miles by patent log." They picked up rather more than a mile and three-quarters of cable, showing that the test was not more than the sixth of a mile in error, and proving how accurate this mode of testing may be rendered; where the fault gives a metallic earth, which rarely ever occurs.

2992. I presume in that mode of testing you alluded to last with two wires the resistance of each must be nearly the same?—If the cable has been properly tested before it goes down you know the different resistances and allow for the difference. It is of the utmost importance that correct data on the conductivity of the cable be preserved, as was the case with this cable.

2993. And that resistance coils be made for every cable before it leaves the works?—It is not necessary to make resistance coils for each cable, but only to know its value compared with any standard.

Very frequently the batteries applied for working cables offer so much resistance as to materially retard the speed of working; that I have frequently seen.

2994. In what batteries particularly do you find that effect?—I am speaking of Daniell's battery. I assume that no person would be so absurd as to talk of using a sand battery now.

2995. Not with a submarine cable?—Not with any line at all; nothing but Daniell's battery is constant.

2996. Are not sand batteries very largely used by Telegraph Companies?—We have annihilated nearly all our sand-batteries, and converted them into Daniell's.

2997. (*Mr. Saward.*) Are not sand-batteries used at the gutta-percha works for testing?—I do not know.

2998. (*Chairman.*) What battery would you use?—Daniell's battery, taking care that its whole resistance is insignificant. Compared with the cable, the resistance of the whole battery should not be more than one-fifth the resistance of the cable, the less the better, under any circumstances. As you increase the number of plates, so you must increase the dimensions of the surface to maintain the same resistance; that is a matter of great importance. It is a great pity that those who touch upon this question do not make themselves familiar with the laws of Ohm; it would set aside all those absurd discussions which take place upon "quantity" and "intensity." People talk about "quantity currents eating up the wire," and "intense currents flowing with little quantity," and such nonsense as that is uttered by people who ought to be ashamed of it. If they would take the trouble to understand the first few of Ohm's laws, which are very simple and easy, it would set aside all that difficulty and discussion. There is a peculiarity about electric force; it is not like heat or mechanical force. You may have a current of electricity, of a given volume of high tension, which can be utilised differently under different circumstances; for instance, supposing you have 20 small cells, of an inch square connected up in a series intensively, those 20 cells, we will assume, decompose 100 grains of sulphate of copper per hour. Connect those up into one single element of 20 times the surface, and the same amount of zinc will decompose 20 times as much copper; but it matters not how you utilise your zinc, whether in one cell of 20 square inches, or 20 cells of one square inch, if you are working for electro-magnetism, provided you can use your wire 20 times as long, and 20 times as fine as in the other case. You will get the same amount of magnetism from the same amount of surface, no matter how you use it. Frequent mistakes have arisen in this way, as to "quantity" and "intensity," from this very fact: while you get a high magnetic effect, you do not get an electrolytic effect in proportion.

I have omitted to mention one cause of faults in telegraphic lines, namely, mice and rats eating through the wire. Here is a specimen taken out between Shoreditch and Stratford, where a mouse has eaten through the gutta-percha and copper wire; and we have had another case where a rat did the same thing.

Adjourned to Thursday next, at Half-past One o'clock.

Thursday, 12th January 1860.

PRESENT:

Captain DOUGLAS GALTON.	Professor WHEATSTONE.
Mr. G. P. BIDDER.	Mr. EDWIN CLARK.
Mr. VARLEY.	Mr. SAWARD.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

GEORGE SAWARD, Esq., a Member of the Committee, examined.

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2999. (*Chairman.*) You have been for some years secretary of a telegraph company?—Yes, during nine years.

3000. You are now the secretary of the Atlantic Telegraph Company?—Yes, and I have been so since its establishment in 1856.

3001. I believe during the latter portion of your engagement with the British Telegraph Company, you undertook, at the request of the directors, the general supervision of all the company's arrangements?—I did. The directors were disappointed with the electrical condition of the European Telegraph Company's property which they had then just purchased, and in consequence of that, their connexion with the engineer in chief ceased; and they requested me in connexion with Mr. William Powell, who was the sub-engineer, to look thoroughly into the matters relating to that department.

3002. Were not the company in possession of an underground line between London and Dover and London and Liverpool and Manchester?—Yes. The company I represented had purchased the rights and property of a company called "The European Telegraph Company," who were the exclusive carriers inland of the messages arriving from and going to the Continent through the cables of the original Submarine Telegraph Company. Amongst this property were underground lines of *gutta*-percha covered conductors between London and Dover, and London and Liverpool respectively.

3003. This drew your attention to the capabilities and properties of gutta-percha?—It did. The whole underground system purchased of the European Telegraph Company began to fall off in working capability soon after it came into our hands, and at the request of the directors I undertook to examine it. I made personal examinations into the condition of the gutta-percha at various parts of the whole from Dover to Liverpool. I found decay at intervals throughout all the distance. This decay of the gutta-percha had several variations in its character, which were very marked and distinct in different localities. (*producing specimens of gutta-percha covered wire*). In chalky or gravelly soil where the wires had not been sunk a sufficient depth in the earth, it presented the appearance shown in specimen No. 1. In iron pipes laid adjacent to gas pipes and subject to the influence of gas leakage, it assumed the still more resinous appearance of No. 2. In the vicinity of oak trees or even of oaken posts where the wires lay so that the drainage from the oak trickled down to the boxes containing them, and also in cases where for economy the trenches had been dug in the soft hedge bottom, and in other situations favourable to the growth of fungi, it came up when fresh in a soft pulpy state, and afterwards dried up and assumed the appearance of No. 3.

3004. Did the gutta-percha lose its insulating power when it was in the state of specimen No. 3?—Yes, when it came up you might in many instances wipe the gutta-percha off the copper with your finger.

3005. It was perfectly rotten?—It was perfectly pulpy; it had the appearance of a substance in which fermentation was going on, and it was much enlarged to what you see it there; it has now shrivelled up; but in its original state it came up pulpy, as though air were inside, as in the case of fermentation of bread or any other material.

3006. (*Mr. Varley.*) Did not the gutta-percha appear much whiter in that state than in its natural state?—It did; it assumed a dirty yellow appearance. In some of the iron pipes it assumed the bronzed and oxydized appearance shown in No. 4, which I cannot account for in such a way as to give you any information. From these circumstances and from subsequent observation, it seems to me that gutta-percha, which is a carburet of hydrogen, and therefore a substance which in its normal condition and at a low temperature has no affinity for oxygen, is yet capable of acquiring that affinity when it is subjected continually either to a warm dry temperature or to the vicinity of certain other substances. In the case of the wires buried near the surface in chalk, the dry warmth kept up around the gutta-percha during summer will account for its perishing. In the case of the injury from gas, the resinous appearance is evidently produced by chemical change, and would probably arise from the constant presence of impurities due to the gas leakage; but as to the destruction of gutta-percha by drainage from oak and by soil containing fungi, these have been the most fatal and the least accounted for scientifically that I have met with.

3007. (*Chairman.*) Have you had those specimens analysed or examined chemically?—I have not; I have examined them myself to such a limited extent as I was able, but it seems to me, from my own examinations, which have been of course necessarily somewhat imperfect, that this destruction of gutta-percha arising from oak drainage and fungi, takes place by communicating a facility for the absorption of oxygen.

3008. Did you find that the gutta-percha had remained perfect on any parts of the line, or had the whole decayed?—No, it was perfect in parts; but I found it universally the case, as I have described, wherever oak trees or oaken posts drained on to it there it was destroyed. I can mention one very marked instance. In the Edgware Road there were a series of posts and rails placed along the side of the road to protect the footpath, which is higher than the other portion of the road. The wires were laid in the lower portion, and it being a gravelly soil any drainage from the path trickled down in the direction of these wires. We took up more than a mile of the wires, and there was the mark of every post along the gutta-percha when it was taken up; wherever there was drainage from an oak post there there was a piece of decay.

3009. (*Mr. Varley.*) Were the wires laid bare in the ground, or were they protected with anything?—The wires I have spoken of, which were purchased from the European Company, were not laid bare; but they were most imperfectly laid; they were laid in common deal troughs without any creosoting, or any protection whatever.

3010. The wires were not rubbed with anything?—No, they were entirely unprotected.

3011. (*Chairman.*) Can you suggest any remedy to prevent this decay?—I have observed the good effect which the presence of Stockholm tar had in preserving gutta-percha, and the further fact that, in many instances, if the gutta-percha were not too far gone, it would to some extent heal it temporarily.

3012. Do you apply the Stockholm tar by painting the outside of the gutta-percha with it?—I am speaking of the fact, that if the gutta-percha has not gone

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too far you may heal it by rubbing Stockholm tar on the outside. I became convinced that the good effect of the tar was due to its antiseptic properties, and that if it could be hermetically sealed up in contact with the gutta-percha, so as not to be washed away by rains, or interfered with by other circumstances; and also if the wires were sunk deep enough in the earth, there would be no danger of decay to the insulator, and that in that case underground wires, though more expensive certainly at the outset than air lines, would be far cheaper ultimately, owing both to the small cost of maintenance, and the certainty of working. I think this is very important, especially with reference to lines for the service of the Government, because if subterranean telegraphs can be successfully maintained, and the gutta-percha preserved from decay at a trifling cost, their usefulness in connexion with the defences of the country and the advantage of their being *out of* sight would be incalculable. I therefore, after some consultation with Mr. Jeffery, the patentee of the marine *glue*, who happened to be a next-door neighbour of mine, and after causing a good many experiments to be made upon the destructibility of marine *glue*, recommended the Directors of the British Telegraph Company to take up the whole of the line between London and Dover; and after surrounding the insulated wire with a double serving of hemp, thoroughly saturated with tar, I recommended them to pass it through marine glue in the manner which has been already described by Mr. *Henley*, who was the contractor for the work, and who has since availed himself of the same plan, somewhat modified, in laying down the wires of the London District Telegraph Company. That is a piece of the Dover line (*producing a specimen*). The marine glue was applied, and the line was sunk in some cases three, four, and even five feet deep. In the earth, all along the Borough Road, we took it under gas, water, and everything, quite out of the way.

3013. In what year was that?—In 1855.

3014. Have the wires remained perfect ever since?—They have remained perfect ever since, while at the same time the Government wires, branching off the main Dover wires into the dockyards at Woolwich, Chatham, and other places, up to the time of my knowing anything personally of them, were continually giving trouble to the Admiralty, owing to their decay. The Dover wires have cost nothing whatever in maintenance since they were properly laid, excepting from accidental causes. With regard to the line from London to Liverpool, which was purchased from the European Company, it was so much decayed, and the pecuniary means of the company were so scarce, that it could not be repaired in the same way as the Dover line; therefore I advised the directors to allow me to effect an union with the Magnetic Company, for bringing about which I had some facilities at the time. That union was subsequently effected, but before it was concluded I made a close personal examination into the Magnetic Company's underground system. I took a man with me, and opened the ground every 20 miles along the whole course of that system, from London to Glasgow, and some small portion in Ireland as well. I carried out that examination with the permission of that company, in order to satisfy my own mind before I made any recommendation to my directors, and I found no decay in it at all. Their wires were cased in tarred yarn, but not protected with marine glue. The tar was washed out, however, in many places, and in those places decay has subsequently set in; and the company have now decided to take it all up, and substitute pole lines for it. I should like to observe, with regard to gutta-percha made into cables, and submerged in water, especially in salt water, that it is in all respects, I think, in the most favourable position for its preservation. The surrounding sheath of tar, closely encased in iron wires, the cool moist temperature, the absence of rays of light, and the antiseptic properties of the sea are all the very elements in which gutta-percha delights; and instead of anticipating injury from anything like decomposition or percolation, I should expect that submersion and pressure, if long continued, would render it more compact, and much better as an insulator.

3015. Have you tried any india-rubber covered wire?—I have had some in my possession, which I have brought for the information of the committee, that came to me direct from Messrs. Silver; therefore it is identified (*producing specimens of india-rubber covered wire*). In the thick piece, which is cut off, you will see that decay is commencing very slowly to set in; in the thinner piece it has already advanced considerably, and in the piece of cable it seems utterly gone. The action seems to be between the copper and the india-rubber. I have not made any experiment upon it; it has been merely lying in my office drawer for two or three months.

3016. (*Mr. Varley.*) Do you know anything of the state of the underground wires between Liverpool and Preston, Preston and Carlisle, Carlisle and Glasgow, and Carlisle and Ireland?—They have been taken up and pole lines substituted for all, I believe, now, or certainly for the far greater portion of them.

3017. (*Mr. Edwin Clark.*) What length of underground wire have you to which marine glue has been applied?—Eighty-four miles, from London to Dover; I took that up first because it was of vast importance to the company that that in any case should be secured, inasmuch as all the continental traffic depended upon it.

3018. (*Chairman.*) What is done with worn out gutta-percha covered wire?—It is sold.

3019. What price will it fetch?—About a shilling a pound, weighing the copper and gutta-percha together.

3020. To be worked up again?—It would not be worked up into cables, I should think; it would be worked up into a common kind of gutta-percha for shoe soles and inferior articles.

(*Mr. Varley.*) We sell a great deal to doll-makers for about 5*l.* a mile.

(*Mr. Saward.*) I think that was about what the Magnetic Company got for their old wire; they got nearly enough out of their old gutta-percha to put up their pole lines where they were required.

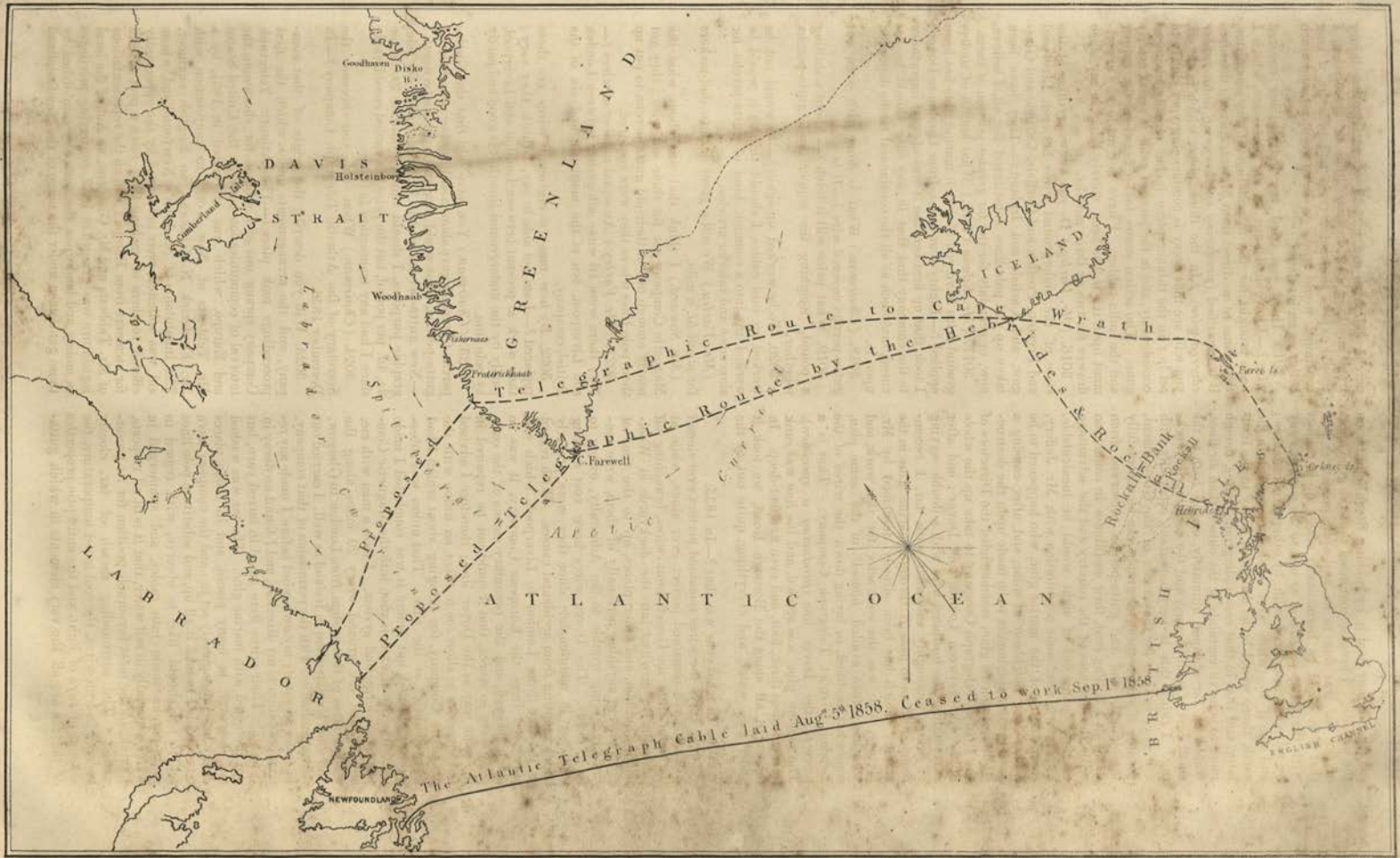
3021. (*Mr. Varley.*) An equal number of wires?—No; there were 16 wires altogether between London and Liverpool, ten of the magnetic and six of the British company.

3022. By how many overground wires are these replaced?—I am not able to say, I think they vary; they work at present between London and Liverpool, partly by the underground line that is good and partly by pole line.

3023. (*Chairman.*) You have said that the Gutta-percha Company gave about a shilling per pound for old gutta-percha, what would new gutta-percha be worth?—New gutta-percha of the best description for telegraphic purposes, I should think, would be 2*s.* 3*d.* or 2*s.* 4*d.* per pound, but the price has varied considerably since I had any transaction in it.

3024. (*Chairman.*) When did you become the Secretary of the Atlantic Telegraph Company?—In December 1856.

3025. Will you describe the origin of that company, and the arrangements under which the first proceedings took place?—The Atlantic Telegraph Company sprang out of a scheme for shortening the interval of communication between the United States and Europe, which was originated in 1851, by a Mr. Tebbetts of New York, in conjunction with Mr. Frederick N. Gisborne, an English engineer. The inhabitants of St. John's, Newfoundland, whose aid was solicited, were influenced by the opportunity offered to them of making their town a port of call for steamers from Europe; and on the representations of Mr. Tebbetts and Mr. Gisborne, an Act of the local legislature was granted in 1852, conferring privileges in money and land upon the company established by those gentlemen under the name of the Newfoundland



Goodhaven
Disko B.
DAVIS STRAIT
Holsteinbor
Cumberland Isle
GREENLAND
Woodhaab
Fiskernass
Frederickshaab
Labrador Current
Spit Bergen
Proposed Telegraphic Route to Cape Wrath
Proposed Telegraphic Route by the Hebrides & Rockall
C. Farewell
Arctic Current
ICELAND
Faroe Is.
Orkney Is.
Rockall Bank
Rockall
Hebrides
BRITISH ISLES
ENGLISH CHANNEL
LABRADOR
ATLANTIC OCEAN
NEWFOUNDLAND
The Atlantic Telegraph Cable laid Aug.st 5th 1858. Ceased to work Sep.1st 1858

Electric Telegraph Company. That Company's proposal was to establish land lines of telegraph from St. John's to Cape Ray, in Newfoundland; a submarine line of about 70 miles from Cape Ray, Newfoundland, to Ashpee Bay, Cape Breton; a land line across Cape Breton to the Gut of Canso, and a submarine line of 12 miles across the Gut of Canso to Nova Scotia, where it joined up with the network of telegraphic lines communicating with Canada and the United States. That Company after executing a portion of their works failed to fulfil the terms of their Act, and became insolvent and indebted to the amount of some 10,000*l.*, chiefly due in St. John's, Newfoundland. Their affairs being in that state in 1853, Mr. Gisborne visited New York with a view to ascertain whether assistance could be obtained to carry out the undertaking to which he had devoted a great deal of personal labour and time. He was there introduced to Mr. Cyrus W. Field. It happened that some time previously to this period the idea of establishing a telegraph across the Atlantic had suggested itself to several individual minds, but it had assumed no form or substance; and Mr. Cyrus Field, though unwilling to apply his means to propping up the falling fortunes of the Newfoundland Electric Telegraph Company by promoting that scheme as an isolated adventure, conceived the notion of obtaining scientific advice upon the subject of a Transatlantic telegraph from his distinguished countrymen, Lieut. Maury and Professor Morse. Their assurances were satisfactory, and the result was, that an agreement was entered into between six American gentlemen, namely, Mr. Cyrus Field, Mr. Dudley Field, Mr. Peter Cooper, Mr. Chandler White, Mr. Moses Taylor, and Mr. Marshall O. Roberts. Those were six American citizens. This agreement was, that if certain specific advantages agreed to amongst themselves could be obtained from the Newfoundland legislature, they would pay the debts of the Newfoundland Electric Company, purchase its property, wind up its affairs, and become subscribers to a new company, to be called the New York, Newfoundland, and London Telegraph Company, the object of which should be to bring London and New York into continuous electric communication. Mr. Cyrus Field and his brother were deputed to go over to Newfoundland to negotiate with the local authorities, and they there, in 1854, obtained the Act of Legislature which I now produce, and which was subsequently confirmed by the Home Government. That Act grants to the American company so established the following privileges, giving them shortly—

Guarantee of interest on 50,000*l.* of bonds.

Grant of 5,000*l.* towards construction of roads across the island.

Fifty years' exclusive right to land cables on the shores of Newfoundland and Labrador.

Fifty square miles of unoccupied land on the establishment of the inland communication in Newfoundland.

Fifty more square miles on completion of the telegraphic communication with Europe.

The company established, as I have described, having executed its works in the island, turned its attention towards extending the telegraph to Europe. It was found impracticable to raise the capital for this part of the undertaking in America; and Mr. Cyrus Field, during one of his early visits to England, associated himself with Mr. John Watkins Brett, one of the projectors of the Dover and Calais line, with a view to the money being raised in England. Mr. Brett, who was then a good deal occupied with other cables, employed Mr. Whitehouse, whom he knew as an electrical experimenter, to investigate for him some electrical facts in connexion with the probabilities of success. Mr. Whitehouse, in the course of these experiments, became acquainted with the present Sir Charles Bright, and finally, these four gentlemen, Messrs. Field, Brett, Whitehouse, and Bright, entered into a contract with each other to become joint projectors of a separate company in England, to carry out the submarine line between Ireland and Newfoundland. Through the medium of Mr. Cyrus W. Field, these projectors in 1856 entered into an agreement with the New York, Newfoundland, and London Telegraph Company, by which they secured to the company about to be started in England the exclusive right of landing cables, acquired by the former company in Newfoundland and elsewhere, subject to conditions as to completion of works, and so on. This was all the Atlantic Telegraph Company acquired by this agreement; they had nothing to do with the other privileges as to guarantee of interest or appropriation of land; they simply acquired the exclusive privileges under conditions as to executing the works

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3026. What conditions as to executing the works do you mean?—They had a certain limited time given them wherein to lay the Atlantic cable; originally that time was up to 19th of September 1859, but of course it was to their interest to extend that period, because they had no other capitalists to come in and take the position assumed by the Atlantic Company, and they have consequently extended that period till 1862.

3027. Provided this Newfoundland Company did not lay the cable within a reasonable time, were there no means of putting an end to their monopoly?—No.

3028. No one else has any right to lay wires to Newfoundland for fifty years?—None except the New York, Newfoundland, and London Telegraph Company.

3029. Is there no limit as to the time in which they must exercise that right?—None; perhaps the Committee would like me to read the clause of the Act of the local Legislature giving the exclusive right; it is clause 14: "The corporation hereby created shall "have the sole and exclusive right to build, make, "occupy, take or work the said line or any line of "telegraph between St. John's and Cape Ray, or between any other points in this island (excepting "only the existing line between St. John's and Carbonear) for the full period of fifty years from the "passing of this Act; subject, nevertheless, to the "right of pre-emption by the Government of this "colony, as herein-after provided; and during the "said period of fifty years, no other person or persons, "body or bodies politic or corporate, shall be permitted to construct, purchase, take, or operate "any line or lines of telegraph on this island, or to "extend to, enter upon, or touch any part of this "island, or the coast thereof, or of the islands or "places within the jurisdiction of the government of "this colony, with any telegraphic cable, wire, or "other means of telegraphic communication from any "other island, country, or place whatsoever. Provided, however, that if the said line of telegraph "shall not have been completed from St. John's to "Cape Ray, or other point on the western coast of "Newfoundland, and a communication by telegraph "across Prince Edward's island, or the island of Cape "Breton, or otherwise established with the continent "of America within five years from the passing of "this Act, the exclusive privileges granted by this "section shall cease." The only condition is if they have not a connexion with America.

3030. What is the right of pre-emption?—If at any time after twenty years from the passing of the Act, it should be deemed advisable, then by giving notice and appointing arbitrators, the Government can take the line upon paying the money. You will observe that that Act only gave power to the New York, Newfoundland, and London Telegraph Company to bring the communication across the Atlantic, but an Act was subsequently obtained permitting them to detach that portion of their scheme, and make a separate company for it, which was done by the Atlantic Telegraph Company taking it up. All these matters having been arranged, a provisional committee was established in London, consisting partly of the projectors and partly of two or three

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gentlemen who were induced to join them, who are not now directors. A very large number of experiments on different forms of cable were then made, chiefly by Mr. Statham of the Gutta-percha Company, of whom I ought to say, that to him more than to any other man living, submarine telegraphy is indebted for its existence, by his care and attention in the manipulation of the gutta-percha. Experiments were also made by Mr. Glass and Mr. Canning, who indeed were the chief designers of the Atlantic cable as regards its external structure. All these matters were completed by the provisional committee, and the contracts for the manufacture of the cable were entered into with Messrs. Glass, Elliot, and Company and Messrs. Newall and Company before the shares had been issued, and before the board of management was elected. The provisional committee moreover obtained from the English Government a grant of 14,000*l.* a year to the Company, conditional on success, and a promise of a similiar grant from the United States Government. Under these circumstances the provisional committee by dint of great exertion succeeded in obtaining subscribers for the first capital of 350,000*l.* in shares of 1,000*l.* each, pledging themselves at the time they did so, and binding the contractors, that the attempt to lay the cable across the Atlantic should be made in 1857. The holders of these shares elected the board of directors in December 1856. I have taken the liberty of availing myself of your patience to give this circumstantial account of the facts with regard to the Newfoundland Act, because it has been urged that the Atlantic Telegraph Company were the creators and upholders of an offensive monopoly. It will be seen from what I have stated, that the Atlantic Company did nothing but purchase and transfer to British subjects and to Her Majesty's Government rights and privileges which had been obtained by citizens of the United States over the shores of an English colony. Her Majesty's Government cannot otherwise obtain the right of priority in transmission of messages or any of the other facilities that will be secured to it if the Atlantic Company be enabled to lay a successful cable. The advantages held out to the original subscribers were very slender when the risk that attended it is taken into account; and I think it was greatly to their credit then, and entitles them now to the fullest and most favourable consideration from Parliament and from the Government, that the great bulk of the capital, whatever may be thought to the contrary, was most certainly subscribed from patriotic motives, and in the interest of scientific discovery, in order to enable a great experiment to be tried for the world's benefit, far more than from any desire or expectation of a lucrative result from the first attempt. At this distance of time, and with the advantage of the light which the Atlantic and other deep sea cable enterprises have thrown on the scientific portion of the subject, it would be, in my opinion, a comparatively easy matter to connect Ireland with America by telegraph, if the business arrangements of the company proposing to do it were also organized in accordance with past experience in that respect. But it was far otherwise in 1857. The whole matter was then a theory, and, in the opinion of many persons, a very doubtful theory, and the way to success had to be fought upwards through successive failures. Still there was no doubt a good deal that might have been better done, and would on another occasion be thoroughly remedied. Among these I think nothing was so prejudicial as the manner in which the first attempt was hurried.

3031. Will you describe the progress of the Atlantic Telegraph Company after the formation of the board, and state your convictions as to the cause of failure?—It was not till the month of February 1857 that the contractors had fairly set to work to make the cable, of which 2,500 miles were required. This length was not completed till the end of June 1857, and considering that the core had to be covered three times representing 7,500 miles of work, at the Gutta-percha Works; that 335,000 miles of iron and copper wire had to be drawn out and spun into more than 47,500 miles of strand, that upwards of 300,000 miles of tarred hemp had also to be spun and saturated, and that these materials had to be put together and woven into a cable in the space of four months; I say, considering this, although the work may be deemed a marvel of human industry, it does seem to me a most wonderful and favourable circumstance that the cable has turned out so well as it has done. The projectors, however, had pledged the company to the public and the shareholders that the experiment should be carried out in 1857, and the directors were constantly assured that it could be done, and they had therefore no alternative but to proceed.

3032. The directors were constantly assured by the projectors?—Yes, in fact by the contractors and all those upon whom they had relied for such information. The Gutta-percha Company said that the gutta-percha could be delivered, and it was delivered; it was all executed as they had promised—all I am arguing against is the hurry. I say that they should not have been put into that position. It was only when the directors assembled at Queenstown at the end of July that they became fully aware of the want of preparedness in every department. However there was nothing for it then but to allow the ships to go on. The cable broke on that occasion in the machinery, and it was too late to try again in 1857; besides which, the cable remaining was too short, and it was deemed advisable not to go out again with less than 3,000 miles on board. The shareholders again came forward and subscribed at par the new capital for the purchase of cable both to supply the loss and make up a total of 3,000 miles instead of 2,500 as taken out at first, although the original shares were then at 50 per cent. discount. The subsequent coilings and re-coilings up to the time of starting again in 1858 were all elements likely to injure a cable of the peculiar construction of the Atlantic cable, added to which were the defective arrangements of the chief of the electrical department, especially as to continuous testing, which have been alluded to by Professor Thomson, and owing to which, and other causes, the directors were prevented from knowing that there were suspected to be injuries or defects of a slight character in the cable.

3033. Were not the directors aware of those slight injuries which have been alluded to in the evidence?—No, not in the slightest.

3034. They considered the cable to be perfect?—They did not know but that the cable was perfect. It was known that one part of the cable tested worse than another. Mr. Whitehouse assured Professor Thomson, who believed the statement and did not make a personal examination into the matter at that time, that it was owing to the superior quality of the portion last manufactured, and not to any defects in the portion previously manufactured; in fact that the difference in testing between the two ends of the cable was owing to the better conductivity of the new portion. We never heard a word of defect or a whisper of anything like injury to the cable up to the time of its starting.

3035. Were regular reports made by the electrician to the directors of the state of the cable?—They were not regular, they came at irregular intervals. The instructions of the board were that the reports should be regular, but circumstances, I presume, prevented it.

3036. But reports were made stating that the cable was electrically perfect?—You have all the reports in your possession, and the inference at any rate to be drawn from some of the reports is that the cable was electrically perfect; in some cases it was stated that it was.

3037. (*Mr. Varley.*) Was no one else consulted upon that point, when it was found that one portion of the cable tested differently to another, besides Mr. Whitehouse?—Yes, at Keyham. Unfortunately there was a great deal too much self-reliance in the matter by Mr. Whitehouse. Mr. Walker, however, went

down at the request of the Board, and you have his report before the Committee.

3038. Was not the cable cut up into a great many lengths at Keyham, at Greenwich, and on board the "Agamemnon," during the different trips?—No doubt there was a great deal too much recklessness in cutting the cable from time to time; that is to say, if a test was wanted, the first thing was to call for a person to cut the cable and begin to test, and then it had to be joined up again.

3039. Did not they very frequently in testing the cable prick for the wire?—That was done occasionally, I am aware, but in very many instances there was a positive cut. I have been informed by Captain Kell that the cable must have been cut into at least a hundred pieces at one time or other. Either from that cause or from some other cause, this fearful result has taken place which is exhibited by these four joints (*producing the same*), which have been cobbled up again. I do not know whether they were in the original cable or whether they were the result of cutting, but those are joints which I have taken out of a portion of the cable.

3040. (*Professor Wheatstone.*) Before the cable was submerged?—Yes, these have never been submerged.

3041. (*Chairman.*) What portion of the cable were those joints taken from?—They were taken out of the portion of the cable manufactured by Messrs. Newall and Company; but I must not be understood from that to state, by any means, that they were made by Messrs. Newall; I cannot identify by whom they were made, but I can state that they were in that portion of the cable.

3042. (*Mr. Varley.*) How can you state that?—I can state that from the fact that that is flattened hemp, whereas in Messrs. Glass and Elliot's cable the hemp is spun, and then laid into strands; that is the reason I can identify it.

3043. (*Chairman.*) Newall's was flattened hemp?—Yes, laid flat round. Glass, Elliot, and Company laid theirs in strands round, and that enables me to identify it. I have placed a properly made joint by the side to show the contrast.

(*Mr. Varley.*) It is very possible that a joint of this defective description may account for the sudden cessation of continuity, and its re-appearance afterwards when the cable got to the bottom, owing to its separation by the stretching of the cable and then coming together again.

(*Mr. Saward.*) We have been assured by competent persons who have specially examined the cable on behalf of the company, that the slight defects which I have mentioned as existing before the cable was laid, were increased after the cable was down by the injudicious use of battery power, and the cable now lies in a state in which, although it may again, by great care, perhaps, be got to work once more to a very moderate extent, can scarcely be hoped ever to fulfil the expectations which arose regarding it from the working after it was first laid.

3044. (*Chairman.*) Are you aware whether the statement which was made by Mr. Chatterton was correct, namely, that the servants of the Gutta-percha Company, who were sent to the contractor's works to make the joints, were sent away because they made the joints too slowly?—I am not aware of it; certainly, if I had been aware of it, I should have done all in my power to prevent it. The primary cause of all the failure I considered to be the great hurry in the first attempt, and too little preliminary experiment and discussion. 30,000*l.* or 40,000*l.* spent in preliminary trials would, in all probability, have saved the whole capital of the company.

3045. And, probably, much less than that?—Probably much less than that. Of course experiments to be carried out in deep water require expensive arrangements and ships, which would eat into a great deal of money.

3046. (*Mr. Bidder.*) What was the actual cash outlay; I suppose of the capital, a good deal was allotted to the gentlemen who were early connected with the company?—No. I will state exactly how the capital stands. The actual cash outlay has been 387,000*l.*, and the capital in addition to that consists of another sum of 75,000*l.*, which arises in this way: the first arrangement made between the projectors and the Atlantic Telegraph Company, and which was entered into before the present board was elected, was that they should receive nothing in the form of remuneration until the cable was laid and successfully at work, and further, until the cable so laid had earned 10 per cent. of dividend to the shareholders; after that 10 per cent. had been earned, the projectors were then to have a right to one moiety of all the balance of profit remaining beyond that 10 per cent.; but, although that looked a very fair arrangement upon the face of it, yet, when I came to look thoroughly into the agreement after my appointment, I found it was so hampered with conditions, that really the projectors were the masters of the company; they were to interfere in everything; one could not raise a shilling of extra capital without their consent; they were to have continual supervision of the books, and to dictate what was to go to capital and what was to go to revenue; and therefore the attention of the directors was very early called to the desirability of getting rid of this agreement by compounding, if possible, for a present payment. This was attempted at that time; but previously to the failure, the projectors were so sanguine as to their prospects under this agreement, that they would not come to anything like a reasonable arrangement. After the failure, however, in 1857, and intermediately between that time and the attempts in 1858, they were, after a good deal of discussion, induced to compromise their chances for a sum of 75,000*l.*, to be paid in shares of the company, which shares were not to be disposed of until the cable was laid and successfully at work. "Successfully at work," has been interpreted by the company and by three of the projectors as meaning a proper cable laid and continuing to work, and they have not parted with a single share; but another of the projectors has interpreted it differently, and has thrown his shares upon the public. I ought to mention that that projector is Mr. Whitehouse; otherwise the statement may be thought to refer to others.

3047. Then the actual money spent would be 387,000*l.*?—Yes; out of which the construction account, that is to say, comprising the cable and "expenses ifi the equipments, fittings, and supplies "to ships, dredging, hire of tenders," and so on, 324,351*l.*

3048. For actual cable, the cost appears to be 300,000*l.*?—Yes; that was for the purchase of 3,400 miles in round numbers.

3049. (*Chairman.*) Will you state what measures have been taken with a view to resuscitate the cable, and to provide the means for purchasing a new cable?—I may mention that from the time it was found that the electrician of the company could not go out with the expedition up to the very last moment that there was any chance of the cable being rendered useful at all, Professor Thomson devoted himself heart and soul to the work, and that without anything in the shape of remuneration for his services; he went out with the expedition, and on shore at Valentia; from the 5th of August until the early part of September he worked incessantly; and it is to his arrangements, especially to his reflecting galvanometer, that the company and the public are indebted mainly for the messages having been brought across the Atlantic to this side. Other gentlemen, however, have been to examine the cable and give their opinion, amongst whom were Mr. Varley, Mr. Henley, Mr. Edward Bright, Mr. Holmes, and Mr. France.

3050. Who was Mr. France?—Mr. France is a gentleman who was connected with the Submarine Telegraph Company; he has had perhaps as much practical experience as most persons in the management of the Morse instruments particularly; he is a

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good electrician besides, and he is now, I believe, somewhere in the Mediterranean, acting either for Mr. Brett, or for one of the Mediterranean companies, I do not know which; he is a man of very considerable talent and experience.

3051. Was not there some little difficulty about his examining the cable at Valentia? — Mr. Whitehouse, I am sorry to say, grossly insulted him. Mr. Whitehouse, as soon as Mr. France arrived, telegraphed to say that if we chose to let him stay there as an instrument clerk, he might remain; but in no other capacity would he recognise him. This related to a gentleman who had come from Marseilles to Valentia to help us in our extremity, without expecting any remuneration or anything of the kind.

3052. (*Mr. Bidder.*) Is it not generally the opinion of the directors and yourself that the failure of the Atlantic telegraph was mainly owing in one respect to the hurry in which the work was carried on, and in the other to the want of authentic information being given to the directors as to the state of the cable before the attempt was made to submerge it?—Yes, added to the want of practical skill in the electrical department; an undertaking of this kind requires previous practical experience as well as scientific knowledge.

3053. (*Chairman.*) Are the Atlantic Telegraph Company at the present time seeking to provide the means for furnishing a new cable, or have they made any attempt for that purpose?—Yes. As early as the end of September 1858 the directors made an application to the Earl of Derby's Government for aid to carry on the work, and feeling that the undertaking had claims to national support on account of the hearty way in which the subscribers had contributed the means to try a very great problem, and the services which the successful solution of that problem had conferred on the general public and on the Government especially, the War department having been the first to realize great advantages by the messages received and transmitted during the working of the cable; the board applied for a guarantee on a new capital of 600,000*l.* similar to that granted to the Red Sea and Indian Telegraph Company on a capital of 800,000*l.* The Government declined to grant the unconditional guarantee in the belief, as I think, that our shareholders would again be induced to come forward and incur another risk upon a conditional guarantee. We offered, moreover, to transfer our exclusive rights into the hands of the Government in order to avoid any suspicion of a desire for monopoly, but they, after very careful consideration, ultimately declined to give assistance in that form. They at length, however, granted us a guarantee of eight per cent. upon 600,000*l.* of capital conditional on success in the following terms:—"A dividend of eight per "cent. per annum to be guaranteed for 25 years, "upon such portion of a new capital as shall be called "up and expended in establishing communication with "America, not exceeding in the whole the sum of "600,000*l.*; this guarantee to commence when the "cable has been successfully laid, and to subsist while "it is capable of being worked at the rate of 100 "words per hour. A minimum sum of 20,000*l.* per "annum to be paid to the company for Government "business, during the time the new cable is capable "of being worked efficiently as above. The company "to be allowed to expend 20,000*l.* out of the new "guaranteed capital in attempts to make the existing "cable available for business. If these attempts are "successful, the Government will immediately commence to pay a minimum rent of 14,000*l.* per annum, "which rent is to be increased to 20,000*l.* per annum, "as above, so soon as the new cable is brought into "successful working order. The existing arrangement of the company with the Government of the "United States is not to be interfered with. Under "that arrangement the Atlantic Company will be "entitled to a minimum rent of $70,000 per annum, "for the business of the American Government, "so soon as the communication is established with "Newfoundland. The Atlantic Telegraph Company "are to transfer to Her Majesty's Government that "portion of their privileges, under their agreement "with the New York, Newfoundland, and London "Telegraph Company, whereby they derive the "exclusive right of landing on the shores of New-"foundland, cables intended to connect Europe with "that country; but the Government will not grant "to any other company or persons any right of "landing cables in Newfoundland so transferred to "them unless the guarantee and contract referred to "under articles 1 and 2 shall have proved efficient "in raising the requisite capital, nor until, by the "instrumentality of such capital, telegraphic commu-"nication shall have been established across the "Atlantic; it being understood that in case of "failure, either to raise the capital or to submerge "the cable with such success as to be enabled to "work it at the rate of 100 words per hour required "by the Government, the company shall perma-"nently revert to its present position in respect to "all its privileges, including its exclusive rights now "existing under the agreement with the New York, "Newfoundland, and London Telegraph Company." That was our agreement with the Government, and great exertions have been used to induce both the public and the shareholders to subscribe under those conditions, but without anything like sufficient success. I myself have personally waited upon nearly every capitalist and mercantile house of standing in Glasgow and in Liverpool, and some of the directors have gone round with me in London for the same purpose. We have no doubt induced a great many persons to subscribe, but they do so as they would to a charity, and in sums of a corresponding amount, such as 20*l.*, 25*l.*, and 50*l.*

3054. (*Mr. Bidder.*) Nothing of a practical character?—No. We have been enabled to raise about 72,000*l.* in the whole, and it is difficult to conceive the amount of exertion that that sum represents when you look at its smallness.

3055. (*Chairman.*) Will you state your views as to what description of aid from the Government would suffice to procure the means of carrying out the undertaking successfully?—The sum required is so large, and the nature of the risk is so suspected by the public, that without some certain prospect of dividend to a small extent, I do not believe the necessary amount of money will be forthcoming for any Atlantic telegraph enterprise for many years to come at the least. I would first call your attention to the fact that the Red Sea Telegraph Company's stock, which is guaranteed 4½ per cent. for 25 years, irrespective of success or failure, was for a considerable time at a discount. I name this to show that under the guarantee which I am about to mention, there are no such advantages as would foster speculation or give anything but the most legitimate and moderate help by means of the temporary security of the Government to a work which is eminently national in its character; an undertaking whose antecedents render it, as I venture to say, worthy of such help; and one which after a good deal of personal experience in these matters, backed by the opinion of individuals of the very highest commercial eminence, I feel justified in saying it will be impracticable to carry out without some such assistance. I think, however, that the requisite capital may be raised with very little chance of the public revenue being ultimately burdened to the extent of a shilling, if the Government will grant to the Atlantic Company a guarantee of four per cent. for 30 years under proper precautions. The guarantee must continue for that time irrespective of the success or failure of the cable; but as precautionary measures the Government should require the careful determination by experiment of all points in the scientific department of the work that may at present be short of solution. They should further require the company to undertake, when the form of cable has been decided on, to provide suitable and responsible contractors who will undertake to make and lay a cable, not one

of their exclusive designing, but such a cable as shall have been decided upon after proper examination; and who shall agree to work it at a given speed for a month at least after it has been laid, under pain of forfeiting their entire profit upon the whole transaction, and a further penal sum of 10,000*l.* or 15,000*l.* in case of failure during that period. As a further precautionary measure the Government should require the company from the first to establish a reserve fund and a fund for the repayment of any advances on account of the four per cent. which may have been made previously to the work being accomplished. The smallest capital that would suffice for making and laying an Atlantic cable, constructed on principles such as experience seems to show to be absolute necessary, would be 600,000*l.* No matter what route be selected for the communication, that is the smallest sum that can be safely relied upon to render success anything like certain; and to raise that amount there must be some assurance to the capitalist that he will not under any circumstances incur the risk of an absolute loss. The object in view being partly imperial and partly commercial, I think it is fair that the risk also should be mutual. The guarantee of four per cent. for 30 years, supposing the cable to fail (the capital of the shareholders being thereby for the time destroyed), would not leave him more than a trifle above two per cent. nett for his money after provision for recovering his sunken capital at the termination of his annuity; meanwhile it must be borne in mind that the price of his stock would be very greatly depreciated. That is in the case of the cable failing. On the other hand the moderately calculated returns upon the business of a cable working at the rate of even three or four words per minute during 12 hours a day, would leave sufficient to pay at least 10 per cent. on a capital of a million, so that once a cable were well and regularly in work, there would be scarcely the remotest chance that the Government would ever be called upon for any portion of the guarantee.

3056. Do you fix any period for the life of a telegraphic cable?—I do not. I must say it is my entire belief that if you can once properly and safely submerge a properly considered cable in the deep ocean, it will, in all probability, last for hundreds of years.

3057. (*Mr. Bidder.*) You would not apply your observation to cables laid in shallow water?—I would not, on account of the risk from anchors and from accidental circumstances; but I would say with regard to those cables, that although they may become expensive as to maintenance, still the property is not gone, because you can raise them and repair them.

3058. But they would be of very small value?—You can repair them.

3059. (*Mr. E. Clark.*) Would you prefer a higher per-centage for a smaller number of years?—It would effect the same object, I think.

3060. (*Chairman.*) Have you considered the question of a subvention, instead of a guarantee of interest, the Government to advance a given sum for every sum advanced by the shareholders?—I think if the subvention were liberal, it would answer.

3061. What would you call liberal?—I should say half and half.

3062. Would that raise the capital?—I think it would; and I am fortified in that position by the opinions of a good number of persons whom I have consulted.

3063. If the Government paid 300,000*l.*, you think the commercial world would raise another 300,000*l.*?—Yes, I think they would.

3064. (*Mr. Bidder.*) Should the Government then have the privilege of transmitting their messages?—Yes, they should have the privilege of transmitting their messages, free of any charge, of course.

3065. (*Mr. Varley.*) Do you not think the price proposed to be charged for telegraphic messages from England to America very much too low? The price of a message from London to St. Petersburg, the greater portion of the line by far being overground wires (which is very much less expensive, and attached to which there is very little risk,) is about 1*l.* 13*s.* 6*d.* for a distance, as the crow flies, of about 1,500 miles; therefore, do you not think 2*l.* too small a charge for a message from England to America?—The charge is 2*l.* 10*s.* I am of opinion, with respect to charges, that it is good policy commercially to make them as low as you conveniently can. I think, as a matter of calculation, the charge would be remunerative, especially looking to the new light which electric science has now thrown upon the retardation of the current and the means of overcoming it.

3066. Do you not think that two such countries as England and America would immediately fill any single wire that was submerged between them, if the rate were so low as 2*l.*, and would not the consequence be that the telegraphic line would have so much delay on it that it would be of very little use?—That is a matter which soon right itself. If the cable were filled with messages to overflowing, that cause would soon provide the means for additional cables. It is a thing which must grow, like the remainder of the telegraphic system.

3067. (*Mr. Bidder.*) You would fix prices that induced just as many messages as the cable could carry, taking the highest price that would fill the cable, whether you laid another cable or not. If you could fill the cable with messages at 10*l.*, instead of 2*l.* 10*s.*, why should not you charge it? You might say, if we had three or four cables we could fill them at 5*l.*?—I am afraid if the price were excessive we should be open to the charge of being monopolists, which is not our object.

3068. (*Mr. E. Clark.*) Has application been made to the American Government for pecuniary assistance as well as to the English Government, or only on this side?—Only on this side. We were informed by Mr. Field that it would not be politic to make such an application on the other side, though since then I have received a letter from him in which he expresses his opinion, founded upon what facts I do not know, that if the British Government would guarantee three per cent. unconditionally the American Government would do the same. Now that the difficulties incident to laying a cable across the Atlantic have been so very much diminished by experience, I believe that the four per cent. guarantee which I have suggested would be a more economical plan of assisting the enterprise than that upon which the Government have already consented to help us, but which has been found inefficient.

3069. (*Chairman.*) The eight per cent. conditional guarantee?—Yes. They have guaranteed 20,000*l.* a year, in case of success, and so long as the cable is in working order. Now the only return we are to render for that is, to send the messages of the Government at ordinary charges up to that annual amount. From experience I feel sure that the cost of the messages of the Government would not be likely to exceed half that amount; consequently the remainder would be a free gift or premium to the company, which would be continuous, and which I think is objectionable on public principle.

3070. (*Mr. E. Clark.*) It would be an annual subvention, instead of a present one?—Yes; it would be the free gift of 10,000*l.* a year to a joint-stock company.

3071. (*Chairman.*) Is there not a great inconvenience when telegraphic lines are blocked up with work, if a person wishes to send an express message?—There is.

3072. Could not some system be adopted of charging a low price for an ordinary message, and a high price for an express message? for instance, 8*l.* for an ordinary message, and 50*l.* for an express message?—At present that is prohibited by law. All telegraph companies are bound to show no favour or preference in the distribution of their messages; therefore, unless the law were altered in that respect, that would not be practicable, although I think it might be desirable; but the worst of it would be its liability to abuse. A

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rich man who could afford to spend his 50*l.* or 100*l.* in blocking up the wire, might use it in speculation to the very great disadvantage of the poorer man engaged in more legitimate transactions.

3073. (*Mr. Varley.*) You are aware, I presume, that an arrangement is in existence in Belgium by which you can send a message on any urgent matter, taking precedence of the ordinary messages, upon paying a higher rate?—I believe that is so; but, you will correct me if I am wrong, I believe that that urgency is not a commercial one; it is urgency of life and death, or some social urgency.

3074. I think it rests entirely with the sender of the message,—*if* he likes to give a higher rate, he can send his message before others?—I was not aware of that.

3075. (*Chairman.*) Will you state your opinions as to the best arrangements and the best form of contract for carrying out, successfully and permanently, any large plans for the establishment of ocean telegraphy?—I think that in a matter requiring so much care, depending so much upon the heartiness of those engaged in it, and involving so many contingencies as the making and laying of a considerable deep sea telegraph, the ordinary system of pitting a number of contractors against each other fails altogether. I therefore consider that any seeking by contracts to lower the cost of construction of your cable to the least price at which a man can be found to make it, is very mischievous to the result you have in view. On the other hand, in saying this I must not be understood as becoming an advocate for any system which would give no check to either extravagance or rapacity; but I think a form of contract like that I am about to describe would abundantly protect the contracting party, while it would leave the contractor in a position of freedom from all care, except that of faithfully performing his duty so as to carry out his work successfully. I would suggest that you should be very careful in the selection of your contractor, and of course, to get a man of integrity; and having done so, he should be bound at his own expense to make such preliminary experiments, unless they were of a very extraordinary character, as were necessary to determine the right kind of cable to be used, of which you and your scientific advisers should be the judges. In getting him to do this, he should be guaranteed that he shall have the making of the cable. When you have decided upon the form of cable, let him proceed to make it, opening a set of books specially for that cable, and allowing to the contracting party a proper check by means of a public accountant against anything like extravagance. When he has made the cable the books shall be balanced, the cost of it ascertained, and then a definite commission upon the cost to be agreed upon beforehand superadded to the cost.

3076. How would you prevent the contractor from charging a higher price for the materials than was actually charged by the manufacturer?—With regard to the gutta-percha core, and all the other materials, they might be purchased by the contracting party. The contractor should be bound to undertake to lay the cable, and work it for one month after it has been laid, at a given rate of speed; if he succeeds in that, then to be paid his commission, and the matter is at an end; but if he fails, he should forfeit both his profit and a penal sum of say 10,000*l.* That must not be deemed an impracticable form of contract, for previous to our attempt to raise capital for a new cable, I obtained from an eminent firm an undertaking to contract for a future Atlantic cable on those terms. The contractor ought to be bound to secure the services, during the making of the cable, of the best electrician that either money or love of science can devote to the service. Everything nearly depends upon the electrician; if his work be intelligently, carefully, and practically well executed, the task of the engineer will be a very light one.

3077. (*Mr. Varley.*) Do you not think that a month is much too short a period for the contractor to hold himself responsible for the working of the cable?—I should certainly like him to take it for a longer period, if a person could be found to do so.

3078. Have you considered what routes are best for an Atlantic cable?—I have some observations to make with regard to the various routes which have been indicated by different persons as the best for the purpose of establishing transatlantic communication by telegraph; and in doing so, I wish it to be understood that neither the directors nor myself have any prejudice in the matter, nor have we arrived at any foregone conclusion in favour of one route over another. The various routes that have been proposed are as follows:—First, the Atlantic route between Ireland and Valentia, which has been to a very great extent sounded and mapped out, though not nearly so fully as I should desire to see it. Secondly, the route from Cape Wrath, at the extreme north-west of Scotland, to the Faroe Islands; or from the Hebrides to Rockall*; thence to Iceland, Greenland, and Labrador, or Newfoundland. Thirdly, from Falmouth or Ireland to Blanc Sablon, in the straits of Belle Isle. Fourthly, from Falmouth to Halifax, Nova Scotia; and fifthly, from Falmouth to the Azores, and thence, touching at intermediate islands, to Brazil, and by way of the West Indian Islands, to Florida. Respecting the first route, which has been practically tested, and found to present no serious obstacles, I need not say anything. As to the second (the Arctic one), I would observe that all the evidence short of the actual and careful survey of the shores of Greenland and Iceland that could be collected was sought after and collated by the Atlantic Company, and the opinions we received included those of experienced navigators in those seas; with no dissentient voice, they were against it.

3079. (*Chairman.*) What were the objections?—We were told that a cable *might* possibly be laid to Greenland, but that at the break up of the ice it would almost infallibly be carried away, and as the deep water commences suddenly at about 2,300 fathoms within some 50 or 60 miles from the western shore of Greenland,† that would render it extremely hazardous.

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* In Norie's Sailing Directions for the Hebrides, &c. (1858), there occurs the following description of the Island of Rockall:—

"ROCKALL, or ROKOL.—Although the Island of Rockall does not come within the limits of the chart which the *preceding* directions are intended to accompany, yet, as that solitary and dangerous islet lies directly in the track of ships coming from the westward towards the Hebrides, or Coast of Scotland, we consider it our duty to point out its situation, and give some description of its surrounding dangers, to warn mariners from approaching too near them.

"Rockall is situated in latitude 57° 36′ north, and longitude 13° 41 west. It is a large, high, and barren *rock*, of a conical shape, having its top perfectly white, from the immense quantity of bird's dung with which it is covered. With the rock bearing N. by W., broken water has been seen about a mile to the N.E. of it; and on approaching nearer, a *rock*, on which the water broke, appeared just at the water's edge. Mr. Richard Peacock informs us, that this rock appears almost like a ship at a distance, and is steep close-to on the north side; he passed it at the distance of about 50 fathoms; but to the southward, or nearly S.E. by E., from the rock, there lies a long *reef of rocks* for about 3 miles, on which, with gales of wind, the sea breaks very heavily. Other authorities confirm the assertion, that there are several dangerous *reefs* in the vicinity of this island.

"On April 18th, 1824, the *Helen*, of Dundee, was unfortunately lost, by striking on a *sunken rock*, about 6 miles E.N.E. ½ E. from Rockall. Mr. Thomas Erskine, the commander, reports that his vessel struck two different times (but did *not* stop) on a *chump of rocks*, apparently not much larger than a ship's length, and on which the sea broke occasionally: there were no other breakers near him, or in sight, at the time. Rockall bore W.S.W. ½ W., he thinks about 6 miles distant; but as the weather was hazy, the distance was probably something less. By this misfortune, his vessel became leaky; and after 13 hours' incessant and ineffectual pumping, sunk in deep water."

† In the Cornhill Magazine for January 1860, is an article on "the search for Sir John Franklin," written by an officer of the "Fox," from which the following is an extract, containing some information on this subject.

Cornhill Magazine, January 1860.

"We left Aberdeen on July 9, 1857; and after a favourable run across the Atlantic, we made our first acquaintance with the Arctic Seas when near the meridian of Cape Farewell, by falling in with the drift-wood annually brought from Arctic Asia by the great current known as the Spitzbergen current—the shattered and mangled state of these pine logs bearing evidence of their long water-and-ice-borne drift. This great Arctic current brings masses of ice from the Spitzbergen seas, at seasons completely filling up the

3080. Is that between Greenland and Iceland?—No, between Greenland and Labrador on the western shore; it would not seem to be prudent to run so great a risk. If you strike out of that route either Greenland or Iceland, then it becomes worse than the Irish route, because the distance from Iceland to Newfoundland is quite as great as from Ireland to that island.

3081. Was the circumstance of the volcanic nature of Iceland brought forward at all in the course of the communications to which you have referred?—Yes.

3082. Was it urged as an objection?—Yes, particularly by Captain Beecher.

3083. Were any facts given?—No, I think not, further or more special than the experiences of Sir John Barrow and some others; an opinion was given as to the possibility of destruction by ice and volcanoes. Nevertheless electrically there can be no doubt that the Arctic route would, if otherwise practicable, possess some advantages on account of the comparative shortness of the divided circuits, other things being equal; and that circumstance was not at all overlooked by the Atlantic company. But it would, in my opinion, be a reckless and unpardonable waste of capital to take a cable to Greenland in the present state of knowledge as to the movements of the ice upon that coast, or until a careful Government survey, *instituted for the purpose and extending over many months*, has been made with a view to gain a sound opinion as to its capabilities.

3084. Has not Colonel Shaffner advocated that route?—Colonel Shaffner has published a lecture in which he states that he has surveyed the western coast of Greenland; but his stay was necessarily so short and the facts which he gives are so slender upon which to base any expenditure of capital, that I should not consider that statement, or the casual kind of examination pursued by that gentleman, as possessing any practical value. There is one point, however, in which the Arctic route must always fall short of the Irish route, which is, that the key to the position is on foreign soil, whereas by the route from Ireland to Newfoundland both ends of the cable are in Her Majesty's dominions. As to the line to the straits of Belle Isle, that must be pronounced impracticable if any regard is to be had to the subscribers' property, because I have had the most positive evidence which demonstrates that the course of the ice, both in and out of the straits, is such as must prevent any reasonable probability of the continued existence of any cable that might be laid in that direction. The fourth proposal, from Falmouth to Halifax, I must leave mainly to the electricians, as the cable required for such a route would be, I suppose, between 3,000 and 4,000 miles, and I fancy, if their law of squares is to hold good, they will consider a line of little more than 2,000 miles a good deal preferable.

3085. What is the distance from Falmouth to Halifax, Nova Scotia?—The geographical distance is 2,756 miles.

3086. (*Mr. Varley.*) I understand that 3,000 miles of cable would do it?—I think not with due allowance for slack, especially as you could not keep a straight course along that route. The cable must either traverse the fishing banks of Newfoundland, where it would be very liable to injury, or it must go south of those banks into exceedingly deep water, far deeper, I believe, than anything the Atlantic company had to encounter between Ireland and Newfoundland. The fifth route by the Azores and the Brazils can hardly be said to come into competition with the Irish route; it might if it were established, perhaps, become a very valuable chain of communication, but the enormous depths it will involve, and the volcanic region it traverses,* joined

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fiords, harbours, and indentations on the south coast of Greenland, and often in a pack extending for 100 miles southward of Cape Farewell. A whole fleet of whale ships were, in June 1777, beset in lat. 76° north, and nearly in the meridian of Spitzbergen, and were drifted southward by the current, until one by one they were crushed. The last and only surviving ship arrived in October, in latitude 61°, in Davis' Straits, and the crew escaped to the land, near Cape Farewell, 116 in number, out of 450 men, who only a few short months before were looking forward to a happy return to their homes.

"Late in the summer, the weather mild and the nights short, and with steam-power at command, we had no occasion for much anxiety about this ice, but determined to push direct for Frederickshaab, and with a fair wind we steered to pass within sight of Cape Farewell. On the night of the 13th July, we were becalmed, and on the following day we steamed slowly to the north-westward, amidst countless numbers of sea-birds. At daylight the coast of Greenland showed out in all its wild magnificence. Cape Farewell bore north 45° east, distant 25 miles; but from the peculiar formation of the adjacent land the actual cape is difficult to distinguish. Hitherto we had not seen the Spitzbergen ice; and we hoped that we might follow the coast round to Frederickshaab without obstruction; but in the course of the forenoon a sudden fall in the temperature of the sea, with a haziness in the atmosphere to the northward, indicated our approach to ice. Straggling and water-washed pieces were soon met with, and in the evening the distant murmur of the sea, as it broke upon the edge of ice-floes, warned us of our being near to a pack.

"We made but little progress during the two following days, the winds being from the northward, and a dense ice-fog rolling down from the pack. On the 17th, Frederickshaab bearing N. 28° E., distant 50 miles, we determined upon endeavouring to push through the pack; and after being at times completely beset, and with a constant thick fog, we escaped into the inshore water, with a few slight rubs, having been carried by the *drifting body of ice nearly 30 miles northward of our* part. We sounded upon the Tallert bank; and on the fog lifting, the great glacier of Frederickshaab was revealed to us, and we bore away for the harbour, which we reached on the 19th. We had a little difficulty at first in making out the entrance to Frederickshaab; but a native kyack coming out to meet us, we were soon escorted in by a fleet of these small canoes.

"We found the natives busily breaking up the wreck of an abandoned timber ship, which had drifted to their harbour, with a few of the lower tiers of cargo still in her; and another wreck was said to be lying upon the Tallert bank—the same wreck, it is said, which Prince Napoleon had boarded on his homeward passage in the "Atlantic" the previous year, and had left a record on her to prove the currents round Cape Farewell."

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* The following extracts from the Nautical Magazine will show the constant existence of volcanic action in the region of the Azores.

Nautical Magazine, September 1841.

"ACCOUNT OF THE LATE DREADFUL EARTHQUAKE AT TERCEIRA. *Western Islands.*

"The town of Praya had in the year 1614 been totally destroyed by an earthquake, which considerably injured the town of Angra, and was felt severely in the island of St. Michael. Since that time it had escaped injury, although menaced by many severe shocks of earthquake.

"On the 12th of June last, at 4 P.M., a violent shock of earthquake was felt at Praya, extending with diminished force to the westward. At 2h. 25m. a second and more violent one was felt; on the 13th the trembling continued with short intervals, but diminished violence during the whole day. On the 14th, at 4 A.M., a perfectly perceptible undulation of the ground took place, which destroyed all those buildings which had been weakened by the former shocks. The inhabitants of Praya then retreated to the fields in the neighbourhood for safety. With the exception of occasional slight motions, the island was undisturbed during the remainder of the 14th, and hopes were entertained that the convulsions had ceased. But on the 15th, at 3 A.M., a violent trembling and horizontal undulation of the ground commenced, and continued with intervals of ten minutes, and a duration of about ten seconds until 3h. 30m. A.M., when a strong vibratory and distinctly visible rocking motion was communicated to the surface, which threw down the entire town of Praya, and several chambers and houses of the adjacent villages, and considerably injured the remaining houses of the villages and many elevated public buildings in other parts of the island. The ground then remained in a comparative state of rest until 2h. 40m. A.M. on the 16th, when a violent shock of earthquake did further damage. After this, although the island did not resume a permanently quiescent state until the 26th of June, no further damage appears to have been done.

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"In former years, mentioned by Buffon in his second volume of Natural History (on the authority of official communications) submarine explosions have taken place between St. Michael and Terceira, which were succeeded by the appearance of newly raised volcanic islands above the surface. On these occasions earthquakes were felt on both islands, most severely in that one to which the eruption was nearest. In 1811 the volcanic islet ejected near St. Michael, and discovered by the officers of H.M.S. "Sabrina," came attended by great convulsions on that island, which were not felt at Terceira, and the force of which was considerably diminished at a distance of fifty miles from the then ascertained centre. It is therefore probable that the origin of the earthquake of last June was a *submarine volcanic eruption*, and that its position or centre was about seventeen miles due east from the eastern end of Terceira.

"This hypothesis may not be without some practical utility. The greater number of seventeen earthquakes which are on record as having taken place in the Azores, have been accompanied by the appearance of volcanic islands over their centres: these islands have by the erosion of the sea gradually disappeared, but during this process have been highly dangerous to ships sailing in their direction. For some time, therefore, after submersion, it must

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to the million and a half of money which would have to be expended upon it, would render mature consideration very desirable before entering into such a speculation.

3087. (*Mr. E. Clark.*) Is not there another route across Russia, which limits the sea journey to 30 miles, over Behring's Straits?—I have not taken that into consideration.

3088. Is not that the cheapest and easiest of all?—The cheapest to erect, but the slowest and most expensive to work, and very far the most difficult to keep in working order. It could never be depended on.

3089. (*Mr. Varley.*) I think there were two messages sent through the Atlantic cable before it failed for the Government about troops. A day or two ago I was in conversation with a member of the present and several previous Parliaments, and on mentioning the fact to him he was not aware of it; will you be so good as to state what you know about those messages, and what estimate you have formed as to the benefit derived from their transmission by the British Government?—The facts, with regard to the messages, were these. During the Indian rebellion it had been expected that it would be necessary to recal further troops into England for the purpose of sending them to India; amongst others an order had been sent out to Montreal and Halifax respectively for the 32nd and 69th regiments to come home. This order was a written one. The state of affairs in India meanwhile took a more favourable turn, and the War Department considered that it was no longer necessary that those troops should return, in consequence of which two messages were sent, through the Atlantic telegraph cable; one to Montreal and one to Halifax, giving orders to the commanding officers at those places not to send home those regiments. In answer to the remaining part of your question, as to the expense saved, of course it would involve the expense of ships to bring those troops home, and subsequently ships to return them to the same place again. The outlay for that purpose certainly could not be estimated at much less than 40,000*l.* or 50,000*l.*

3090. Have you observed, while you were connected with the British Telegraph Company, any underground wires destroyed by lightning?—I have had specimens brought to me, which were said to have been injured by lightning.

3091. Did you notice any peculiarity about those specimens to lead you to see how the lightning had struck the wires?—Not how it had struck them, but how it affected the gutta-percha.

3092. Was it at all similar to the appearance exhibited by the specimens which have been produced?—No. The gutta-percha in the cases which I have seen was split open, but not riddled like the specimens I have seen on the table. I had never seen one affected in that way by lightning until I saw it here. In those that I have seen the lightning seemed to have flashed open the gutta percha altogether.

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have been necessary for the commanders of such ships to observe great caution in passing by the Azores, that part of the sea in which the eruption had taken place. A similar caution would be necessary now. It is by no means a great stretch of hypothesis to suppose that the late earthquake has, like former ones, been accompanied by the ejection of *submarine volcanic matter*, which may have been thrown up within a short distance of the surface; so that, in fact, in that part of the sea where there was previously 200 fathoms of water, there may at this moment exist a most dangerous shoal.

"T. C. HUNT."

Nautical Magazine, November 1841.

"Capt. Boyd gives a list of the eruptions of St. Michael which deserves attention; it may be seen at once that the orifices of these eruptions are continually changing, and do not show any tendency to a common crater.

"After the elevation of Alagoa de las siete Cidades, the island remained tranquil. In 1522 an eruption hurled into the air the two hills, Sorical and Rubacal, and covered the town of Villa Franca, which was entirely destroyed. Four thousand inhabitants lost their lives on this occasion.

"In 1563 there was an eruption of the Pico Sapadeiro. A large current of lava ran into the sea on its northern side near Rebeira Secca.

"In 1638, a large island appeared 15 miles to the west of St. Michael, it remained quiet for several years, and then disappeared suddenly leaving in its place a fathomless ocean.

"In 1652 the hills of Pico do Foro, Romos, and Pico do Pava, to the north-east of Rosto do Cao, near Punta Delgada, threw up an immense quantity of stones and cinders, spreading destruction over the surrounding country.

"In 1691, after some very violent earthquakes several small islets appeared not far from the coast.

"In 1719, a new island appeared 15 leagues west, its diameter was nine miles, and it disappeared in 1723, leaving 70 fathoms water.

"The great earthquake of Lisbon in 1755 was felt at St. Michael by severe shocks without an eruption.

"On the 11th of August 1806, violent motions of the earth were felt. In the northern part of the island fire burst from the fissures, and there was an eruption of the peak of Genates in the south-west part of it.

"On the 19th of June 1811, the island of Sabrina appeared and disappeared in the month of October. From that year till 1835, the island remained undisturbed.

"The island of Terceira has a crater, six miles north-west of the town of Angra. Large fissures on the sides of the mountain emit vapour in abundance. These fissures were formed after the earthquake of 1614, by which the town of Praya was destroyed. From that time these phenomena have ceased on the island. The crater is called Furnas d'Euxofres; it appears to be entirely surrounded by hills of pumice stones.

"Captain Boyd states that these stones frequently fall and throw down trees, which being buried beneath them, make it appear that they were surrounded by the eruption of these stones. A single eruption which took place in 1761, poured forth lava from the peak of Bagacina, which, after running a league in extent, fell into the sea.

"The island of St. George, so close to the central volcano of Pico, is also the most agitated. An eruption in 1580, a league and a half from the port of Velhas, lasted several days, and numerous currents of lava ran into the sea, where it formed an indented and steep coast. In 1691, eruptions appeared in the sea. Many small islets appeared near the coast, but disappeared soon after. This phenomenon occurred again in 1720, the year in which the island appeared at the south-west extreme of St. Michael, and about a mile from the shore; and in 1757, eighteen small islets appeared about a thousand feet from the coast, which, after a few years, disappeared. In May 1808, the great eruption took place described by Mr. Dabney."

Nautical Magazine. February 1858.

"REMARKABLE PHENOMENON NEAR THE AZORES.

"The subjoined deposition and letter, having reference to a presumed submarine convulsion observed near the Azores in November last, have been received at the Admiralty:—

"British Vice-Consulate, Terceira, December 21st, 1857.

"SIR,—I have the honour to enclose copy of a document obtained at my request from Mr. William Cook, Master of the British schooner 'Estremadura,' of Glasgow. It appears that he did not think of trying the temperature of the water, which is to be regretted, as it might have tended to prove that the effects observed were those of submarine action. No shock of earthquake was felt at this island at the time. It serves, however, to show that mariners never can be too cautious when approaching these islands, as from one day to another there is no knowing what formation may take place either above water or awash; for instance, the island Sabrina, in 1811, and the danger seen by three vessels proceeding to St. Michael's in 1849, about 40 miles to the W.N.W. of that island, which gave rise to Mr. Consul Hunt inducing the Masters of the vessels 'Æolus' and 'Prospero' to go in search of it, but without success.

"I have, &c.

"JOHN READ, H.B.M. Consul."

"To the Secretary of the Admiralty."

"*Deposition.*

"I, the undersigned, Master of the British schooner '*Estremadura*,' of Glasgow, whilst on a passage from Lisbon to the Island of Fayal, with a cargo of coals, do hereby declare that in lat. 39° 27' N., long. 29° 50' W., at seven P.M. of the 25th of November last, I observed abaft the beam what I considered to be a squall, but which eventually turned out to be a kind of mist or warm steam. It being my watch on deck, I asked the helmsman if he found any difference in the air; to which he replied, it was quite warm. I called the mate up, and he as well as those on deck felt the same. This lasted for half an hour; there had been no fog or mist before. The wind at the time was N.E. by compass, and continued so throughout without any alteration in force. We had a following sea previous to falling in with this mist, but the sea then changed to a kind of boil or topping sea, as if surged up from beneath, but afterwards returned to the former state when we were clear of the mist. When this phenomenon occurred the Amplimont Rock, as laid down in the chart, would bear N.b.E. by compass, distant 140 miles. I calculate the vessel to have been going at the time from 7 to 7½ knots, with a following sea. Sighted Terceira on the 26th, and anchored in Fayal Roads on the 27th November.

"WILLIAM COOK.

"JOHN READ, *Consul.*"

"Those who know something of our pages will remember our account in 1841 of the different earthquakes and convulsions of nature which have taken place at and in the neighbourhood of the volcanic region of the Azores, and they will not marvel at the foregoing account, which has recently appeared in the daily papers; but we preserve it as an addition to the history of that unquiet region. At the same time, as such phenomena may be always anticipated in that neighbourhood, we caution seamen to be on the look-out, especially when east of Terceira, or between it and St. Michael."

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3093. Can you give the Committee any information as to the power and the kind of battery used for working through the British Company's wires from London to Liverpool; when they began to fail, I believe you noticed some peculiarities in them?—I did; I found that as many as 36 twenty-fours of Daniell's battery were being used upon them. The person in charge said he could not make it work, and he put several more batteries on; he said there was no sign, and I remarked, "Supposing you try the con- "verse, and begin to take a few off, and see if it will do "any better then." He did so, and after he had taken off I think 12 of those batteries, the line began to work again with the smaller power, and continued to work even with much lower power than that for some time afterwards. It seemed to me, although I could not presume to give any electrical opinion upon the subject, as though he had attained the maximum power that would do any good.

3094. How many of the six original wires worked from the first?—At first five worked for a very short time fairly; one never did work.

3095. How long did they last in workable condition?—Not more than eight or nine months.

3096. Have you any record as to the speed that you obtained through those wires?—The speed that could be obtained when they were working fairly and using a little instrument invented by Mr. Edward Highton, which is a single needle instrument, with two good manipulators, was 20 words a minute.

3097. You mentioned having noticed that underground wires in chalk were a good deal injured; did you ever in your experience with those wires find any case in which the heat of the sun had penetrated the ground sufficiently to melt the gutta-percha?—No; I never saw any case in which the gutta-percha had been melted from that cause. I have had it taken up from the vicinity of baker's ovens, and found it matted together under those circumstances. But I attribute the injury to gutta-percha in chalky soils to the fact, that it was not sunk sufficiently deep into the soil; consequently it was continuously in the presence of a dry warmth, which I think is fatal to gutta-percha.

3098. We have had several pieces in chalk cuttings near Watford at the bottom of the cutting where the white banks condense the heat of the sun on to the lower portion of the cutting; in such places we have had the gutta-percha so melted that it has oozed out between the folds of the tape covering?—I have never seen anything so bad as that.

3099. I believe you have been of opinion from the commencement that a larger conducting wire would very much contribute to a higher rate of signalling, and also to much greater regularity in the received currents at the distant end?—I have, on the ground of its analogy, in my opinion, to a water pipe, which applies or nearly so to an electric conductor. If from its great length you are necessitated to put in a larger force you require a larger pipe to conduct that quantity of fluid; whether water or electricity, which is poured into it.

3100. The directors of the Atlantic Telegraph Company, I believe, were under the impression from a letter written by Professor Morse that they should obtain easily nine or ten words a minute. Were you present when any experiments with the 3,000 miles of line were made by which those conclusions were arrived at?—Not at the actual experiment. I saw, however, the means provided for carrying out those experiments.

3101. Do you not think they were deceived in some way or other by those experiments, and that they could not have been working through the whole length of the line?—Certainly, they worked through the whole of the line; but I think it is very possible that at the points at either end where the wires were connected together so as to form one continuous wire, there might have been some element of disturbance to their theory which was not taken into account; for instance, there might have been an escape at those points, which would of course lessen the induction.

3102. Do not you think that cables ought to be made after the summer has passed; manufactured during the winter, and submerged, if possible, before the summer has attained its maximum heat?—I think it would be very desirable; and I think the more a cable is kept under water the better for its preservation, besides giving additional facility for testing.

Adjourned to To-morrow, at Half-past Two o'clock.

Friday, 13th January 1860.

PRESENT:

Captain DOUGLAS GALTON.	Mr. EDWIN CLARK.
Professor WHEATSTONE.	Mr. VARLEY.
Mr. G. P. BIDDER.	Mr. SAWARD.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

Commander JOSEPH DAYMAN, R.N., examined.

Commander J. Dayman, R.N.
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3103. (*Chairman.*) You have had considerable experience, I believe, in deep-sea soundings connected with submarine telegraphs?—I have.

3104. Have you not conducted several expeditions for the purpose of obtaining soundings between England and America?—Yes; between England and America, and also between England, Gibraltar, and Malta, and to the Azores.

3105. What are the methods which you have adopted for deep-sea soundings?—Between England and America I adopted a modification of the American plan usually known as Lieut. Brooke's, U. S. Navy.

3106. Will you describe the modification?—I can hardly describe without drawing it; the modification refers principally to the method of bringing up specimens of the bottom, and to the shape of the sinker.

3107. Will you be good enough to describe the system which you pursue for taking deep-sea soundings generally?—Brooke's system is distinguished by an apparatus for detaching the weight when the bottom is reached, and a specimen of the bottom is by its means procured at each sounding.

3108. Is it the striking of the weight against the bottom that enables you to know that you have reached the bottom?—Yes.

3109. (*Mr. Bidder.*) Do you find the reaching of the bottom quite distinctly marked?—Quite distinctly.

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3110. Have you any doubt as to the accuracy of the soundings obtained in that manner?—I have now no doubt of their accuracy. I should state, at the same time, that depths obtained in that manner are somewhat too large, and subject to a correction amounting to about 150 fathoms in 2,000.

3111. To what do you attribute the excess of depth registered from that actually prevailing?—First, to the difficulty of exactly estimating (by means of noting the intervals of time at which equal portions of line pass out) the moment when the weight reaches the bottom.

3112. (*Chairman.*) Are those intervals the intervals of time during the revolving of the machinery?—The intervals of time at which equal parts of the line leave the reel, or ship, or boat.

3113. What correction do you apply to get rid of that anomaly?—A correction has been found, by subsequent experience, in a different mode of obtaining depths.

3114. What mode was that?—By discarding the detaching apparatus of Brooke's, and using instead a large fixed weight, which is lost at every sounding.

3115. Do you mean that you sent down a large fixed weight, and then pulled the cord taut until it broke?—I used a large weight, greater than the line could lift off the ground in great depths, and which enabled me, by employing a strong line, to get the nearest possible approach to a measure of the perpendicular depth, the weight being down, and remaining at the bottom whilst the line was tightened.

3116. (*Mr. Bidder.*) Where was the fracture occasioned; had you part of the line weaker than the rest?—No, the line was equally strong throughout.

3117. Were you sure that it would break at the bottom close to the weight?—No; it might break at any part of the line; sometimes it was necessary to cut it.

3118. (*Mr. Varley.*) Did you ascertain the depth by the cessation of the running out of the line?—Not always.

3119. (*Chairman.*) By hauling it taut?—By hauling it taut, either by the reel or by a number of men, very gradually to its extreme tension.

3120. (*Mr. Bidder.*) What weight did you attach to the line?—According to the size of the line employed; on one occasion 188 lbs., and on another, 100 lbs. of lead.

3121. (*Chairman.*) What was the size of the line?—It was contract line made on purpose—small and light; I cannot give the exact measure of the line, not being now in command of the ship in which the soundings were taken, but have no doubt that a specimen of it might be easily procured; there is nothing peculiar about the line.

3122. (*Mr. Edwin Clark.*) Was it a hemp twisted line?—Yes, made by Messrs. Newall.

3123. You said there were two reasons why the depths you obtained were rather greater than the actual depths. One reason was because you had a difficulty in ascertaining the intervals of time and the length of the line run out. You did not state the second reason?—The second reason was that in consequence of the weight in the American plan being detached immediately that it strikes the bottom, there is no means, excepting by guess, of correcting inaccuracies in the measurement. The weight being gone, immediately you pull upon the line, it comes up with all its curves, whose lengths are unknown.

3124. With any belly it may have got?—With any curves it may have described.

3125. (*Mr. Varley.*) Supposing a steel wire were used of very small diameter attached to the weight instead of a hempen line, do you think that would give more accurate results than when you use a line with a large surface; it appears to me that the surface of the line is the great difficulty in the way of getting correct soundings?—I do not know what you mean by a steel wire.

3126. If instead of attaching a cord to the weight of, say, one-sixth of an inch in diameter, you attached a fine steel wire of one-fiftieth of an inch in diameter, and let that run out, inasmuch at its surface would be very small compared to that of the cord, would you not get more accurate results than when you used a hempen line?—I cannot answer the question, never having tried wire; it depends upon its relative weight and strength. If the steel wire could be made of equal strength to the hemp line and of smaller weight, I should suppose that the accuracy of sounding would be greater with a steel wire than with hempen line.

3127. (*Mr. Bidder.*) It would be less affected by currents?—It would be less affected by currents; but I should say that wires have been tried and have failed in deep sounding from the fact of not being able to know by the time intervals when the weight ceases to descend.

3128. (*Mr. Varley.*) Has not copper wire been generally used for that purpose?—Iron wire.

3129. (*Mr. Bidder.*) In taking deep-sea soundings, do not you generally choose calm or moderate weather?—Moderate weather is necessary; calm weather is not.

3130. (*Chairman.*) How long does it occupy to take a sounding in 2,000 fathoms?—I will quote two examples in the "Gorgon," one the 15th of October 1858 and the other the 16th,—2,400 fathoms, 48 minutes 1 second; 2,400 fathoms on the next day, 48 minutes 5 seconds. Recently in the "Firebrand," with a weight of 100 lbs. and the line before described, the taking a sounding of 2,400 fathoms occupied 47 minutes 48 seconds.

3131. Did you mark the position of the sounding on the chart by means of the dead reckoning?—By means of astronomical observations and distances run by patent log, and when in sight of land, by bearings.

3132. Then you require clear weather for the purpose?—The stars were employed, and the sun.

3133. (*Mr. Bidder.*) Did you succeed on any occasion in hauling the line taut in lifting the weight off the bottom before you broke the line?—In no case, in the great depths.

3134. (*Chairman.*) In all cases of soundings taken between England and America, did you bring up parts of the bottom?—Yes, in each sounding.

3135. The bottom consisted of very fine shells, did it not?—The bottom consisted of matter which, for want of a better name, I call ooze. I believe Professor Huxley has stated that it is wholly composed of microscopic shells.

3136. The same material of which chalk consists?—Yes.

3137. Have you made observations upon the temperature of the bottom?—Not in my recent voyage in the "Firebrand," but before that I made experiments in various depths in the North Atlantic.

3138. What temperature did you find the bottom to be?—The minimum temperature, to the best of my recollection, at about 1,900 fathoms was about 39° Fahrenheit.

3139. In what way were those experiments made?—With maximum and minimum self-registering thermometers, sent down in copper bottles.

3140. Did you ascertain the temperature at different depths at the same time by different thermometers?—According to the time that was at my disposal, I sent down two or three bottles at different parts of the line so as to get the temperature at different depths.

3141. Did you find much difference between the

temperatures at the different intervals ?—Yes, a great difference.

3142. Can you state what the temperatures were at the different depths in different cases? what was the temperature of the surface, to begin with?—I find from my report on board the "Cyclops," at 1,000 fathoms, the temperature was 40·8°; at 1,700, 38·6°; at 2,320, 39·2°; and at the surface, 54°.

3143. (*Mr. Edwin Clark.*) Was any general law established at all with respect to the decrease of the temperature as you described?—The general law in the North Atlantic, in summer, appears to be, that the temperature decreases regularly from the surface of the sea, to a certain depth (which depth varies according to geographical position), and from thence downwards it either increases or remains the same to any depth whatever.

3144. Have you experimentally found the depth beyond which the temperature began increasing again?—I do not think that point is sufficiently established, but I believe it to be, in that part of the Atlantic where the experiments were made, about 1,900 or 2,000 fathoms.

3145. That is merely an opinion, not supported by any positive facts?—No; the experiments are not sufficiently extensive to establish the point.

3146. (*Chairman.*) Were you induced from your experiments in the "Cyclops" to believe that the temperature rises after it passes a certain depth?—From those and other observations made by Sir James Ross, in the Antarctic expedition, under his command.

3147. At what depths were the experiments made in the Antarctic expedition?—Speaking from memory, I think a number of experiments were made, to about 1,000 fathoms.

3148. (*Mr. Varley.*) How did you take the temperatures at great depths, did you measure the minimum temperature or the maximum temperature to which the thermometer had been exposed?—The thermometers having been put into a bucket of the surface water, and their indices brought by a magnet to its temperature, are then sent down to depths previously determined on, and both the minimum and maximum temperatures are read on their return. The accuracy of a determination of the temperature at great depths may in some measure be tested by noting whether the maximum index has moved; the minimum being the register of the temperature, as it is always colder in the Atlantic in summer, at the bottom than at the surface.

3149. (*Mr. Edwin Clark.*) Does not the great pressure to which the thermometer is subjected at a great depth interfere with its height?—I think not.

3150. (*Chairman.*) What arrangement did you make for preventing the shock of the weight striking the bottom from injuring the thermometer?—I never allowed the thermometer to go to the bottom.

3151. To within what distance of the bottom?—Not to within fifty fathoms of it.

3152. (*Mr. Varley.*) Do you not think that the apparent decrease of temperature at the increased depth might have been caused to a certain extent by the compression of the bulb of the thermometer, or was any precaution taken by inclosing the thermometer in a glass tube, which should bear the pressure and relieve it of that source of error. There is an instrument made by Negretti, indicating the compression of steam which depends upon the compression of the glass bulb throwing the mercury up into the tube. It seems to me, that it is possible such a thing might take place with the thermometer, unless precautions were adopted for relieving the thermometer of the strain by enclosing it in another vessel?—The thermometers were made expressly for the purpose by Negretti and Zambra, they were enclosed in copper bottles with about an eighth of an inch clear round them. I do not think it probable that their indications were affected by compression, as faulty thermometers break under the pressure.

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3153. Was that copper vessel hermetically sealed before the thermometer was submerged?—No; a certain amount of water reached the thermometer.

3154. Then the copper bottle was merely a safeguard to prevent accidents to the thermometer, and not to relieve it of pressure in any way?—Not to relieve it of pressure.

3155. (*Mr. Saward.*) Have your observations tended to any examination of the question of pressure in deep water affecting bodies?—I made an attempt at experiments of that kind the other day, but I am sorry that I cannot give the results, and can only generally describe the experiments. I sent cubes of wood (oak, elm, and fir) a specimen of each, down to different depths to find out at what depths they would become sodden, and also their specific gravities, when sodden at different depths. These are the only experiments that I have made upon pressure.

3156. Was your mind directed to the question as to whether a cable would be likely to be injured by being submerged in deep water?—I never had an opportunity of making experiments with a cable on the subject.

3157. (*Chairman.*) Did you bring up specimens of the bottom in the Bay of Biscay on your voyage in the "Firebrand"?—I did; they were sent to the Admiralty.

3158. Had you any greater depths than 800 fathoms?—The specimens were sent for, of part of the Mediterranean as well, where the depths reach to 1,500 fathoms and upwards. In the deep sounding which I mentioned just now, as taken in the "Firebrand," of 2,360 fathoms in the Atlantic, the bottom was brought up.

3159. What was the nature of the bottom in that case?—The same, as far as I could judge, as had been before found by me in the Atlantic, 700 miles further north, on board the "Cyclops."

3160. (*Mr. Bidder.*) What were the specimens of the bottom of the Mediterranean?—To the naked eye they were mud. I had no microscope, so that I could not decide whether they contained anything else.

3161. (*Chairman.*) Did you observe any peculiarities in the soundings in the Mediterranean which are worthy of notice with reference to an alteration of the bottom since the former soundings?—I observed a great difference in the depths before recorded and my soundings in the Strait of Gibraltar, where they are about half as large as those shown in former English charts.

3162. Do you attribute that to any alteration of the bottom going on by silting up?—I am not competent to answer the question.

3163. Or do you think that those differences may arise from errors of observation in a part of the sea where it was very difficult to obtain soundings, in consequence of the current? Do you think they can be accounted for by any inequalities of the bottom, and that your soundings may have been taken, for instance, on the side of a hill, where the slightest deviation to the right or left would make a great difference?—The differences, perhaps, may be accounted for partly in that way, but not wholly.

3164. Do you think there is any part of the Gut of Gibraltar, which is as deep as it is represented on the Admiralty survey?—I do not.

3165. Do your soundings agree with the French soundings?—They agree with the French soundings, except that we have discovered one single patch of shoal water which has escaped them.

3166. What is the nature of the bottom of the Gut of Gibraltar?—Dead coral and rock in its western side, and mud to the eastward, near Europa Point.

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3167. Would it be possible to lay a cable in the Gut of Gibraltar from the Atlantic without passing over rock or coral?—No.

3168. Would the cable be subjected to strong action of the tide or current?—I believe it would be subject to the action of a strong current.

3169. (*Mr. Bidder.*) What is the velocity of the current?—It is variable, depending upon the strength and the continuation of winds from certain directions.

3170. Within what limits is the velocity?—The velocity of the surface current varies from two to four and a half knots per hour.

3171. Is there any way of approaching Gibraltar itself from the Atlantic, so as to confine the region of rock to short lengths?—I pursued a track in the "Firebrand" with that object, which is marked upon the chart sent to the Admiralty.

3172. (*Mr. Edwin Clark.*) Have you ever met with any depth so great that it rendered it impossible to obtain a sounding?—I have not, in the course of my experience.

3173. If there were such depths as four or five thousand fathoms, would it be impracticable to obtain soundings, or where would be the limit do you think?—It would not be impracticable to obtain a sounding, I think; but the accuracy of the measurement obtained by the present method of sounding diminishes in proportion to an increase of depth. At the same time, I should state that I have no doubt of the accuracy of depths measured by the plan which I have described so far as they go.

3174. (*Chairman.*) In the course of the soundings from England to Gibraltar, did not you perceive very great inequalities in the depth in certain parts approaching, for instance, Cape Finisterre and when you were nearly off Lisbon?—Along the whole of the Western Coasts of Spain and Portugal from Cape Finisterre to Cape St. Vincent, the depths decrease very suddenly in approaching the shore, more so than I have found anywhere else.

3175. More suddenly than in approaching the coast of Ireland?—Yes.

3176. In what distance did you obtain a decrease of depth?—Speaking from memory there is, in a distance of three miles, a decrease of depth from 1,430 to 650 fathoms.

3177. A difference of 800 fathoms in three miles?—Yes.

3178. Can you say at all whether there are cliffs near that part at the bottom of the sea, or whether it is a regular shelving descent?—I think that it is a regularly shelving descent.

3179. Before attempting to lay a cable in that part of the sea, should you think it desirable to take more soundings, with a view to ascertaining that fact?—I should not.

3180. Will you inform the Committee whether you have observed any peculiarities about the soundings in the Mediterranean between the coast of Sicily and that of Africa?—I found the depths of the sea greater there, and more regular, than are marked in some charts.

3181. And therefore you think it possible that some alteration may have taken place in the bottom since that time?—At first I was led to suppose so; but as the nature of the bottom is of the same character throughout, (soft mud,) I am now in doubt whether the change can be attributed to volcanic action.

3182. Have you had any experience in sounding on the Adventure Bank?—I have not sounded there.

3183. You are aware that alterations are supposed to go on from volcanic action adjacent to that bank?—My attention was called to that subject.

3184. You do not think that any similar alteration of the bottom had gone on south of Pantalaria in the line of soundings which you took?—I think not.

3185. Have you taken any soundings in the North Sea near the coast of Greenland or Iceland, or anywhere to the north of the line of soundings for the Atlantic Company?—No.

3186. Have you had any experience in those seas?—I have not.

3187. I believe you accompanied the Atlantic expedition on each occasion of its proceeding to lay the cable?—Yes, from the commencement to the end.

3188. What duty was assigned to you?—My first duty was to sound the ocean across.

3189. That was before any expedition sailed?—Before the laying of the cable was undertaken.

3190. Then you accompanied the expedition which laid the cable?—I accompanied the American frigate "Niagara" to Trinity Bay (Newfoundland) with the western half of the cable.

3191. In order to show the way?—In order to render assistance as it might be required.

3192. Do you consider it essential to the success of laying any long line of ocean telegraph that a pilot vessel should accompany the expedition, or do you think all the observations could be made from the ship which is laying the cable, either across the Atlantic or any line of ocean telegraph where particular attention is required to be paid to the course?—There was difficulty in steering the "Niagara" from the passing out of the cable affecting her compasses.

3193. (*Mr. Bidder.*) Were you at all sufficiently near the "Niagara" to observe what was going on?—I was constantly near her.

3194. Did anything strike your mind as to whether any improvement might be introduced in the mode adopted for passing the cable out?—The paying-out gear in the "Niagara" on the last occasion when the cable was laid appeared to me to be as perfect as possible, and to answer its purpose fully.

3195. What was the smallest speed at which you went in laying the cable?—I think never less than from three to fourknots, and as much as seven.

3196. Supposing a vessel could take the whole of a cable on board from America to England, would you prefer leaving the coast of England or the coast of America?—I should prefer leaving the coast of America.

3197. On account of the prevailing winds?—On account of the prevailing winds, and of the fogs and ice sometimes met with, which render it difficult and occasionally dangerous to make the coast of America from seaward.

3198. (*Chairman.*) Has the experience which you acquired in accompanying the Atlantic expedition led you to form any opinion as to the class of cable you would adopt for deep-sea telegraphy, with regard to the facility of paying it out, or otherwise?—It did not appear to me that there was any difficulty in paying out the Atlantic cable which is now laid; but I think it would be an improvement if a cable of equal strength and of smaller weight could be adopted.

3199. You think it would be desirable for the cable to sink more slowly than that one sank?—Yes, I think it should sink more slowly.

3200. (*Mr. Varley.*) Did you observe any peculiar tendency in the Atlantic cable to untwist as it sank in the water?—I have heard that stated, but I had no means of ascertaining whether it did so or not.

3201. Do you find that hempen log lines untwist much from the action of the water on them when a ship is sailing rapidly?—I think not.

3202. What is the greatest length to which log lines are generally allowed to go out behind a ship?—About 500 feet.

3203. Two hundred yards of log line, when lying out behind a vessel, does not untwist to any material extent by the action of the water?—Certainly not, in my experience.

3204. (*Mr. Saward.*) With regard to sounding lines, which are twisted up tightly, I believe, have you observed any action in them?—Not of untwisting.

3205. Do they come up clear, or in kinks, when they are drawn up?—The sounding lines that I have used have come up clear, excepting on two or three occasions, when a greater amount of line has been paid out than the known depth. In these cases the line has coiled on the bottom.

3206. The inference, then, would be, that there is no serious twisting or untwisting?—There is a rapid spiral motion, which is very evident in the sounding line as it descends.

3207. (*Chairman.*) Do you attribute that to the friction of the water?—To the friction of the water acting upon an uneven surface.

3208. (*Mr. Saward.*) That, if I understand you, does not so interfere with the texture of the line as to draw it into kinks, or otherwise impair its utility?—I do not think that the operation of sounding would settle that question, because the moment you pull the line taut after sounding, you get rid of any kinks which may have been taken in whilst going down.

3209. (*Mr. Varley.*) Would not a log line which frequently lies out behind the ship for a long while, solve that question?—No; the log line is only out for a few minutes whilst the ship continues in motion; and when it is stopped from running, any kinks which may have been taken in are taken out by the strain brought upon it by the ship's continued progress.

3210. If there were any serious twisting action it would untwist the rope entirely, or nearly so?—I have never observed that; nor have I known an instance of a line being untwisted in the operation of heaving the log.

3211. Did you make any experiments in the Atlantic to determine to what depth the Gulf Stream extended. Did you ascertain any law as to the decrease of speed in any one direction. Did you find any depth, or have you any means of indicating whether at any great depth there was a current flowing in the opposite direction?—The course of the Atlantic cable lies to the north of any perceptible influence from the Gulf Stream; I have, therefore, made no experiments on the Gulf Stream with respect to under-currents, but I am of opinion that in that part of the Atlantic they do not exist, or are not of appreciable force. My reason for believing that is, that on one occasion when a sounding was obtained by me, a much larger quantity of line was paid out than the known depth purposely, and the line was coiled at the bottom nearly to the whole amount of the excess over the depth, showing that it must have descended nearly perpendicularly. If under currents had existed they would have carried the line away from the perpendicular in proportion to their strength.

3212. (*Mr. Edwin Clark.*) How did you ascertain that the line was coiled at the bottom?—By the part which had lain in coils there coming up covered with ooze; the interstices of the line were filled up with it to a surprising extent. That occurred twice in the Atlantic, and leads me to believe that there are no appreciable under-currents there; but I have no further data to give upon this subject.

3213. (*Chairman.*) Can you give the Committee any opinion as to the practicability of laying a telegraph to America by Iceland, Greenland, and Labrador; or upon the advantages or disadvantages of such a route, compared with that from the coast of Ireland to Newfoundland?—I should think that the depth would probably decrease in going north, and approaching the Greenland shore; the distance across would also be lessened; but difficulties might probably arise from grounded ice near the Labrador and Greenland shores, and from icebergs further off.

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3214. (*Mr. Saward.*) Would those difficulties be so great as to induce you to prefer the Irish and Newfoundland route?—In ignorance of the depth, and never having been in that part of the world, I cannot give any opinion worth having.

3215. It was the Antarctic expedition to which you belonged?—Yes.

3216. (*Mr. Varley.*) Do you think it would be possible by means of steam vessels to lay a cable in between the surface ice that floats about in the northern region?—We laid the cable from the "Niagara" between small icebergs in safety, but that is very different to what is called pack or even pan-cake ice; I should think that it would be impossible in such ice.

3217. Even if you paid the cable out through the bottom of the vessel?—I think the floating pieces of ice would snap the cable wherever it was paid from the vessel.

3218. Have you any idea to what depth those pieces of pack ice extend?—I have not.

3219. (*Mr. Saward.*) Would not the cable be in continual danger after it was laid from the breaking up of the ice, tearing away portions of the coast and bottom near the coast?—That would, I think, depend upon the depth near the coast; if the sea is deep, that danger would not probably have to be apprehended.

3220. I believe at present there exist no surveys of Greenland made with any object that would be analogous or have reference to a case like the laying of the Atlantic cable?—The shores of Greenland are well known and laid down in the Admiralty charts; with regard to the depths, I know nothing of them.

3221. (*Mr. Edwin Clark.*) Is there any description anywhere published of the American apparatus for ascertaining depths?—Lieutenant Maury, of the United States navy, has described the apparatus in his book on Physical Geography, it is also fully described in my report of the voyage in the "Cyclops," and drawings are there given of it.

3222. (*Mr. Varley.*) You are aware that, electrically speaking, it would be very much easier to work through a cable 700 miles in length than it would be through one of 2,000 miles?—Yes.

3223. Therefore it is a matter of great importance to know whether the northern route is possible on that account alone. Can you suggest any means by which such information can be obtained with respect to the route from the north of Scotland to Iceland, from Iceland to Greenland, from Greenland to Labrador or Newfoundland, both as to laying and to subsequent maintenance of a line in working order. If that route be practicable, it would increase the speed of working through the cable some nine times?—I am not aware of any source whence information can be got excepting from the published Admiralty charts and also from an expedition which I saw in the papers as being in progress by a foreign Government.

3224. (*Mr. Saward.*) You are not aware that that expedition is by a foreign gentleman, Colonel Shaffner, on his own account?—No; that is the only source from which I am aware that information is to be obtained.

3225. (*Mr. Varley.*) Would you recommend that soundings should be taken or some small pieces of cable should actually be submerged?—I think that the opinion of officers who have been in the Arctic regions should be taken as to the question of ice near the shore. I believe, (that is,) I have heard or read that on the western side of Davis' Strait, near the land, there is constantly a large mass of ice at all

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seasons of the year, which might be known by reference to captains of whalers, or naval officers who have navigated those seas. I believe also there is a large mass of ice which always remains on shore in Davis' Strait.

3226. Through which you could not get?—Not except by a doubtful passage; and approach to the Greenland shore is often impracticable.

3227. (*Mr. Saward.*) May I gather so much from your evidence as to understand, that at all events, before any large amount of capital were expended in laying a cable in such a route, it would be desirable to have special observations extending over many months, made to determine the practicability of laying and maintaining such a work as a cable?—I should say that observations would be necessary on the subject.

Mr. C. West.

Mr. CHARLES WEST further examined.

3228. (*Chairman.*) I believe you are desirous of saying something upon the origin of submarine telegraphs?—I am. Since I was examined here the last time, the report of the correspondence between the Treasury, the Foreign Office, the Office of Woods, &c., the Admiralty, and the Submarine Telegraph Company, on the recent convention between that company and the French Government, has been put into my hand, and I am desirous of correcting the mistake that the Messrs. Brett were the originators of the submarine telegraph between Dover and Calais.

3229. What are the facts of the case?—The facts are these, that in the year 1845 Captain Taylor and myself applied to the British Government, and also to the French Government, I undertaking to apply for the permission from the English Government, and Captain Taylor going over to Paris to get permission from the French Government. I applied first to Sir Robert Peel, and he referred my letter to the Admiralty, and I got from the Admiralty within three days afterwards a letter granting us the permission that we sought for.

3230. Was that in 1845?—This is the official letter dated the 10th of January 1846:—"Admiralty, "—Gentlemen,—Your letter of the 6th instant "having been referred to this department by Sir "Robert Peel, I am commanded by my Lords Com- "missioners of the Admiralty to acquaint you that "my Lords have no objection to the proposed under- "taking for establishing an electric telegraph." I must first state that our application to Sir Robert Peel was to establish a telegraph between the Irish and English coast, and also between Calais and Dover.

3231. Did you make the application to the French Government upon receiving that permission?—We made an application to the French Government, at least Captain Taylor went over forthat purpose.

3232. Did the French Government give you leave to lay the cable?—They did.

3233. Why did not you lay it?—An application was first made to the Minister of Marine, who wrote this reply to Captain Taylor (*handing in an original letter*).

3234. (*Mr. Edwin Clark.*) This letter relates to a project for a telegraph between the Mediterranean sea and London?—Yes; not only between Calais and Dover, but a continuation from Marseilles to Algeria.

3235. (*Mr. Varley.*) At what date was that application made to the French Government?—The application was dated the 21st of January 1846. The letter I have produced in reply is dated the 29th of January 1846. It was not, however, till the beginning of April that Captain Taylor had any communication from the Minister of the Interior.

3236. (*Chairman.*) Did the Minister of the Interior accede to your request?—There is a subsequent letter on the 4th of April, and one on the 9th, from the Minister of the Interior, wherein he accedes to our proposition under certain conditions.

3237. Why did not you proceed to lay the cable after having received permission to do so?—In the meantime, during the delay in consequence of the negotiations with the French Government, the parties interested were compelled to make other arrangements, and therefore they did not carry this project on.

3238. (*Mr. Saward.*) This letter refers to an application to the Minister of the Interior?—Yes; that is the letter from the Minister of the Interior. Here is also a letter from Admiral De la Susse to Count D'Orsay, dated the 9th of April 1846, in which he incloses from the Minister of the Interior permission on these three conditions:—"That you "will justify having the authority of the Admiralty "Board; that you will justify having the funds "necessary to carry out this affair, and that your wire "will stop at Calais, and not traverse France." Those were the conditions upon which the permission was granted to us.

3239. (*Chairman.*) You did not proceed to lay the telegraph, because you had not the necessary funds?—No; at that time there would have been no difficulty whatever about the funds. Subsequently, a portion of the cable was tried, as I have before stated, across Portsmouth harbour, in the presence of Mr. Hay, the referee of the chemical department there.

3240. In what year was this?—1846. After this an application was made to the Electric Telegraph Company, which application was entertained, and an agreement as to the amount that was to be paid, and everything else was settled; but at the eleventh hour, when the Electric Company went into the matter, they found that they had not the power to carry their wires over the South-Eastern line to Dover, consequently there was a delay. They intended to carry it out at some future period, either underground or with the power that they would have from the Brighton line; but while this delay was going on, Messrs. Brett went over to Paris, and instead of asking, as Captain Taylor and myself did, for mere permission, they asked for an exclusive concession, and I was informed that they had it. Of course, when we were prepared to lay ours down, we found that we were debarred from doing so, in consequence of this monopoly having been granted to Messrs. Brett.

3241. (*Professor Wheatstone.*) Your communications with the Electric Telegraph Company took place two years afterwards, did they not?—The communications with the Electric Telegraph Company took place in 1847.

3242. October 1847?—That was the wind-up; the communications took place at the latter end of 1846; but the negotiation altogether did not close till 1847, ending in the agreement that was made; but Messrs. Brett obtained the concession from the French Government in December 1847.

3243. (*Mr. Saward.*) Were you not, with Captain Taylor, in communication, in 1846, with Mr. Brett as to the use of his patent for this purpose?—In the year 1846 I was in communication with Mr. Brett.

3244. Were you not negotiating with Mr. Brett for the purchase or use of some patents which he possessed?—I was negotiating with Mr. Brett at that period for the use of an instrument that never was brought to anything like perfection; it was Mr. Jacob Brett's Roman Type Printing Telegraph.

3245. (*Chairman.*) This letter of the 4th of April

1846, from the Minister of the Interior, grants no permission to lay a telegraph at all?—No.

3246. (*Mr. Varley.*) I understand you to state that the present concession of the French Government was obtained by Messrs. Brett upon a statement that they were the originators of the idea of submarine telegraphy?—Yes. When I was over in Paris I had a letter from the Minister of the Interior. In my interview on the 5th of December, 1858, with M. le Directeur des Lignes Telegraphiques, he stated to me that he thought that my application could not be entertained; that it was out of the power of the French Government to do so, seeing that they had nearly completed the negotiation with Mr. Brett, and, at all events, that his present concession would not expire till 1862; that the Messrs. Brett had made an application for a renewal, of their exclusive privilege for thirty years, which application was under the consideration of the Conseil d'Etat; that they had also applied for concessions from Boulogne to Folkestone, Havre to Southampton, and from some point to the Channel Islands, and that they had undertaken to lay down new lines of their own from Folkestone to London, and from Southampton to London; and that they had claimed the right of preference to lay down any other submarine lines to any other parts of the coast, upon the plea that they were the original projectors of the submarine telegraph. This interview took place on the 5th of December, and on the 11th of the same month, after my return from Paris, I wrote the following letter to the Minister of the Interior, in the hope that after the favourable consideration of my proposal by His Majesty the Emperor I should not be debarred from carrying it out by the renewal of Messrs. Brett's monopoly.

15, Mornington Place, Camberwell New Road,
London, Dec. 11, 1858.

His Excellency the Minister of the Interior.

SIR,

IN my interview on the 6th instant with M. le Directeur des Lignes Telegraphiques on the subject of my application to His Majesty the Emperor, to establish a telegraphic communication between Brighton and Dieppe, and Havre and Southampton, and which was forwarded to your Excellency's department with recommendation, I stated that, should the permission I ask be accorded to me, there would be a considerable reduction in the tariff, but I do not think I mentioned the particulars; I beg, therefore, to add, that the proposed reduction will be *no less than* 33 *per cent.* upon the present charge for all messages sent to and from every part of England. For instance, the general tariff now for all messages throughout the United Kingdom to and from France is 7f. 50c.; this would be reduced to a general charge of 5 francs.

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I was informed by M. le Directeur that Messrs. Brett had applied for further exclusive privileges from the French Government, upon the plea that they are the originators of submarine telegraphy. I beg to deny the correctness of that plea, and to state most positively and unequivocally that years before Mr. Brett had even thought of the subject I had brought it to practical development.

As it cannot for one moment be supposed that the Imperial Government will tax the French and English public, and retard the progress of international communication, merely to benefit Messrs. Brett and their friends, I trust that my application will receive equal consideration from your Excellency, as it has already received from His Majesty the Emperor, especially when I offer increased facilities of telegraphic intercommunication at a reduction of one-third of the cost to the mercantile and general public of the two nations, and at the same time secure an extra source of revenue to the French Government from the telegraphic department, in consequence of the additional correspondence which these increased facilities and reduced cost will give.

Of course I have no wish to interfere with existing privileges. All I request is, that I may have the permission I seek, as soon as the concession to Messrs. Brett expires, when I shall be, as indeed I am now, fully prepared to carry out the project which has received His Majesty the Emperor's approval, and which, I trust, may meet with the same consideration from your Excellency and the Conseil D'Etat.

I have the honour to be,
Your Excellency's obedient servant,
CHARLES WEST.

Wednesday, 1st February 1860.

PRESENT:

Captain DOUGLAS GALTON.
The Right Hon. J. STUART WORTLEY.
Mr. G. P. BIDDER.
Mr. EDWIN CLARK.
Mr. FAIRBAIRN.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

Admiral Sir JAMES CLARK ROSS,
Admiral HORATIO AUSTIN, C.B.,
ALLEN YOUNG, Esq.,
} examined.

Admiral Sir J. C. Ross, Admiral H. Austin, C.B., A. Young, Esq.
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3247. (*Chairman to Sir James Ross.*) You have had considerable experience in polar navigation?—Yes, I have passed 10 winters and 20 summers in the polar seas.

3248. We wish to obtain your opinion upon the practicability of laying a submarine cable from England to America, either by Iceland or straight to Greenland, and thence to Labrador, instead of laying a submarine line direct across from Great Britain to Newfoundland?—From what part of England do you intend to start?

3249. The north coast of Ireland we have thought probably would be the best point to start from; but a further suggestion has been made, that the line should start from the Shetland Isles, thence to the Faroe Islands, and thence to Iceland, or direct from the Shetland Islands to Labrador, leaving the point of departure from England entirely open?—There would be difficulty in laying the cable to the Faroe Islands on account of the strong tides and whirlpools, and the abrasion of the cable against the rocks would necessarily be very great; so that at any rate, I think, it would be better to avoid the Faroe Islands. As to Iceland, the same objections apply in a less degree, but with the additional one of the ice coming down along the east and west coasts, which would be very likely to interfere with the cable.

3250. Even on the south coast of Iceland would it be impossible to find a place to land a cable?—On the south coast of Iceland there are very strong tides as there are on the Faroe Islands, though not so dangerous.

3251. Do the tides set round the island?—They come from the north along both shores, and at the south they form into eddies.

3252. Is the ice cast on shore at the south side by those eddies?—It is not cast on shore; the greater portion of the ice is upon the east and west sides of

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Iceland; the pressure is not great upon the south side.

3253. (*Mr. Fairbairn.*) Would not the pressure of the ice be great on the north?—Yes. There would be no possibility of landing a cable on the north shore; it is scarcely ever free from ice.

3254. (*Chairman.*) If the icebergs did not ground to the south of Iceland, do you think the ice would be liable to injure the cable?—The cable would be liable to be injured by the ice drifting along the coasts.

3255. By floating, grounding, or would the mere shore ice be likely to injure it?—There is no fixed shore ice there, as there is on the Labrador coast; but the ice in drifting past might destroy the cable if very heavy. It is generally deeper ice there than anywhere else, but there are no icebergs to interfere with the cable; the icebergs are all on the west side of Greenland. On the east side of Greenland there is none formed.

3256. Therefore Greenland is not exposed to ice?—The east coast of Greenland is not exposed to icebergs, but is inaccessible on account of the floe ice.

3257. Then the ice you have mentioned is floe ice or flat ice?—Yes.

3258. To what depth?—Generally from 15 to 20 feet; it may be sometimes as much as 30 feet.

3259. (*Mr. Stuart Wortley.*) Does the ice actually form upon the shore?—Upon the south coast of Iceland it does not, but it comes down upon the north and west coasts, and becomes fixed there in the winter season.

3260. (*Chairman.*) If I understand, you would not recommend a cable being laid to Iceland?—I should certainly not recommend a cable being laid to Iceland. I should prefer laying a cable from some part of Scotland to Rockall in about 13 or 14 longitude, and then from Rockall I would go direct to Cape Farewell in Greenland.

3261. (*Mr. Stuart Wortley.*) Do you know what that distance would be?—I do not know exactly, about 900 miles.

3262. (*Mr. Fairbairn.*) Would not there be some danger at Cape Farewell, of the ice floating down there, round the east coast?—There would be danger, but as you cannot land on the east coast, you must go round the cape and land it on the west, or rather, south-west, coast, where the ice does not come in with so much force. There are several fiords, between Cape Farewell and Cape Desolation, some one of which might be suitable.

3263. (*Chairman.*) How far is Rockall from Scotland?—I think about 300.

3264. You would go round Cape Farewell, if I understand you, into some of these little inlets?—Yes, Julianeshaab or Frederieshaab.

3265. Between Cape Farewell and Cape Desolation?—Yes, the Danes have a colony at Julianeshaab.

3266. Do you think that it would be necessary to carry the cable inside the Straits of Belleisle?—I should think it would be better to take it higher up the Labrador coast.

3267. Have you no fear of the icebergs or floating ice cutting it?—Yes, there is the great danger; the most difficult part would be laying the cable to the coast of Labrador, I think.

3268. The suggestion about the straits of Belleisle was assuming that the icebergs did not ground off the mouth of the river St. Lawrence?—I am not aware how that is. I understood that the intention was to have gone much further north, but I think there is a great objection to that. Parallel with the coast of Labrador there is a bank of rock and sand, in latitude 57½° N., extending 30 or 40 miles. I have crossed it on two occasions; the depth is only 60 fathoms. A long line of icebergs was aground upon that bank, which it would be necessary to avoid. How far it extends to the southward I cannot tell.

3269. Does that bank extend as high up as Cape Chudleigh?—I do not know. I crossed it south of Cape Mugford, near Okak, the Moravian settlement.

3270. (*Mr. Stuart Wortley.*) In which direction does that bank extend from Cape Mugford?—It extends both ways; it is parallel with the coast both north and south. The line of icebergs marks the shoal, because they always ground upon it; you can tell the position of the shoal by the grounded icebergs.

3271. (*Chairman.*) It has been stated that there is a bank running from near the coast of Greenland across the mouth of Davis's Straits towards the coast of Labrador, which prevents icebergs of more than a certain size passing to the southward; is that correct, according to your experience?—I am not aware of any such bank. I do not believe that there is any such bank in existence.

(*Admiral Austin.*) I think if you had a chart showing the soundings, it would point out all that portion of the shoal water where icebergs would ground.

(*Admiral Fitzroy.*) According to the latest Admiralty charts, that part has never been sounded; there are no soundings published at present that are not laid down in one or other of those two charts.

(*Sir J. Ross.*) Captain Washington has a chart with a number of soundings, that I gave him, which are not on either of those charts. I had them put on to a chart myself in manuscript.

3272. (*Chairman.*) Do you recollect generally what those soundings were?—No.

3273. Did they exceed 1,500 fathoms?—We were not in the habit at that time of sounding such great depths; the greatest depth was 1,000 fathoms.

3274. Was the depth under 1,000 fathoms?—It was above 1,000 fathoms; at 1,000 fathoms we did not strike the ground.

3275. Upon the whole, your opinion seems to be rather adverse to a line carried by the coast of Greenland?—I think it would be a very difficult route.

3276. And be very liable to injury?—Very liable to injury. I can scarcely give an opinion upon it, because there might be deep water from Cape Farewell to some part of the Labradore coast, with which I am unacquainted, where a cable might be landed; certainly not to the northward of Cape Mugford.

3277. (*Mr. Stuart Wortley.*) What is the farthest point northward where you think there would be any chance of landing a cable?—I am not able to judge; as far as I saw, the icebergs grounded north and south.

3278. You actually saw 13 or 14 miles of ice?—Certainly, 20 miles each way. Further to the south that bank may cease, and the cable might be laid in deep water, so as to prevent the icebergs touching it.

3279. Do you think it possible to judge of the practicability of laying a cable across these seas without a more accurate survey of the bottom?—I think it would be very unwise to attempt it. The first step should be to get a line of soundings.

3280. (*Mr. Fairbairn.*) Have we any record of soundings from Scotland to Cape Farewell, so as to ascertain the possibility of laying a cable in that direction?—No, I do not think we have. I do not know of any such line of soundings. It is certainly 1,000 fathoms deep in some parts and it may be 2,000 for what we know.

(*Admiral Austin.*) I should think under any circumstances where you are going to lay a cable in the locality of ice it would be most important to have soundings taken. Wherever there are icebergs the most certain mode of avoiding them would be to lay the cable in the deepest water.

3281. (*Mr. Fairbairn.*) Would it not be very unsafe and uncertain to lay a cable in any case unless you had soundings?

(*Admiral Austin.*) Yes, particularly where there was ice.

3282. (*Mr. Stuart Wortley to Sir J. Ross.*) Is there a current known by the name of the Spitzbergen current?—Yes, it is a reflection of the Gulf Stream which comes round to the east and north of Spitzbergen and then passes to the southward between Iceland and Greenland.

3283. Does it run down the east coast of Greenland to the extreme southward of Cape Farewell?—Yes, and after passing the Cape, it runs up into Davis's Straits, as far as Queen Anne's Cape, where it meets the Baffin's Bay current, and is turned by it to the south, along the coast of Labrador.

3284. Is not there a quantity of drift wood brought down from Arctic Asia?—Yes; the drift wood is landed on the coast of Spitzbergen in tiers five or six feet high; along the whole line of coast the piles upon the beach would be ten or twelve feet in breadth.

3285. Is any drift wood cast upon the coast of Greenland?—I do not know; we always met drift wood when we got into this stream; we saw trees floating about, but I do not think it is cast upon the shore because the current turns round Cape Farewell and runs north again; but it does not go so close in shore as to land much drift wood.

3286. It does not go round the coast upon the west side?—No; as soon as it reaches Queen Anne's Cape, the current brings it down upon the coast of Labrador.

3287. Does not the drift wood occasionally become the nucleus of large ice-fields; does not it get frozen together?—No, I have never seen any in that condition.

3288. (*Chairman.*) Have you visited the coast of Labrador sufficiently to be able to say whether there are indentations which would be favourable to the landing of a cable?—No, I have only landed at one place in Labrador, that is Okak, the Missionary establishment to the south of Cape Mugford.

3289. There appeared to be fiords or deep indentations?—Yes, but you could not avoid the bank. It would not do to attempt to land a cable there.

3290. (*Chairman.*) The fiords are not favourable places for the landing of a cable?—(*Admiral Austin.*) Not where the ice is formed.

3291. (*To Sir J. Ross.*) From what do the icebergs form?—The icebergs form from the glaciers; in the northern part of Baffin's Bay there is a vast extent of coast, where the whole country is covered with ice, and there is not a particle of land to be seen amongst it. These vast glaciers gradually slide down and break off, in pieces of various shapes and sizes, and form the icebergs.

3292. (*Mr. Stuart Wortley.*) What is termed pack-ice?—That which is formed on the sea; the other is snow-ice.

3293. Is there also another description called floe-ice?—Yes.

3294. Does that form in the fiords?—Yes.

3295. Does it reach the shore in the fiords?—Yes, in Greenland and the northern parts of Labrador.

3296. Are not the icebergs, generally speaking, floating in deep water?—Yes, they must be.

3297. The floating icebergs are in deep water where a cable might be safely laid, nevertheless?—Yes; if you can have deep water to the shore, say 200 or 300 fathoms, you may be quite sure that the cable will not be disturbed by icebergs.

3298. Would not there be a risk of the cable being disturbed where it came to the shore?—That must be guarded against. If you can get it to the shore, it must be protected in some way, so that the floe-ice cannot get to it.

3299. (*Chairman.*) At what depth do you think a cable would be safe from icebergs? I suppose there is some limit to the depth?—By the time icebergs get so far south, they are seldom more than 150 feet above and 150 fathoms below the surface of the sea.

3300. (*Mr. Stuart Wortley.*) As soon as the iceberg floats, the cable is safe, I presume?—Yes, if it is at the bottom.

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(*Admiral Austin.*) If you have shallower water to the northward, it is reasonable to suppose that the cable would be safe, the ice drifting south.

3301. (*Admiral Fitzroy to Sir J. Ross.*) Has it occurred to you that all these fiords are comparatively deep in the middle; that at each point of the fiord there is a projection of rock or land, and that no iceberg, in passing by it afloat, can touch the bottom of the fiord, because it must go over the shoal which booms it off, as it were, to a distance; it would pass from point to point, in drifting down, and must leave the hollow bottom of the fiord untouched?—Yes, of course.

3302. Another matter has, I dare say, occurred to you, that while an iceberg is afloat, it does not carry up anything from the bottom with it?—No, it ploughs deep grooves in the bottom as it passes along.

3303. Not, I think, as long as it is floating, and not touching: the moment it moves it would separate from the bottom. If a cable is lying on the bottom the iceberg is but pressing it down for a time, the moment that iceberg floats it rises, and then it drops the cable; there is nothing of an adhesive nature in ice. When once afloat it passes on without moving the cable. If then the fiords have spurs on each side, and a drift of ice passes along the shore from spur to spur, being boomed off, as we should say, by these projections, there could be no injury to anything lying at the bottom. These fiords almost invariably are filled with silt or detritus of some kind, and have a safe way out to sea. You are probably aware that Americans have lately been examining these fiords at the south part of Greenland with a special object, and that the report made by the officer in charge of the vessel is that he has found no difficulty whatever in taking a cable or a telegraphic wire up the middle of these fiords. He does not allude to the question of ice floating or grounding, but he simply states the fact that he finds it easy to take a line up the middle of these fiords. I would ask you, having seen these places, whether the idea that the bottom of the fiords would be safe as a place of deposit for a cable is inconsistent with your practical knowledge of them?—I do not think that any cable would be safe where there was a liability for the icebergs to ground; the bottom of the iceberg is not always smooth; sometimes there are large hooks or prongs at the bottom, which would catch hold of anything; they go scraping along the ground, and would carry the cable away.

(*Admiral Austin.*) They are also subject to turn from the effect of the tide or weather making them lighter.

(*Sir J. Ross.*) It would not be safe to lay a cable where icebergs have a chance of touching it in any way.

(*Admiral Fitzroy.*) My belief is that no iceberg could touch the middle of the fiords, because it must first pass the spurs; if it had not draft enough to touch the projections near the fiords, it seems to me quite clear that in going from point to point it must pass over the deeper interval between without touching.

3304. (*Mr. Stuart Wortley to Sir J. Ross.*) Do not the fiords get gradually shallower as they go inland?—Generally they do.

3305. At the mouth of the fiord, I think I understand you to say, there would be some considerable danger from the floe-ice?—That must be provided against in some way, certainly.

3306. Does not that ice break up every spring?—Yes.

3307. Would not it be likely to carry away anything frozen in it?—Yes; if you landed the wire at the head of a fiord, for instance, it would be necessary to cut off the ice from wire in the spring, before it broke away.

3308. Would not the ice come again the next winter?—Yes, and you must do the same again; you must cut it every spring.

3309. (*Chairman.*) Does not ice form from the

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streams going out from the shore?—Yes; and it covers the surface of the whole fiord.

3310. (*Mr. Stuart Wortley.*) I have an article in the "Cornhill Magazine," entitled "The Search for Sir John Franklin," written by some officer connected with the expedition, in which he states:—"We left "Aberdeen on July 1, 1857, and after a favourable "run across the Atlantic, we made our first acquaint- "ance with the Arctic seas, when near the meridian "of Cape Farewell, by falling in with the drift-wood "annually brought from Arctic Asia by the great "current known as the Spitzbergen current; the "shattered and mangled state of these pine logs "bearing evidence of their long water-and-ice-borne "drift. This great Arctic current brings masses of "ice from the Spitzbergen seas at seasons, completely "filling up the fiords, harbours, and indentations on "the south coast of Greenland, and often in a pack "extending for 100 miles southward of Cape Fare- "well." Is that consistent with your experience?—Not quite. I have never known the pack to fill up the harbours north-west of Cape Farewell. If a cable were to be landed there it would be free from the liability to great pressure, at any rate; there would be only drift ice, and that drift ice would not enter into the fiord or harbour which you would select for your cable.

3311. The point I am drawing your attention to is that the ice fills up the fiords on the south coast of Greenland, and does not at the west?—There is no south coast strictly speaking; you probably include the coast between Cape Farewell and Cape Desolation, more properly a south-west coast. Perhaps Mr. Young will state where he got his information.

(*Mr. Young.*) My information is obtained from yours. All we know of the east coast is from Captain Graah, and principally from Sir James Ross's notes.

(*Sir J. Ross.*) The east coast is out of the question entirely.

3312. (*Mr. Stuart Wortley.*) What do you call the south-west?

(*Mr. Young.*) Round about Cape Farewell.

3313. (*Chairman.*) Would you include the coast from Cape Farewell to Cape Desolation?—Not so far to the west as that; as far as Julliansh aab.

3314. (*Mr. Stuart Wortley.*) Was your information derived from others?—From information from the natives.

3315. Upon which you have every reason to rely?—Yes, and from seeing the ice there ourselves.

3316. In the fiords?—No, we only went through the off-lying pack. I believe, after September, there is no ice whatever on that coast.

3317. It forms in the winter, and breaks away in the spring?—No, it was foreign ice completely. It comes down from Spitzbergen, where it breaks up in January, and comes round the eastern coast.

3318. (*Sir J. Ross.*) Did you land at Julianeshaab?—At Fredericshaab.

3319. What time did you land?—About the middle of July.

3320. It would be clear enough then?—We had difficulty in getting inside it.

3321. (*Chairman.*) Would not Fredericshaab be blocked with ice coming from the north?—No, from the south.

3322. (*Adm. Austin.*) It is merely temporary, I suppose?—It is always so during the summer. It begins, I apprehend, off Cape Farewell, at the end of January or the beginning of February, and from that time to the end of August it is contiuually streaming round that coast.

3323. (*Chairman.*) Does the ice always block up Julliansh aab?—Yes.*

3324. (*Admiral Austin.*) Are the icebergs off the land?—No, not icebergs, but heavy ice, sometimes as thick as 40 or 50 feet.

3325. (*Admiral Fitzroy.*) Do you suppose that the ice which floats round the coast grounds in the depths or blocks up the depths of the fiords, or that it hangs in the trough of the fiords from point to point in masses without touching the depth between. If it moves along the coast, it appears to me clear that it cannot touch ground in the water, which is deeper than that which must stop it and prevent it from going on. I allude to the projecting points. If the ice passes the projecting points clear it cannot have depth enough to touch the hollows between?—I think you are supposing that those points have spurs off them.

3326. That such points have ridges running out under water I have found in most similar cases where there are projections. I am told by persons who have been there that those fiords are very much like the fiords on the coast of Norway, and the fiords on the west coast of America. Tierra-del-Fuego and other places of similar geological structure may be instanced where there is very deep water in the middle with steep shores and projecting points running out to some distance?—(*Mr. Young.*) My opinion is that the ice does not ground at all on that coast. I have seen it lying up against the precipitous rocks.

3327. (*Admiral Fitzroy.*) But not touching the bottom at all?—(*Sir J. Ross.*) This is a very bold coast; we sailed past Cape Farewell on one occasion at about 25 miles distant, and we could see no ice, and on returning another year, at the end of October, we came close in by Cape Desolation, but we could see no ice.

3328. (*Mr. Stuart Wortley.*) If the icebergs or floe ice, as Admiral Fitzroy supposes, passed without grounding through the chops of the fiord, then a south wind or a south-west wind would necessarily blow it into the fiord till it did ground?—(*Mr. Young.*) Yes, it would go right against the rocks; *i.e.*, would lie *afloat* against the precipitous rocks.

3329. Supposing it entered the mouth of the fiord, then it would proceed till it grounded?—Yes, at the head of the fiord.

3330. And would injure anything at the bottom?—No, I do not think it would have sufficient way on it; it might plough the bottom up a little; but the bottom would be composed of mud and detritus, into which the cable would sink deeply.

(*Sir J. Ross.*) We beat up along this coast several days against a northerly wind, close in with the land on purpose to take advantage of the current; we generally stood in to within two or three miles of the shore.

3331. (*Mr. Young.*) You did not see the pack?—No, we saw no pack.

(*Sir J. Ross.*) We stood off 10 or 15 miles.

(*Admiral Austin.*) In my experience the ice has not been hung upon this shore, but that there has been open water between it and the pack.

3332. (*Chairman to Sir J. Ross.*) Do you think that these spurs, which Admiral Fitzroy mentions as stretching out from the coast on each side of the fiord into the sea, would not become by the time they get some little distance from the land, if the current set past them continually, abraded so as to form a uniform depth at a very little distance out?—This coast is so very perpendicular that we never could get soundings when we were within two or three miles of land, less than 200 or 300 fathoms, so that there are probably no such things as these spurs along this coast.

3333. If there are any spurs they would be solid rock?—Yes.

3334. And go precipitately down very soon after they threw off teh ice?—Yes,

(*Admiral Fitzroy.*) The geological character of that coast is said to be that there are intervals between one ridge and another, as there are valleys between heights on the land; the ridge projects to a certain distance under water; that ridge is solid rock, generally primitive rock, and water washing past it

* "It frequently happens that vessels bound to Julliansh aab from Copenhagen are obliged first to put into some harbour more to the northward, and wait until the ice is so much dispersed round the South Coast that they can continue their voyage to Julliansh aab."—*Irminger on the Arctic Current.*

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either way crumbles it away by degrees very slowly; it is not at all like a projecting bank of sand, shingle, or mud. For instance, on one of the most exposed coasts in the world, Tierra-del-Fuego, many such ridges have their detached rocks or pinnacles projecting to some distance off, so much so that it is a maxim with the sailors accustomed to those seas that you must give every point a wide berth, because you never know what rocks there may be projecting from it under water, though when the point is passed you have deep water.

3335. (*Mr. Fairbairn.*) Assuming it to be the case that these projecting points come out under the water if an iceberg floating along the coast came in contact with them the result would be that it could not pass, it would float into the deeper water, it must either go round the point or else be carried clear out into the stream.

(*Admiral Fitzroy.*) If the wind were very strong it would prevent ice from passing the point in the first instance, because pressed hard against it. If ice passed the point it must be by wind, a current taking it past; that current would continue along the coast from point to point. If the iceberg is drifted by the wind, it will go on with the wind along the coast.

3336. (*Mr. Stuart Wortley*) You mean that the iceberg will be carried across the mouth of the fiord?—(*Admiral Fitzroy.*) Yes; the current always goes from one headland to another, supposing the water to be uniformly deep. The general characteristic of those fiords, which are only valleys under water, is that you can get no anchorage, or even no sounding in the middle, and that you must go very close to the land on either side, or very far up to get hold of the ground at all.

(*Mr. Fairbairn.*) It is quite clear that the great difficulty in laying a cable will be, not the nature of the coast, but the icebergs or floating ice.

3337. (*Admiral Austin.*) Do not you suppose that we are indebted for the depth in such channels in some measure to the formation of heavy ice and its subsequently tearing up the bottom?—(*Admiral Fitzroy.*) Not at all, in my opinion.

3338. (*Chairman.*) In the northern seas, you mean?—(*Admiral Austin.*) Yes. Under these circumstances the ice clears away the shallow part.

3339. (*Mr. Stuart Wortley to Sir J. Ross.*) Independently of icebergs does not ice form in the fiords themselves?—Yes.

3340. Can you tell us what is the general character of the south-west of Greenland; you say that it is bluff and precipitous; do the rocks extend out into the sea?—There are many small islands and rocks lie off the main coast.

3341. The probability is that it is a rocky bottom?—I think most likely it is; I cannot speak of it with certainty.

3342. In the 12 miles of Atlantic cable which have lately been raised in Newfoundland, it has been found to be laid on a rocky bottom; consequently it has been injured throughout, but more in places where there was iron in the composition of the rocks, or copper in the composition of the rocks. Speaking generally, where it was laid in mud or sand it came to the surface quite perfect, from a depth of 150 fathoms; and wherever it laid upon rock it was injured, and rendered utterly useless. Taking that into account, do you think there is any part of the south west of Greenland where you can avoid a rocky bottom?—I have no means of judging.

(*Mr. Young.*) There is the Tallert Bank, 28 miles to the north of Frederickshaab.

3343. (*Mr. Stuart Wortley.*) Is that bank in the open sea or in a fiord?

(*Mr. Young.*) Right in the open sea, facing the glacier.

(*Sir J. Ross.*) There is a bank,—I do not mean the Victoria Bank,—where the "Conqueror," 74, struck during the American war. It is just to the north of Holsteinburg, and extends a considerable distance off. We had only three or four fathoms on that bank.

3344. (*Chairman to Sir J. Ross.*) There are two things to be guarded against in laying a cable in these seas; one is the ice, and the other is placing it upon a rocky bottom, where it is subject to movement from the tide or current, causing a continual abrasion, which would of itself destroy the cable. For example, the cable which was laid for the Channel Islands was destroyed by the current moving it backwards and forwards upon a rocky bottom?—No doubt; where there is a strong current it is always a difficulty.

3345. Are not the points of the fiords subject to strong currents?—Yes, most of them are.

3346. (*Mr. Stuart Wortley.*) Have you any means of knowing the depth to which the Spitzbergen current reaches?—I have not the least notion.

3347. As it appears to be a current of enormous force, does not it probably run very deep?—It is not a current of very great force; perhaps two miles an hour. I should not think it was any very great depth, but that is quite a matter of speculation.

3348. It is generally supposed that if a cable is sunk as deep as 150 fathoms it finds repose and tranquillity?—Yes, it is free from the action of tide and waves.

3349. (*Admiral Fitzroy.*) May I ask whether the bottom of an iceberg, in your opinion, is of the same character, with respect to hardness, as the part exposed to the atmosphere, which we know is capable of scratching rocks?—That depends very much upon the character of the berg. Some bergs are perfectly flat, which we call table-land bergs. Those are very even at the bottom, and they generally keep nearly the same level, but others are awkward-shaped masses, that keep turning over and over again every day; therefore the character of the ice in these must be the same in every part.

3350. At any given moment, taking an iceberg of any shape,—is the part that is immersed deepest below the water in the same condition as the part above the water?—It depends upon whether the water or the air is the warmer of the two, that is whether the decomposition is taking place from above or below.

3351. Is not the water in those latitudes at the depth of an iceberg always above the freezing point?—No, not always; it is seldom above the freezing point.

3352. Has it ever been found below the freezing point by any authentic experiments?—Below the freezing point of fresh water.

3353. Below the freezing point of salt water?—No, never below that, or of course it would not remain water, the temperature of the surface of the sea is 28° pretty nearly, when the thermometer in the sun may be 50° or 60°.

3354. Is it 28° at the surface or below?—At the surface.

3355. What has it been below?—There has been no great difference to the depth of 50 or 60 fathoms.

3356. So far as the results of sounding hitherto have gone is anything recorded showing that the temperature of the sea at 10 or 20 fathoms is lower than 30°?—Yes, we have frequently had it at 100 fathoms down to 29°.

3357. (*Chairman.*) Where?—In Baffin's bay.

3358. (*Mr. Stuart Wortley.*) What is the freezing point of salt water?—28°.

3359. (*Mr. Fairbairn.*) We want a good series of experiments upon this subject, we have no accurate experiments to prove at what depths you have different temperatures?—(*Admiral Austin.*) It has been a puzzling point even to those who have been engaged in it.

3360. (*Chairman to Sir James Ross.*) Have not you made several experiments with regard to the temperature of the sea?—Yes, I have made them in the southern regions but not in the northern.

3361. What were the general results of your ex-

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periments in the southern regions?—They were symmetrical and followed a clearly established law.

3362. Will you be good enough to give the law that you found established?—During the many thousand years that the earth has revolved on its present axis, the ocean has been absorbing heat from the sun, especially at the equator, where its rays are most powerful. There the temperature of the surface of the sea at the present time is about 80°; this temperature diminishes very rapidly in descending below the surface for the first few hundred fathoms, and then very slowly to a great depth.

Thus the surface temperature is			80·0
At	150 fathoms below the surface,		54·0
At	300	„ „	46·0
At	450	„ „	44·0
At	600	„ „	42·8
At	750	„ „	41·4
At	900	„ „	40·8
At	1,200	„ „	40·0
At	1,500	„ „	39·5
At	1,800	„ „	39·5
At	1,950	„ „	39·5
At	2,100	„ „	39·5

This temperature of 39°·5, which I believe continues to the greatest depth, I call the normal temperature of the ocean. In proceeding southward from the equator the surface temperature diminishes at first very slowly to the 50th degree of south latitude, and then very rapidly to the 56th degree, where the normal temperature of 39·5 is found at the surface. In this parallel of latitude, therefore, the ocean has never absorbed heat from the sun, its rays have been deflected; and beyond this latitude the heat has been continually radiating into space from the sea. It is further evident that about this parallel of latitude there is a belt or circle round the earth, where the normal temperature of the sea obtains throughout its whole depth, forming a boundary or kind of neutral ground between the two thermic basins of the ocean. This circle was crossed by the Antarctic expedition on seven different occasions, and on different meridians, and experiments obtained, in which there was not the smallest difference of temperature of the sea from the surface to the depth of 2,000 fathoms. This temperature of 39·5 I believe to be the original temperature of the ocean at the creation of the waters that cover the sea, partially disturbed by the absorption of heat from the equator to the 56th degree of latitude, and by the radiation of heat to the southward of that parallel. We accordingly find that beyond the 56th degree of latitude the surface temperature gradually diminishes to the latitude of 65° S., where it attains the lowest possible temperature of 28°. There the normal temperature is found to be about 800 fathoms below the surface. This circle of normal temperature of the southern ocean is a standard point in nature, which would afford to philosophers of future ages the means of ascertaining if the globe we inhabit shall have undergone any change of temperature, and to what amount during the interval. These observations also force upon us the conclusion that the internal heat of the earth exercises no influence upon the temperature of the ocean, or we should not find any part of it in which it was equable from the surface to the great depths we have reached, a new and important fact in the physics of our globe.

3363. (*Admiral Fitzroy.*) From 28° at the surface the temperature increases as you go down deeper?—Yes, after you pass this point where absorption terminates and radiation begins, till at length you come to a point where the surface temperature comes to 28°; then nature puts on a covering of ice, which is a non-conductor, and the nearer you get to the pole this ice gets thicker and thicker, and prevents any further radiation.

3364. There is a point at which 28° continues to the bottom from the surface?—No; but from 28° you go down to 700 or 800 fathoms before you reach the normal point.

3365. But with 28°?—Yes; then it gets warmer by degrees as you descend.

3366. Are there any observations recorded which show a temperature of 28° as low as 100 fathoms below the surface?—I think you will see them in Parry's voyages.

3367. I have searched for temperatures, at great depths, specially; but I have found none lower than those taken by the Americans in their deep-sounding expeditions, and by Captain Cochrane at various places, as well as under the Gulf Stream. In none of the Arctic voyages have I yet found any records of temperatures at very great depths; they are given, frequently near the surface?—(*Admiral Austin.*) You are aware that there is great difficulty about it.

(*Sir J. Ross.*) The temperatures were never taken in those days at great depths; seldom below 300 fathoms.

3368. (*Mr. Fairbairn.*) If I understand you rightly, it appears that the point of uniform temperature varies as you go north; you have to go to a greater depth to get from 28° to 29°?—From 39° it goes on gradually diminishing till you come to 28° in latitude 66°, then if you go on for 300 miles further, of course the temperature does not diminish any more at the surface, but the temperature of 28° is found much below the surface.

3369. Supposing you have 28° at 100 fathoms in depth, the temperature gradually increases to 39°, and at 1,000 fathoms you get to 39°, then it becomes uniform?—Yes.

3370. Supposing you went 10° further south, have you ever found 28° at a great depth?—So long as you have got the warm water beneath, it compensates the radiation, and keeps it at 28° to a considerable depth; going further south again you get 28° at 40 fathoms then to 60, then to 100 fathoms; the depth of water at 28° increases until the formation of ice on the surface begins, and you go to a great depth before you arrive at the normal temperature.

3371. (*Admiral Austin.*) How did you obtain the temperatures?—By self-registering thermometers.

3372. (*Chairman.*) Did you take the temperature at a depth below 300 fathoms?—Yes, below 2,000 fathoms; the self-registering thermometers were made on purpose for me; those supplied when I sailed broke under the pressure of 700 fathoms; I wrote for others, and they stood the pressure of more than 2,000 fathoms.

3373. (*Mr. Stuart Wortley.*) I think I collect from you as regards the route by Iceland, that there would be great difficulty in landing a cable on the coast of Iceland on account of the currents and whirlpools, but not so much on account of the ice?—There would be also a difficulty from the ice, but that is not so material. It might be overcome.

3374. On the east coast of Greenland you think that the landing of a cable would be impossible from the ice, and even on the south or west coast it would be very hazardous?—It would be attended with difficulty and require some judgment in the selection of a proper place.

3375. (*Mr. Fairbairn.*) The difficulty of landing on the south coast of Greenland would not be so great as on the south coast of Iceland?—No.

3376. (*Mr. Stuart Wortley.*) On the east coast of Labrador you think it would be impossible to land a cable off Cape Mugford?—I think it would not be safe to land a cable anywhere to the north of Nain where there is a missionary settlement.

3377. Are you sufficiently acquainted with the coast of Labrador to the south of that to be able to give any opinion?—No, I know nothing about it.

3378. That may be of the same difficult nature as the rest you have seen?—It may, and therefore points to the necessity of a previous examination.

3379. (*Mr. Fairbairn.*) Do not you think it may be possible to land a cable in a quiet bay or creek?—I think looking at the chart that you might get into Hamilton Inlet from Cape Farewell. I think very

likely that might be the case. You might thus keep the cable in deep water the whole distance.

3380. (*Mr. Stuart Wortley.*) Is not the coast of Labrador a very rocky coast?—Yes, it is a sandy bottom mixed with outlying rocks and islands; all the soundings give sand, but there is a great number of rocks and islands sticking up amongst the sand.

3381. You might perhaps cheat the rocks?—You would have to sound your way very carefully, but I think that the most promising looking place upon the chart from Cape Farewell is to Hamilton Inlet.

3382. Do you know the Faroe Islands?—No, I have passed them, that is all; we were very nearly running against them once, but were swept away by the strong tides.

3383. Is that coast a very rocky coast?—Yes, very rocky.

3384. Are there any points standing out of the sea indicating reefs or anything of that sort?—We could not see anything of the kind, except the whirlpools, which always indicate rocks under water, but we were only there for a short time; the accounts given of them make me fear that they would be very undesirable places to touch at under any circumstances.

3385. Are not they Danish territory?—Yes.

3386. And Greenland also is Danish territory?—Yes.

3387. (*Chairman.*) Have not the Danish Government granted a concession to lay a telegraph from Denmark to the Faroe Islands?—Yes; Sir John M'Neil told me that they had granted it to him. It appears to me that from Rockall to Cape Farewell, and from Cape Farewell to Hamilton Inlet is the best course.

3388. You think that is the most feasible, but you do not seem to have much confidence in laying a cable in that direction?—No, unless it was very desirable, in that case the difficulties might be overcome.

3389. (*Chairman.*) You think the difficulties are at least equal or even more than equal to laying a cable across direct to Newfoundland?—Certainly, much greater.

3390. (*Mr. Stuart Wortley.*) The bottom, between Newfoundland and Ireland, although not perfectly surveyed, has been to a considerable extent surveyed, and the depths accurately laid down by Lieutenant Morse?—I am not aware of it.

3391. There is what is commonly called Lieutenant Morse's plateau?—Those soundings were proved to be inaccurate. Captain Dayman obtained a very accurate line of soundings where the telegraph cable now lays, but would be no guide in the present inquiry.

3392. Apart from telegraphic cables as well as with reference to them, do not you think it is of great importance that the Government should make thorough surveys of the depths of the Atlantic?—I think it is most important, and almost a reproach to England, that the irregularities of the bed of the ocean are not as well known as the mountains and valleys of the continents.

3393. (*Chairman.*) What I understand Mr. Stuart Wortley to mean is, that a further line of soundings should be made from England to the coast of Greenland and Labrador and round the coast before anything was done?—After ascertaining the points on Greenland and Labrador where the cable is to be landed, the line of soundings is of next importance.

3394. (*Mr. Stuart Wortley.*) Have you any knowledge of the South Atlantic; for instance, it has been suggested to take a line from Gibraltar across by St. Michael's and the Azores to the coast of Florida?—I have no knowledge of that.

3395. Beyond general knowledge that it is liable to volcanic disturbance?—I do not think there are any active volcanos just now in that region.

3396. In the Mediterranean, cables have suffered, or are supposed to have suffered, from volcanic action between Sicily and Malta; how is that with regard to Iceland; would there be any danger of volcanic action from Hecla?—I am not able to give an opinion upon that subject.

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3397. There are no natural phenomena that indicate volcanic disturbance there?—Not ordinarily that I am aware of.

3398. No islands jumping up now and then, as they do in the Mediterranean, and disappearing again?—No, no such thing is known there.

3399. (*Mr. Fairbairn.*) There seem to be very great difficulties in taking a cable by Iceland and Greenland?—Very great difficulties.

3400. And the ice appears to be the most formidable of those difficulties?—Yes.

(*Admiral Austin.*) Wherever you have ice drift it would be very unsafe to attempt to lay a cable without thorough sounding; it is very difficult to know what will be the action of ice in deep water when it is passing.

3401. (*Chairman to Admiral Austin.*) You have heard the evidence which has been given by Sir James Ross; do you concur with it?—I concur generally with Sir James Ross. It is a most difficult thing to give answers as to what would be the effect of ice in its movements upon a cable laid near the shore, but at the same time I would observe that if a line of shoal ground could be found by way of Iceland on which the heavy ice and bergs might ground until sufficiently reduced to admit of their drifting freely over the cable in deeper water, the apparent advantage of being able to lay down each cable separately between places of a moderate distance apart, either at the same time or at different periods, gives this a preference over the long route to Newfoundland. However, it is manifest that this can be alone decided by careful lines of soundings.

3402. (*Mr. E. Clark.*) Does an iceberg bring up stones and material from the bottom?—Yes, the stones are more generally from the shore.

3403. (*To Sir James Ross.*) Has it come within your observation that icebergs in reversing have brought up any of the bottom?—Yes; I have seen mud sticking to them.

3404. Any large stones?—I have seen stones and a quantity of mud, particularly when the iceberg has been aground when it turned over.

3405. (*Chairman.*) In Kane's Journal there was a very curious account given of the effect of icebergs in transferring large boulder stones. He described seeing some large boulder stones on a glacier, just going to be carried off?—That must have been on the upper surface, not taken from the bottom.

3406. (*Mr. Stuart Wortley.*) Do not icebergs, if they melt gradually away at the bottom, turn bottom uppermost?—Yes.

(*Admiral Austin.*) And they are very awful in their operation.

3407. (*Mr. Stuart Wortley.*) In what cases have you observed any of the bottom brought up?—In some cases where the icebergs have been lying aground, when they turned over, I have seen mud and stones brought up.

3408. (*Mr. Fairbairn.*) When an iceberg breaks away from the coast, does not it take away with it large boulder-stones, if they are loose at any part of the bottom?—Yes; anything frozen into the detached mass would be carried away with it.

(*Admiral Austin.*) The power of an iceberg in launching itself in the water, upon being forced out by the upper part, which has formed it, is something terrific.

3409. (*Mr. Stuart Wortley.*) What would be the distance from Cape Farewell or Fredericshaab to Hamilton Inlet?—(*Sir James Ross.*) About 500 miles, speaking roughly.

3410. (*Chairman to Mr. Allen Young.*) You have heard the evidence of Sir James Ross—do you concur with it?—I have no acquaintance whatever with the coast of Iceland, or the coast of Labrador.

3411. You have considerable acquaintance with Arctic navigation, I believe, and with the coast of Greenland?—So far as having been two years with

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Captain M'Clintock; that is the only time I have been there. With regard to Fredericshaab, the natives and Danish authorities told us that the ice never set into the harbour to endanger the shipping at all. There is generally a ship lying there during the summer. The harbour looked very exposed, and very open to seaward. I said, "In the event of a gale of wind, shall we lie safely here?" They said, "No ice ever comes into the harbour."

3412. Does it ground outside?—No; the current, as Admiral Fitzroy has mentioned, shunts the ice off the points where it strikes the rock, and it goes away into the current again. A few pieces might come slowly round, but directly they come under the lee of the high rocks, they scrape against the rocks without touching the ground, till they come to the head of the harbour, and there they will lie quiet.

3413. Is the bottom of the harbour at Fredericshaab sand or mud?—Very soft mud.

3414. For what distance does that extend?—The mud extends four or five cables from the head of the fiord.

3415. What is there beyond that?—Rocky bottom.

3416. Is not there a strong current sweeping along the coast?—The current is 14 miles in 24 hours.

3417. (*Mr. Fairbairn.*) Is that the same current that goes round the coast of Greenland?—Yes.

3418. (*Mr. Stuart Wortley.*) In going out you were at the south of Greenland in the month of July?—The end of July.

3419. In coming home you were there again about May?—We came home in August.

3420. It would be about the month of June or July that you were off the south of Greenland in coming home?—In coming home we were there at the beginning of September.

3421. Therefore you had not the opportunity of seeing with your own eyes what the ice was in the winter there?—No, we had not.

3422. Did you understand that ships lay all the winter in the harbour without injury?—The Danish ships do not winter there.

3423. In the winter is not the harbour filled with ice?—No, it is just skimmed over occasionally; the natives use their kyacks all the winter.

3424. Is it your opinion that a cable could be laid safely in that harbour?—Yes; the only risk would be in pushing through that pack into what I call in-shore water.

3425. Does the pack ground at all?—I do not think the pack does ground.

3426. The difficulty would be in laying the cable originally to the ground?—In carrying the ship to the land through this pack.

3427. After the cable was once at the bottom, you think it would lie safely?—Yes, as regards the ice; the ice is not so heavy.

3428. Was the pack lying there when you entered?—Yes; we pushed through it.

3429. Does it lie there all the summer?—Yes, till September; then it is all dissolved, and there is no ice till the following January.

3430. Is it blown northward?—It is supposed to have dissolved. The Spitzbergen ice is carried by the current to the northward, along the south-west coast of Greenland, into latitude ranging from 64° to the Arctic Circle, and the natives say that it there *sinks*. The natives are led to think that the ice *sinks*, from its sudden disappearance from the coast; the probability being that it is there taken up by a westerly current, aided by the first off-shore wind, and is borne rapidly away to join the great ice-stream from Baffin's Bay and along the shores of Labrador.

3431. When you were there in July, was the pack lying off the coast of Greenland?—It was.

3432. At what time of the year does it disappear?—It disappears in September.

3433. As winter approaches?—Yes, because it is foreign ice, and in winter ceases to break away from the Polar regions, it does not belong to this coast; this ice all comes from the Spitzbergen seas.

3434. You say that it is melted in September; when does it form again?—It breaks away in January, and comes down the east coast.

3435. It is all foreign ice?—Yes.

3436. (*Admiral Austin.*) Does not ice form at all during the winter?—It might form in small pieces, but not to that extent.

(*Sir J. Ross.*) Only over the surface of the harbours.

3437. (*Admiral Austin.*) Are not the whole of the straits frozen over?—(*Sir J. Ross.*) No.

(*Mr. Young.*) There is open shore as high as Holsteinberg all the winter.

3438. (*Chairman.*) Do you concur generally with Sir James Ross's evidence, from your experience?—I am not acquainted with the coast of Labrador or Iceland.

3439. (*Mr. Stuart Wortley.*) The result of your opinion is, that you do not consider it impossible to land a cable on the south coast of Greenland?—I do not consider that there would be any difficulty after September, excepting from the violent winds.

3440. Do you think that there would be no risk to the cable during the time the ice is there?—I do not think so, if, as Admiral Fitzroy proposes, it was carried up to the head of the fiord.

3441. (*Admiral Austin.*) I do not suppose that you would be able to operate there after October or November, on account of the temperature?—I do not think the temperature alone would be the obstacle.

3442. (*Mr. Stuart Wortley.*) The Atlantic Company were advised against commencing any operations after the month of August?—I should propose to start from the coast of Greenland, to take advantage of any westerly winds.

3443. (*Chairman.*) Coming towards Great Britain?—Going towards Iceland.

3444. Between Iceland and Scotland, would not that season of the year be rather unfavourable for operations such as laying a cable?—There would be heavy westerly gales, no doubt, in the autumn.

3445. (*Mr. Stuart Wortley.*) What season of the year would be practicable for laying a cable between Labrador and Greenland?—The same season, after September.

3446. I suppose all these regions are liable to heavy weather?—Yes; the violent westerly gales are all in your favour if you are sailing to the eastward.

3447. (*Mr. Fairbairn.*) The month of September would be the best for laying a cable, on account of the ice?—Yes.

3448. (*Mr. Bidder.*) That drives you into the bad weather, and it may be so heavy that the vessel may have to heave to?—I think if a vessel was lying in one of the fiords, with a watch upon the hill, she could go out at any season of the year, by taking advantage of an opportunity.

3449. (*Admiral Fitzroy.*) If a ship can go out at any season of the year, what would prevent her from veering away a wire through a proper opening, either astern or in her bottom, she herself making a passage through the ice as she forces through it. Do you see any difficulty in her veering out the wire as she goes through the ice?—No, the only danger is being beset in the pack.

3450. (*Chairman.*) Would there be no danger of the ice closing behind the ship, and cutting the cable?—Yes, I think there would be that risk, if beset.

3451. (*Admiral Fitzroy.*) Do not you think that the cable would sink as the ship was going very slowly through the pack?—You could pay out the cable in that way, but I do not see how you could be certain of arriving at any fixed point upon the coast.

3452. Starting from the coast?—You could not go out in a given time;* if you come out, and get into the pack, you may be beset for 24 hours.

* I meant to express my opinion that a ship could not depend upon starting seaward from a point on the south coast of Greenland on any fixed date during the summer; but that, if all ready, and with a look-out from the greatest attainable elevation, a ship might dash out upon an opportunity, which would occasionally offer, and when but little ice would be off-lying.—*A. Y.*

3453. (*Mr. Stuart Wortley.*) You may be beset as the "Fox" was, and dragged away many miles to the north?—Yes.

(*Admiral Austin.*) You cannot make your way through the pack; you must follow the leads, which may be very circuitous.

3454. (*Mr. Stuart Wortley.*) Independently of the difficulty of making the coast, there would be the waste of cable in dodging backwards and forwards?—Yes.

3455. (*Admiral Austin.*) In making a way through the pack, I recollect an occasion when we were supposed to have passed 500 miles through the pack, and had only advanced 200 miles.

3456. (*Mr. Stuart Wortley.*) You might lay 500 miles of cable where you only required 200 miles?—(*Admiral Austin.*) Yes, it is so uncertain through the pack.

3457. (*Chairman to Mr. Young.*) What is the distance through the pack?—It varies; it has been known to extend as much as 100 miles southward of Cape Farewell.

3458. What would be the smallest thickness of it?—We passed within 20 miles of the south coast, and saw none whatever till we got round to Cape Desolation, and there we fell in with it.

3459. There is no pack at all off Fredericshaab?—In July 1857 there was a pack of Spitzbergen ice, of some 20 miles in breadth, off Fredericshaab. We passed near the coast from Cape Farewell to Cape Desolation, without seeing more ice than a few light streams under the land; but on arriving off Cape Desolation, we fell in with the above pack, through which we bored, and by which we were beset, and bound for Fredericshaab, we were carried 30 miles to the northward of that port, before we could get into the land. (*See* Question 3463.)

3460. In such a case as laying a cable you start from Fredericshaab or Julianishaab?—You cannot depend upon that; you might have ice over and over again in going direct from here to one of these places, the changes are so great.

3461. It would never be safe to commence laying a cable from Great Britain to one of these ports if you were not certain when you arrived there of being able to go in?—No, it would never do to start from the eastward on to the coast with the cable astern with the chance of finding it clear; after September I believe you might, when there were not westerly gales to contend with, in which you would do nothing.

3462. (*Mr. Stuart Wortley.*) In this article you state that on one occasion when you were beset you drifted 65 miles: "October 17. We obtained good observations and found that we had drifted 65 miles"?*—Yes, that was in two days; you must remember that we were beset in the moving pack at that time; the whole pack was moving from the effect of a violent gale.

3463. Then when beset you might be taken that distance out of your course?—With a violent gale: for instance, we meant to touch at Fredericshaab outward bound, when we fell in with this pack; we pushed into it and were pushed 30 miles to the north before we could strike the land; we sounded on Talbert bank, which extended nearly 30 miles.

3464. I see that you say, "After a zigzag drift out to the westward until the 24th instant, we have commenced a sudden drift,"† so that the operation of passing through drift-ice may be liable to be very circuitous?—If you are beset in the pack you go before the wind generally, assisted by the currents.

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3465. (*Chairman.*) Have you had any experience in sounding?—No, none whatever, except for the ordinary purposes of navigation.

3466. (*Mr. Stuart Wortley.*) I suppose you agree on general principle with the importance of having soundings in all these instances?—Certainly.

DEAR SIR, Riversdale, Twickenham, Feb. 12, 1860.

I RETURN you the proof of my evidence before your Committee upon the Greenland, &c. telegraph, and I have made a few corrections necessary to explain my meaning in reply to your questions, and you will see by those corrections that I am sanguine of the possibility of laying and landing a cable upon the south-coast of Greenland by starting *from the coast*, but on the other hand I cannot think that a ship could depend upon carrying a cable to any desired position *on to the coast* in the summer season. But the changes are very great, and the quantity of ice is ever varying. The ice breaks away from the polar seas around Spitzbergen, and, coming down the east coast of Greenland, is usually first seen off Cape Farewell towards the end of January; it is heavy *oceanic* ice of all sizes and forms, but seldom exceeding 50 to 60 feet in thickness, and not a piece of it to be compared to a Baffin's Bay or Davis Strait berg, but very different in character from the lighter ice formed in narrow seas or straits. The Spitzbergen ice drives along the south-west coast of Greenland up the west coast as far as latitude 64° and even 67° N., where it is supposed to be taken up by a westerly current across Davis Straits, and joins the vast stream down the west side of those straits and the coast of Labrador. The current which brings the ice also brings quantities of drift-wood from the rivers of arctic Asia and is known as the Spitzbergen current; it has a velocity of from 12 to 14 miles in 24 hours.

The limits seaward of this moving pack are so variable that it has been known in some seasons to extend for 100 miles off the land, while at others the south and south-west coasts have been found almost clear, but all attempts hitherto made to approach the east coast have failed. Now from the information gained by experience of former voyages, it would appear that there are occasionally *breaks* in this pack, that is to say, that from some change of wind, the ice may move along one part of the coast, while it is stopped on another part, and consequently an open space of water left; therefore I think that by watching an opportunity, a vessel might readily pass out when one these breaks in the pack occurs.

The fiords are all deep and all similar, and I doubt not that one might be found suitable out of the many on the coast. The fiords are mostly protected seaward by groups of islands, and the ice does not appear to drive into them with any force, and the water being very deep and the coast precipitous, the ice does not ground, although a few straggling pieces may get up to the heads of the fiords and there lie quietly in the mud. The Spitzbergen ice is seldom seen after September, by which time it is pretty well dispersed or melted, and but very little ice forms on the south-west coast during winter.

I think that we have a good many soundings in Davis Straits, at any rate in the parallel of 66° 30′ N., the narrowest part of the Straits, where we have a fair idea of the depth of water across. These soundings are principally by Parry, and appear in the Admiralty Chart, Arctic Sea, Baffin's Bay, Sheet 1.

The greatest known depth in the above parallel is 388 fathoms, the water deepening from the banks on the east side to the west side of Davis Straits. We also obtained a few soundings in the "Fox" in these parts.

Believe me, &c.

Capt. Douglas Galton, R.E. ALLEN YOUNG.

Admiral ROBERT FITZROY, F.R.S., examined.

Admiral R. Fitzroy, F.R.S.

3467. (*Chairman.*) Will you give the Committee your opinion upon the practicability of laying a cable between England and America by way of Iceland and Greenland?—For several years I have been decidedly of opinion that a line by Rockall bank, by the banks to the southward of Iceland, the banks to the southward of Greenland, and the offing of Labrador and Newfoundland is the best for a telegraph wire between Europe and America. If I may say so without presumption, I believe that mistakes have been made in the character of our wires. I think that the wire which should be used is a single copper wire of large size, with peculiar jointings that would make it easy to have any length, short or long, and which could be joined with facility at sea. Copper wire, instead of being covered with gutta-percha or India-rubber or any

* This was at the head of Baffin's Bay.

† This was in Baffin's Bay.

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vegetable substance that is liable to decay under water, might be covered with a vitrified substance, a kind of varnish of the nature of the varnish on a plate, or the glazing on pottery; for example glass, with a sufficient quantity of lead or other annealing mixture to make it sufficiently pliable, might be used. A copper wire coated with a vitreous substance of that nature, would last under water any time, and would be quite as manageable and as pliable as a wire of any character hitherto used. I speak as a practical sailor only, I beg to observe, and not as an engineer; but from what I have been able to collect, I have no doubt whatever that the conductivity of a copper wire increases as its diameter geometrically; whilst the inductive action only increases arithmetically.

The induction increases unquestionably as the superficies of the wire increases, but the conductivity increases geometrically with the diameter; the one, therefore, may be increased immensely, while the other is *comparatively* but little augmented. That argument, I apprehend, if true, in favour of as large a copper wire as can be used, makes all this iron coating a mistake. It is not only unnecessary weight added, and making the wire unnecessarily stiff and unmanageable, but this iron work will inevitably rust away in the course of two or three years. If it does not rust away in some places where it becomes buried in soft ooze and is covered over, it will rust away where it comes into contact with rocks, particularly the *copper* rocks which exist on many shores. I have known a ship's chain cable so damaged by lying upon copper in the bottom of a harbour in South America, that after lying there for two months, the chain-cable was unserviceable. Many links were eaten away by the galvanic action of the copper at the bottom, and the salt water, upon the iron. Of course, wherever there is a copper vein cropping out of the ground under water, a vitreous covering would protect the wire more than anything else. If an iron-covered cable were to lie over it, the iron work would be eaten away very quickly. There can be no doubt that the difference of duration of the iron-covered wires on different coasts—one lasting a very little while, and the other not appearing to be injured—is in a great measure due to the action of substances at the bottom upon the iron, independently of the actual friction, whatever that may be, supposing it lay upon rocks.

3468. Will you look at that cable (*handing a specimen to the witness*). Would that obviate your objection to the iron covering?—So far as not being coated with iron; but all this would be chafed away in a very short time by the sharp edge of a metallic vein, or the edge of a basaltic rock.

3469. Would not even a vitreous covering, such as you propose to give to the wire, be chafed away?—I think not. I think nothing would last so long as a sufficiently thick coat of flinty varnish,—silex, lime, and lead being perhaps the main ingredients. It would become like the covering of a plate, or piece of earthenware, only much thicker. It might be increased to any extent. Of course, the materials for a vitreous varnish are abundant. I would have such a covering as that of a piece of earthenware, only very much thicker and tougher. Many coats, one after another, might be found necessary to protect the wire; to make it as nearly like tough stone as it could be, consistently with the necessary amount of pliability. I would not lose sight of the pliability. Some stones, even some kinds of coral, are tough, hard, and pliable to a certain extent, sufficiently so for coiling in large coils.

3470. Have you seen any wire covered with such a material?—No; I merely suggest it. It is well known that the addition of lead to glass makes the glass pliable. There is another fact which is perhaps not generally known with respect to glass (to which a person now in this room can bear witness), that under water glass is much more pliable and manageable than it is in air; so much so that with a pair of scissors you can cut thin glass under water without its breaking. The glasses of compass bowls have been cut round in that way.

3471. Was that done with a common pair of strong scissors?—Yes.

3472. Merely by putting it under water?—Yes. I only offer these as suggestions. There may be something much better for covering the wire; but, looking at the experiment in the Black Sea, the Balaklava wire, which was only a small single wire, and which answered well till it parted—looking to that fact, and to the unnecessary weight—saying nothing of the expense—of a covering of any kind throughout the great extent of an ocean where the wire lies at the bottom, sinks down into soft ooze (now found so generally), and is not exposed to much friction—I think an iron coating objectionable.

If exposed to much friction, it should be coated, and particularly near the shore, with copper wire, but not with iron. On most shores iron will rust away; therefore the covering ought, if required at all, to be copper.

Lightning and electricity are only different degrees of the same quality. In lightning conductors the joints are not brazed together. In the lightning conductors which are now let into ships' masts, one part only touches the other; they are not brazed together. I apprehend that there has been some unnecessary scruple as to the manner in which joints have been made, and as to having many joints. Now—it is the custom to soft-solder the different wires at various places. Suppose the joint of a solid wire were more like the simple joint of a common gold-chain (commonly called a snap joint) and covered with a ferule—soft soldering might also be used if necessary; or if found to answer without, which in a sufficiently large wire might be found to be the case—the difficulty and inconvenience of joining at sea would be readily overcome.

3473. (*Mr. Stuart Wortley.*) Are you aware of any invention which has been made public or patented of any vitreous substance such as you describe?—No, none. I never heard anybody suggest the substance. The glass that is used for toys made of spun glass, is sufficiently ductile. The glass lamp chimneys supplied to lighthouses formerly were found to be much too brittle; and the remedy then was to put more lead in them, to make the glass tough. The glasses did not then break, but they became rather obscure when heated, which would not matter at all in this case. The glass became darkened when much heated.

3474. (*Chairman.*) What extent of elasticity have those glasses? would it be sufficient to allow them to bend?—Not so much, only enough to make them tough, not brittle; but you can give glass as much ductility as you can give a piece of copper of the large size necessary for a single telegraph wire.

3475. The more you increase the copper wire, the thicker must be your outer covering of glass?—I think not exactly so; but the more you increase the copper wire the less pliability you would require in the glass. The smaller the wire, the more it might be bent.

3476. (*Mr. Edwin Clark.*) Do you think that a wire coated with the vitreous substance would run round ordinary paying-out machinery?—If the material were sufficiently pliable, and the machinery suitable.

If the wire used were comparatively light and manageable, without any iron around it at all, if gutta-percha or any other substance were used for insulating it,—with a large single wire of that kind, without any iron, it would be easy to run it round from Ireland* by those points I have mentioned: namely the extremity of Rockall Bank, the vicinity of the Faroe Islands, without touching them; by the banks near the south of Iceland; by the vicinity of Cape Farewell (but not landing); then near Labrador, and so to Newfoundland. Such a wire should not be all in one length, but in several lengths, and in several vessels,

* Blacksod Bay?

joining the wire together being a minor question: coiling it, and the manner of dealing with it in several vessels, being much easier than if all the wire were put in one or two ships. The object of taking that route would be to get comparatively shoal water all the way.

3477. (*Chairman.*) What depth would you get?—From such descriptions as I have been able to get on the subject (you are aware that the knowledge of these waters is exceedingly scanty); and from information obtained from Mr. Crowe, the Consul-General in Norway, who has had much communication with Iceland and the Faroe Islands, and has had a good deal to say to the fishermen who have gone from Denmark in that direction, and from information which the Dutch have of those localities, I conclude there are banks something like the banks at Newfoundland, only not so extensive, running off under water to some distance south of Cape Farewell, to some distance south of Iceland, and also from the Faroe Islands.

It should be a primary object, I apprehend, to carry a line of soundings from the extremity of Rockall Bank to the depth of 200 or 300 fathoms, near the Faroe Islands; then to a few hundred fathoms' depth off Iceland, and so on by Greenland to Labrador and Newfoundland. It seems probable, from the soundings that have been hitherto obtained, that there are but small intervals of deep water, if any; that it is quite possible you might go from Ireland, taking that round, to Newfoundland, without having deeper water than 1,000 fathoms.

3478. Should not we get a much longer line?—One third longer, which if you increase the conductivity of the wire, would not be objectionable, provided that it were laid out in different lengths, from different vessels, not all from one ship. The conductive power of these little wires that have been used hitherto has been trifling; the wires have been so very small that not only have they not had enough conducting power in themselves, but when heated by repeated electric currents (as electricians know they must be), their power of conducting became less and less, and much of the delay occasioned in transmitting messages was due to the heating of the wires themselves by the repeated action of electricity. That I apprehend has been found to be the case; but I speak under correction. The landing of the wire, if necessary, at those places, could no doubt be effected by selecting the season at which they are open, at which sailing vessels now frequent them. There are traders between Iceland and Europe every year; also to Greenland.

By taking the wire in comparatively deep water, say 200 fathoms, until you get opposite the mouth of the selected fiord, and then, at the proper season (having arranged so as to hit that season), taking it up through the deepest part to the head of the fiord, and there covering it over to a sufficient distance from the bottom, to guard it from any ice that might be formed, or move along the bottom of the place where the wire is taken, the object would be secured.

3479. In what way would you cover it?—After it had been fairly laid, I would cover it with earth, first, and then with heavy stones.

3480. Have those fiords rivers in them, or are they merely still creeks of water?—There are watercourses, and very many cascades, from melting ice and streams running into them.

3481. Would not they be liable to disturb the cable?—Undoubtedly. But you may select a place, and build over it to any extent; you may lay any amount of rocks over the wire when once it is covered with soft earth.

A question was asked in this Committee the other day respecting temperature at various depths, and the means used for ascertaining it, to which I may as well take this opportunity of referring, because the instruments are in the room. For several years past the thermometers used for sounding at great depths have been fortified with additional glass tubes, for the purpose of preventing any possibility of the bulb being compressed; and between the outer glass and the bulb there is a certain amount of mercury. This is one of the instruments (*producing a thermometer*). I believe the statement by a witness the other day was to the effect that the thermometers used were not strengthened, and therefore their indications were not to be relied upon. There must have been some mistake; they have all been of this kind lately, and were so in the "Cyclops."

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3482. (*Admiral Austin.*) How is the mercury supplied from this tube?—The interior bulb has spirits of wine; this is merely an exterior guard, as it were; the mercury is outside the bulb containing the alcohol, it is merely as a cushion between the outer case and the inner bulb—a kind of "jacket."

3483. (*Chairman.*) Is there a vacuum in addition besides the mercury?—There is a partial vacuum. Within the innermost tube there is a little air sufficient to act against the alcohol which is in the bulb. The instrument is like a once straight thermometer bent double or trebly. There are alcohol, mercury, alcohol, and a little air successively.

3484. What is the object of the quicksilver jacket?—When great pressure comes upon the middle part of the long bulb, with its outer cover of glass—when great pressure comes upon the weakest part, the interval between, being filled with mercury, tends to resist that pressure by diffusing the action along the whole length.

3485. The mercury is squeezed up?—The mercury is squeezed up both ways to act upon all points instead of upon one only. Then again, there is another reason; if one were put outside the other with only air between, air being a non-conductor of temperature, error would be caused, therefore it becomes necessary to have mercury.

3486. Have you any records of the temperature at the bottom of the sea in different localities?—Yes, and they *should* be known in these regions; the records made with good instruments have been in the Atlantic, north and south, and in the Indian Ocean.

3487. (*Admiral Austin.*) This instrument is a recent introduction?—Yes. It was first made, at my suggestion, in 1856. This other (*producing another*) is a French plan, the glass being excessively thick, with the view of resisting pressure.

There is also a cause of error from the vibration of the indices in hauling up; they may gradually move either up or down, and probably have done so, occasionally.

3488. (*Chairman.*) Has any regular series of observations been put in hand to be conducted at the present time with respect to the temperature of the ocean?—None that I am aware of. The last were those made by Captain Dayman in the "Cyclops"; he did not take any recently in the "Gorgon," having another object in view. Captain Pullen has taken some in the "Cyclops," now in the Red Sea—or the Indian Ocean.

3489. Did not Commander Dayman take some observations upon temperature when he was sounding for the Gibraltar cable in the Bay of Biscay?—He took soundings, not temperatures at *depths*. He took these when he was sounding the Atlantic.

(*Sir J. Ross.*) Very valuable soundings they were, and very well done.

(*Admiral Fitzroy.*) There is a very interesting chapter, the 17th, I think, it is in the eighth edition of Maury's book on the specific gravity and temperature of the sea, under various circumstances. The results of the experiments are worked out by Professor Hubbard, who is said to be a thoroughly competent person, and who has controverted some of the opinions that have been held by savants in Europe as to the temperature of the sea.

Its heat, density, and subjects of that nature, he has treated in the 17th chapter, which goes immediately to the question that Sir James Ross took so much trouble and interest about, and to which he was referring to-day, namely, the permanent temperature of the sea at a certain depth in various latitudes, as

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well as the least temperature that has been obtained anywhere.

3490. (*Sir J. Ross.*) Do you remember what he considers the least temperature?—Yes, he says 25° or 26°; that is the temperature at which he considers it is at the greatest density. It had been supposed, by the best authorities, that 39° is the greatest density of salt water. 39° has been proved to be the greatest density of fresh water. Professor Hubbard, however, thinks he shows cause for salt water being entirely different, and for the greatest density of salt water being about 25° or 26°, 13° lower than the greatest density of fresh water.

3491. Are there any facts given to bear out the theory?—The experiments are given in great detail: and the opinions of Herschel and others are controverted.

3492. (*Chairman.*) Does he not concur with Herschel's opinion?—He is at variance with Herschel, and with Forchammer, who made experiments at Copenhagen upon the density of water. It is a purely philosophical question, ably treated by Professor Hubbard, who has been accustomed to such studies.

Reverting to the question of the size of the wire. Perhaps it has been mentioned before in the Committee that there was, only a year ago, and may be still, great difference of opinion among electricians upon that very point. Last year some maintained that very small wires conducted electricity better than large ones.

3493. (*Mr. E. Clark.*) To what thickness do you propose to cover the wires with this vitreous substance?—According to the size of the wire; perhaps about two-tenths of the diameter of the wire.

3494. You will have an enormous inductive effect, with so thin a covering round a wire?—I do not apprehend that the vitreous covering would alter the inductive action notably, while it would be a nearly perfect insulator.

For ages past, in some parts of the East (Cochin China particularly), buildings have been supposed to be protected against lightning in exactly the opposite manner to ours, namely, by lumps or masses of glass on the tops of the most projecting parts.

3495. Has that any effect in preventing damage by lightning?—They say in those countries (I have only the statements of persons who have travelled there) that no building so protected has ever been struck by lightning. Some of the largest structures in the East have such glasses, commonly supposed to be ornaments, on the tops of the pinnacles or highest parts, which have been put there as a protection against lightning. But without referring to that—insulation by a small thickness of glass is a matter of every day occurrence; it would only be carrying the property of glass as an insulator into effect, and making it sufficiently pliable to answer the purpose of a coat upon the copper.

3496. Are you not aware that the insulator has nothing whatever to do in preventing induction. Supposing you have the glass the thickness of an ordinary Leyden jar, there would be no way whatever of preventing induction. Induction is not the escape of electricity; it is electricity which becomes present on the other side of the insulating material?—I am quite aware of that, but venture to doubt your inference in this case. The vitreous coating between the metal and the water would not be ordinary glass. The experiment has yet to be tried, and the mixture of flint and lead may be modified with other substances. I do not for a moment pretend to say what should be the exact mixture, but I believe, from what I have been able to collect, that the insulation may be completely effected by a mineral medium between the copper and the water, as the gutta-percha, when used in a sufficient quantity, answers now.

3497. (*Chairman to Mr. Siemens.*) The better the insulator the greater the induction almost; is it not so?—Generally.

3498. The more perfect the insulator the greater will be the induction?—Yes, you can only have induction strongly developed with a good insulator.

3499. (*Admiral Fitzroy.*) If there were no insulator at all, but a very large wire, what would be the result?

(*Mr. Siemens.*) You would have no circuits; the electricity would return immediately to the other pole of the battery, without going round by America, but being forced in that channel the induction is produced.

3500. (*Admiral Fitzroy.*) Why do you use gutta-percha so as to insulate the wire as completely as possible?

(*Mr. Siemens.*) There are two causes operating against the speed of the electric current; one is by discharge, by leakage, and the other is by the absorption of electricity by induction. One material may allow no escape at all, and yet absorb a large quantity of electricity by induction; hence there would be great difficulty in speaking through that conductor. Another wire may be so insulated, with gutta-percha for instance, that a great proportion of the current would escape by leakage. That again would be a badly insulated wire; but there are those two causes operating quite independently in different materials, and even in the same material.

3501. (*Admiral Fitzroy.*) You do now seek to insulate the wire perfectly by gutta percha?

(*Mr. Siemens.*) We speak better through a moderately insulated wire; for instance, we speak quicker through a gutta-percha coated wire than we should through an india-rubber coated wire, because the induction is less in proportion to the leakage, but that leakage is not sufficient to interrupt rapid communication.*

3502. (*Chairman to Admiral Fitzroy.*) I understand you to suggest that the cable should be in several vessels; would not that necessitate making more joints at sea?—Yes.

3503. Of course, in rough weather you would have a difficulty in making joints at sea?—If the wire were divested of all that armament which makes it so heavy and unmanageable there would be no difficulty in passing it between the vessels with the aid of a light steamer going backwards and forwards.

3504. Even if there was a strong gale blowing?—In a strong gale you must suspend operations; you must wait for a time and use proper buoys; not large unwieldy buoys that would by the force of the wind and sea tear away the cable, but light buoys of a peculiar construction, visible at a sufficient distance, and which would ride by the cable itself through any gale.

3505. Would not any buoy which was sufficiently exposed to be seen at any distance, be liable to create a great strain upon the cable from the pressure of the wind upon it, or could a buoy be devised which would not have that effect?—Not if it were such a buoy as has been lately proposed for another purpose, namely, a glass topped buoy—which would be seen glittering at a considerable distance in the day-time and would not be liable to be injured by water, but might remain for some time.

3506. You must assume that there are no currents?—There are not currents of any importance in the deep ocean; you do not find currents of any strength in the depths of the sea, except the Gulf Stream, and in a few other exceptional localities. Generally speaking, there is very little current in the open ocean.

3507. Taking the line for laying the cable which you suggest, are there not several points at which there would be currents of some strength?—I think not. If you do not approach the land nearer than 200 miles, you will not feel much current. Supposing there are three or four hundred fathoms, near the land, and a strong current sets along the shore, you must increase your distance so as to avoid the strength of the current, and yet not have an unmanageable depth

* Admiral Fitzroy ventures to *dissent entirely* from the theory of *circuits*, as above mentioned; and *formerly* held generally. For reasons unnecessary here, he advocates the *largest wires*,—and *most perfect insulation possible*.

of water. You should endeavour to keep in about 800 fathoms, or thereabouts, probably.

I would observe, that having a cable made of twisted wires appears to be very objectionable, because you cannot bring a strain upon the twisted wire, or relax any strain, without causing a tendency to turn it round and round, and if it is relaxed, of course, to make kinks. If you veer out a cable that has been twisted up, and which has been strained to a certain extent, it will immediately, as soon as the pressure is off, form into kinks under water.

3508. Must not any cable that is coiled receive a twist in it?—No, not if it has no twist in itself. The mere act of coiling a straight wire need have no tendency to twist it. The tendency to kink is owing to the wires being laid up like a rope; and if it is indispensable to have several wires instead of one, I think they ought to be, as my brother seamen will understand, "*selvagee-fashion.*" They should be laid parallel to each other, and united as much as necessary at short distances.

3509. Would you unite them in that way (*handing a specimen of cable covered with plaited wire to the witness*)?—No; I am not speaking of the outside wires. I mean that the inner wires should lie parallel to each other, in lengths united here and there, and that there should be no twist; also, that, if necessary to cover them on the outside anywhere with copper wire, it should not be *twisted* wire.

3510. The outside you would cover with copper?—Yes; I would have no iron anywhere.

3511. Does not copper stretch very much more than iron, when a weight is put upon it?—Yes; but there is no objection, I apprehend, to that.

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3512. If the core stretches, it becomes thinner at that point, and therefore the size of the conductor is diminished to that extent?—I set out by advocating a copper wire much larger than has been used, so that the strength should be in the wire, and not in the covering.

3513. Would not copper wire if it were extended tend to fracture the outer covering?—Undoubtedly. If the copper wire stretches much, while the covering is not as ductile or yielding as the metal itself.

3514. Would it not be difficult to get a vitreous substance which would extend in the same way that a metallic substance would extend?—Not if it were sufficiently annealed or prepared.

3515. (*Mr. Edwin Clark.*) Is not all glass very elastic?—Yes; it may be tempered to a great extent by the mixture of lead or other minerals.

3516. But copper is not elastic, and does not possess the property, when it is stretched, of returning to its form again, which glass does?—The glass being elastic, would not prevent its giving as far as the copper gives, and remaining so if held. If you stretch a piece of india-rubber, and keep it stretched, it remains in that state. In the case of a wire, you would not let go; it would be stretched, and would keep the covering stretched.

Having succeeded so well with the Balaclava cable, which was a single wire, I think that further steps should be taken in order to ascertain the possibility of a more extended use of a similar arrangement.

Adjourned to To-morrow at Two o'clock.

Thursday, 2nd February 1860.

PRESENT:

Captain Douglas Galton.
The Right Hon. J. Stuart Wortley.
Mr. G. P. Bidder.
Mr. Latimer Clark.
Professor Wheatstone.
Mr. Edwin Clark.

Captain DOUGLAS GALTON in the Chair.

Latimer Clark, Esq., Member of the Committee, examined.

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3517. (*Chairman.*) I believe you are engineer to the Electric and International Telegraph Company and to the Atlantic Telegraph Company?—Yes.

3518. You have had experience for some years, have you not, in the superintendence of telegraphic works?—I have had the charge of the submarine cables of the Electric Telegraph Company since 1851, and I have had the charge of one or two other cables for other parties.

3519. You have generally been connected with electric telegraphs for several years?—I have been exclusively connected with the subject since 1851.

3520. (*Mr. Stuart Wortley.*) Have you devoted your attention almost exclusively to the subject as an engineer?—I have devoted my attention as much to the electrical as to the engineering department since that time.

3521. (*Chairman.*) Have you had experience as an engineer in the manufacture and paying out of submarine cables?—Yes; I have never been present at the paying out of any ocean cables, but I have been present at the paying out of several shallow water cables.

3522. Are you acquainted with the break machinery which was used by the Electric Telegraph Company?—Yes, I saw it several times during its manufacture.

3523. What is your opinion of it?—I think it had some most serious defects; one was that which has been noticed by other parties, namely, its great weight and inertia, and the difficulty there would be in changing its rate of motion suddenly according as the vessels rose and fell from the motion of the sea. I think that evil was very much aggravated by the circumstance of the wheels being coupled together, so that the cable had no chance but to make the wheel revolve slower or quicker as the vessel rose and fell. There was another defect which I think has not been noticed by other parties, and which, in my opinion, was a serious defect,—the friction-straps were controlled by weights which were hung upon them. Now those weights, by a well-known law in mechanics, become very much heavier every time the vessel rose in the sea, which was the very time when they ought to have become lighter, because at that time the cable ought to have been enabled to pay itself out more freely; but instead of that, owing to the inertia of those weights as the vessel rose, they became very much heavier at the time when the vessel was rising.

3524. What would be about the weight of those weights?—Judging from their appearance, I should think they probably might have weighed seven or eight cwt., but their effect would be very much magnified by the leverage or that which acted upon the cable. I have noticed, and other parties have noticed, that there was another serious defect in that break owing to its being coupled, that is to say, if any tar, dirt, or other matter got on any one of the grooves of the wheels, it caused them to be of unequal diameter, and formed one of the most powerful machines for breaking a cable that is known in mecha-

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nics, namely, winding a rope from a small drum on to a larger one.

3525. What grooves do you mean?—The grooves upon the pair of paying out wheels which were geared together, and therefore unless they were of perfectly equal size, the cable might be much tighter or much slacker on one of the wheels than on the other. Had the wheels been separate, that evil would, to a great extent, have been avoided; and I think in any future break it would be very desirable, instead of using weights, that springs of some kind should be used.

3526. Have you in your mind any form of break which you would adopt?—I have planned out in my mind a form of break which I think very superior. I should prefer to describe it to you in the form of a separate report on the subject if you would allow me to do so.

3527. What do you think of Mr. Longridge's break?—He has two kinds of break; one is a cone.

3528. I mean the revolving cone?—The revolving cone I do not altogether like; I do not know what argument to use against it exactly.

3529. Do you dislike the friction without any revolving motion?—I do dislike the friction without revolving motion. I think Mr. Longridge's other break, by which he proposes to compress the cable between two moving surfaces, is very superior to the cone, and is an idea well worthy of more consideration. At first sight the cone break would appear as if it made the cable bend in very sharp turns. I admit that that is not really the case, but I do not like repeatedly bending the cable in the manner in which it would be bent in passing over the cone.

3530. Have you formed an opinion upon the effect of ocean currents in the paying out of light cables or heavy cables?—I went into that subject very minutely with the late Mr. Brunel, and the opinion which we formed was one which I hold now, namely, that there is nothing whatever to be apprehended from the effect of currents in paying out cables, whether they are excessively light or excessively heavy, or even if the currents are flowing in opposite directions at two different depths, I think that strain, if it came on a cable held at both ends, would be perfectly irresistible, and that no cable could be practically manufactured which would for a moment withstand that strain. But any movement of the water pressing laterally on the cable would become converted into a longitudinal force. The cable has perfect facility of motion through the water in a longitudinal direction, and therefore would at once relieve itself from any strain which these currents would place upon it by forming itself into an S curve.

3531. Has a cable any longitudinal motion in the water when it has been laid down? Is not it rather the motion of simply being driven through the water vertically as it were?—Theoretically it may be said to be a lateral motion or an oblique motion through the water, but in practice more cable is always paid out, especially in very deep water, and more especially still in the case of very heavy cables; a great deal more cable is paid out than the distance that is run. In practice it is always prudent to do it, so that the cable may not be taut either when it lies at the bottom or during its descent.

3532. To that extent the cable has a longitudinal motion?—Yes, to that extent.

3533. You think practically the currents have no strong effect upon a cable?—I think practically the effect of currents may be entirely neglected, provided we allow the cable to pay itself out to a sufficient extent, to accommodate itself to the currents while sinking to the bottom.

3534. Do not you think that involves the necessity of paying out a very much greater length of cable than the actual distance crossed?—I think not. I think the difficulty is best met, practically, by carefully avoiding putting upon the cable anything like its breaking strain, so that whenever currents do begin to draw away the cable, the cable shall pass itself out from the break without much strain. And for that reason, among others, I consider light cables much better than solid iron cables.

3535. (*Mr. Stuart Wortley.*) In deep water?—In deep water, of course.

3536. (*Chairman.*) For a light cable, would you use a combination of iron and hemp, or do you agree with the view expressed by Mr. Jenkin, that iron is of no advantage whatever to a cable covered in the same way as the Gibraltar core is covered?—In the extreme depths of the Atlantic a simple iron cable, or a piece of iron wire or bar of iron, would not be strong enough to bear its own weight; and, therefore, to that extent iron may be argued to be objectionable. But, notwithstanding that consideration, I am still clearly of opinion that the best cable that I have yet seen for being laid in deep water is a cable similar to that which you are proposing to employ on the Gibraltar line. I think that all experiments show that hemp and iron, and hemp and steel, combine well together, and that you get the full effect of their united strength. I think also that you want a certain degree of weight to enable your cable to sink steadily to the bottom, especially when it has to fall into the hollows and cavities, and not lie loosely across elevations.

3537. Therefore you require a certain amount of pliability in a cable?—You do. I think that all experience has been against hempen cables. I am not aware that any one has ever been laid or has existed for any length of time successfully. I should not at all like to lay a pure hempen cable.

3538. Even if it were covered with marine glue or some other compound?—Not even if it were covered with any compound. Another of the great advantages of the combination of iron and hemp, in the form in which it is about to be used in the Gibraltar cable, is, that the iron is protected from oxidation by the hemp which surrounds it.

3539. Do you think that the greater portion of the oxidation or rust to which the iron would be subject would not be washed out of the hemp?—I am alluding rather to the oxidation before the cable is laid. There is no doubt that the Atlantic cable suffered very severely in some respects from the wires being rusted before it was laid, and that the risk of the operation was very much enhanced by that circumstance. Now, if that cable had been so protected, that those iron wires could not have rusted, its full strength would have remained, instead of its having incurred the risk of some one spot in the cable being partially rusted through, and the strength at that point being thereby greatly impaired; no such risk exists in the case of the Gibraltar cable.

3540. Have you considered the effect that would be produced, if, instead of placing the iron in strands, it were placed in lateral bars along the outside, in the same way that this cable is proposed to be done (*handing a specimen to the witness*)?—I have done so, and I do not fully approve of it. One of the defects of placing the iron perfectly longitudinally is, that a cable so made does not bend quite so kindly and freely as one in which the iron is laid spirally, although I do not consider that that would be a fatal objection. That system has an advantage in this respect, that the material is placed directly in the line of strain, which in a spiral cable it is not. In a spiral cable it is placed somewhat obliquely, and that is a disadvantage, because, if the core contracts, either by the compression of the water, by a strain placed upon it, or by any other cause, that contraction of the core would allow the iron covering wires to elongate, while the core itself has no power of elongation, except that which is due to its own elasticity. To that extent straight iron wires are better than spiral ones; but still, practically, it has been abundantly shown that the elasticity of any covered conductor, which is likely to be used in submarine operations, is very much greater than the elasticity of even a hempen cable. In almost every case the hemp, or the iron, or the outer covering will break before the insulated wire will be injured, and, there-

fore, I do not think the slight objection that exists to a spiral covering is sufficient to counterbalance the many advantages it has in flexibility, and in the security with which it retains itself upon the cable.

3541. Have you examined the proposed Gibraltar cable?—I have.

3542. What is your opinion of it, first, as regards the outer covering of each portion, and next as regards the core?—I think it is a very well designed cable; I think hemp and steel in something like the proportions which are used in that cable form the most useful combination that has yet come under my notice.

3543. For the deep sea portion?—For the deep sea portion only.

3544. What is your opinion of the shore ends of that cable?—My opinion of the shore ends is that, if anything, they are rather too slight. I have seen so many cases of injury to cables lying over rocks from their being too small, that I should, if anything, increase the weight of the shore ends of the cable.

3545. To what extent would you increase the ends which are to be in depths up to 100 fathoms?—Up to 100 fathoms I think I would use a cable weighing not less than 12 tons to the mile. I do not know the weight of the second portion of the Gibraltar cable, but, I think, beyond 100 fathoms, a weight of from four to five tons would be sufficient.

3546. To what depth would you use a weight of from four to five tons, between 100 fathoms and 500?—Up to 400 or 500 fathoms, I think an iron cable might be laid with advantage. There are considerations of a military character quite independently of those connected with electrical and engineering points, upon which, of course, you are as well able to form a judgment as I am. There is no doubt that up to 400 fathoms a cable might be wilfully injured if parties knew where that cable lay, whereas if it were made very strong, it would be scarcely possible to make a rope which would injure a cable at that depth. A light cable, I believe, might be injured easily up to a depth of 400 fathoms.

3547. (*Mr. Bidder.*) Would you recommend extra weight for any other purpose?—I would not; the deep sea portion of the Gibraltar cable has one very serious defect in my opinion. If you take a piece of that cable, attach one end of it to an air pump and exhaust the air, the water flows very freely indeed among the conducting wires. I have no doubt in the enormous depths in which it is likely to be submerged—if it is submerged in its present condition—that water will find out either at the joints or at some other part of the coating some spot weaker than others, from whence it will burst through into the interior conducting wires. There can be no doubt that the pressure of the air within the conducting wires will be only equal to that of the atmosphere, or even if it were more, it would equalise itself with the atmosphere by gradually flowing up between the conducting wires. Now it is perfectly well known that in the pressure due to the depth of the Atlantic, water will flow tolerably freely through even such materials as cast iron and brass when they are at all porous, and therefore I should be extremely apprehensive that the water would force itself in at some cavity or weak spot in the cable by continuing to flow right and left along and among the conducting wires, and would wear such a hole through the conductors as would entirely destroy the insulation of the cable. Indeed, in my own mind, I have always had a strong feeling that there is an extreme degree of probability that the Atlantic cable was injured in that manner. The points at which the two defects have occurred were exactly those at which the full effect of this phenomenon would take place, namely, just where the shoal water changes into deep water. I should most earnestly impress upon the engineers of the Gibraltar line the importance of their either injecting oil or tallow or any other suitable material into those wires in order to prevent the possibility of there being a cavity inside into which the exterior water would force itself, and thereby damage the cable.

3548. (*Chairman.*) Would not it be extremely difficult to force oil or tallow into lengths of a mile in the Gibraltar core?—There would be no difficulty in forcing any liquid into that length; melted tallow would only flow a very short distance in, but it would form nodes, and to some extent the communication with the exterior air would be mitigated; there would be no difficulty in filling it with Hughes' fluid, that would entirely obviate the evil of which I speak.

3549. Have you no fear that Hughes' fluid might act injuriously upon Chatterton's compound?—I believe it would act injuriously upon Chatterton's compound.

3550. Would olive oil act injuriously upon Chatterton's compound?—I am not able to speak with confidence upon that point; it is a mixture of tar and gutta-percha. I fear that it would do so; if such be the case, it would be worth making experiments to see whether it would hurt the compound; the Gutta-percha Company could tell you at once what fluids affect Chatterton's compound.

3551. (*Mr. Bidder.*) If you have the cable cut up into lengths by nodes, would you in that space compress the air so as to equalise the pressure?—I think so; the gutta-percha would be squeezed into the cavities until the pressure on the outside was equalised. There is another effect which has been perceived, and would be perceived in laying any spiral cable, namely, the twisting or the untwisting of the cable as it sinks through the water; that is an effect which has been noticed and described by different observers; by some as a twisting effect, and by others as an untwisting effect; it is rather difficult to gather from their statements what effect really is produced, because some speak of the portion of the cable which has gone into the water, and others speak in reference to that portion which is between the water and the stern of the vessel. I have no doubt, however, that the real effect is the twisting of that portion of the cable which is above the water, and the untwisting of that portion which is below the water. I have lately taken a short piece of cable and put it on centres, so that it could revolve freely; I placed it inside a tube perpendicularly, and allowed water to flow in at the bottom of the tube; it rapidly passed round the cable, and on doing so I found all the cables that I tried revolved very rapidly, and with considerable force.

3552. *That is a vertical cable?—That is the water rising vertically; the cables rotating horizontally.*

3553. *The cable being vertical and the water passing it longitudinally?—Yes, so that to the extent to which cables sink vertically through the water there would be a tendency of the character I have spoken of.*

3554. (*Chairman.*) If the cable were being paid out vertically down to the bottom?—Yes.

3555. If one end is held, and it is being gradually deposited as it goes away from the ship, in effect would the same thing take place?—To whatever extent it passes through the water, to that extent it causes it to rotate; that has been very evident on all deep sea cables.

3556. (*Mr. Bidder.*) Do you think you may attribute that motion to the fact that when the cable is wound up in the coil on board, there would be a very heavy twist in the cable, and that twist would come out either in the shape of twisting or untwisting, simply in the act of being paid out again, reverting to its normal condition?—In the act of manufacturing the cable, the wires are laid in such a manner that there is no twist. When the cable is first laid in the tank in which it is tested, one turn is put in the cable at every coil that is made; when the cable is taken out of the tank, and passed along into the vessel, as the cable straightens itself, that turn is taken out, and in the portion of the cable between the tank and the vessel there is no turn or lay in the cable beyond that which it had when it was manufactured; when it is

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again laid in the ship, there is a turn put in, which again is taken out when it goes into the water. I do not think beyond that there should be any tendency to untwist, because cables, as they are now manufactured, are not made by twisting up wires together, but by laying them into spiral coils.

3557. You cannot lay a wire in a spiral form by any possibility without incurring a tendency to twist. Do you think it is possible that you could lay a cable of 1,000 miles in coils of five miles, without taking it from the straight direction, and having a twist in it?—The effect has been noticed in cables after having been laid in deep water and picked up again, that they are extremely liable to kink, which may be due to either of the causes which we have discussed.

3558. (*Chairman.*) Is the fault which you have mentioned in the Gibraltar cable the only one which occurs to your mind?—I do not know of any other defect in the Gibraltar cable. I think it is a very excellent cable in every other particular. I would remark that the plan of welding the steel wires, I think, is rather objectionable. I think it would be very much preferable to lay two wires side by side, and whip them round with small wire, and then solder them, as is commonly done with conducting wires; a joint of that character is quite strong enough to break the steel wires, and is much more reliable than a welded joint, which, in such a very small wire, is extremely liable to be imperfect.

3559. Have you considered the effect of pressure at great depths on gutta-percha?—Some experiments are now being prosecuted by the committee with that object. At present they have not led to any results, owing to the machinery not being in a sufficiently forward state. I believe gutta-percha to be one of the most impermeable substances that has been used for insulation at present. I am of opinion myself that pressure at great depths would not at all cause water to permeate gutta-percha, but that the pressure would, on the contrary, condense the pores of the gutta-percha (the gutta-percha itself being virtually liquid under this enormous pressure), and would make it as impermeable to water under great pressure as it is under ordinary pressure, that is the opinion I have formed.

3560. Therefore you are of opinion that the insulating power of gutta-percha will not be in any degree affected by great pressure?—I think all experience has shown that its insulating power is not at all affected by great weight, because there are many cases in which cables are still existing under very great pressure without any change whatever taking place in the insulation of the cables, nor have I ever heard any authentic case in which pressure has injured the insulation of a cable.

3561. Have you formed any opinion upon the causes of the failure of submarine cables?—In shoal water there are numerous causes which need scarcely be mentioned, — anchorage, and, above all things, rocks; and I may also, perhaps, speak of rust as being a very common cause of injury; but in deep water, the chief injury, I should apprehend, would be from lightning, and from a phenomenon which constantly occurs in gutta-percha wires, which I believe now is confined to them; it is this, that after lengthened use, especially after great battery power has been long sent through them, a small fault will sometimes display itself, without any apparent cause; that will not interfere with working at first; but, after a time, the currents will gradually enlarge the fault, and make it become so great as to stop all working. I never yet satisfactorily, to my own mind, accounted for those faults, and therefore they remain, in my mind, a sort of shadow on submarine cables. I should be very glad if any explanation of them could be given; at present, as far as I have seen, gutta-percha is liable to have these small faults, commencing in it without any very apparent cause, and gradually enlarge themselves till they stop the working. One result has been that we have had to lift cables, in shallow water, to cut out those faults, and replace them, which we have done without learning much from them, unless they have been made, as they have in very many cases, by lightning.

3562. Do you consider that some action is going on in consequence of the current of electricity?—We have always found, when we have cut these particular local faults out from cables, that the insulation of the remainder of the wire is as perfect and as good as when it was first submerged. At the present day our cables across to Holland, which were laid in 1853, after these local faults have been taken out of them, are quite as perfect in their insulation as they were when first laid. There is this liability to faults coming on in gutta-percha from which we never feel safe.

3563. Have you formed any opinion as to the best mode of obviating defects of that nature?—I think we may hope that india-rubber, or perhaps Hughes' fluid, or Wray's compound, in combination with gutta-percha, or alone, may be found free from that peculiar defect. I have a great hope that it may be confined to gutta-percha alone; but experience only can determine that. I am at the present time submitting a number of lengths of various kinds of cable to the constant action of a very powerful current, in the hope that we may learn which of those cables is most liable to that kind of injury, and also ascertain the cause of it.

3564. Is not this liability to injury diminished by increasing the number of coats of gutta-percha?—I have no doubt it is greatly diminished, or perhaps entirely obviated, by either increasing the coats or by increasing the thickness of the insulator.

3565. Do you think it would be sufficient to increase the thickness of each coat, and not to increase the number of coats?—I think an increase of the total thickness would probably obviate the effect, for we all know that the tendency of the current to burst through increases very rapidly as the thickness of the coat diminishes.

3566. Do you consider that this effect takes place from the current having burst through the outer covering?—I think the current first forms channels, through which it works rather more powerfully than it does through the generality of the cable, and having established a channel for itself, it incessantly pours through this channel, until at last the quantity of current that passes is sufficient to galvanize the channel, and then the process of enlargement goes on rapidly. If I take one of these minute faults before it has grown to any magnitude, and put a powerful battery upon it, it would instantly burst through in the shape of visible sparks.

3567. Have you seen any cases of injury from lightning to submarine cables?—Injuries from lightning are very frequent both in submarine cables and in underground wires, wherever they are in connexion with overground wires. I think the best remedy for this in submarine cables is to lay a considerable length of the telegraph near the shore in an iron-coated cable, and to place lightning conductors both at the margin of the sea where the submarine cable commences, and also at the end where the land cable commences. In that way some resistance is opposed to the lightning travelling on and causing injury to those parts which are beneath the water; the large injuries will probably occur near the sea. I have also designed a peculiar lightning conductor, which I believe to be extremely well adapted for burying in out of the way exposed places.

3568. What is the form of that lightning conductor?—It is made by taking a slab of slate, drilling holes through it, and placing it upon a planed slab of iron; pins of brass are then thrust loosely through those holes, so as to rest on the iron; melted lead is poured over it, so as to cement the holes solid; this compound of a cake of slate, with these points passing through it and resting on the iron, is then lifted up, and a piece of ordinary silk or oiled silk is placed between the slate, the points, and the slab of iron, and there supported. The conducting wire is placed in

connexion with this slab with the points, so that any lightning would burst through the oiled silk, and make its exit to the earth. The whole of this apparatus is enclosed in a cast-iron box, luted in with pitch, and some quicklime is enclosed with it, so that the atmosphere is always kept dry within.

3569. Have you tried that lightning conductor?—Not practically, only experimentally. In its general form it corresponds with the lightning conductor which has been used commonly for many years past, the peculiarity consisting in the method of adjusting the points and the use of quicklime.

3570. Can you give any account of your experience with respect to the injury of cables from rust?—We have found that the portions of our cables that are thoroughly imbedded in the sand are excessively durable; in fact, scarcely any change whatever takes place in the iron coating, but wherever the cables are exposed to the constant action of sea-water, rusting takes place very rapidly. I have sometimes thought that the reason why the cables are so protected by sand alone, is, that the injury is caused either by a small quantity of oxygen which is in the water, or by minute proportions of iodine or other corrosive salts which are in the sea-water, and which having once attacked the cable as it lies imbedded in the sand, destroy themselves by their action on the cable, and the sea-water which then remains in contact with the cable is no longer charged with oxygen or with those corrosive salts. I have tried to obviate the injury which is caused to all iron cables exposed to sea-water, by coating them with a covering of yarn or tape, saturated either with asphalte or marine glue, or some protection of that kind, knowing, although it would be impossible absolutely to prevent sea-water injuring the iron, yet hoping that the effect which takes place in sand where the water is not allowed to change itself freely, would also take place with even an imperfectly saturated covering of marine glue or asphalte. A sufficient time has not yet elapsed to enable me to form a correct opinion of the value of that process.

3571. What cables have been laid to which that process has been applied?—The only cable yet laid coated in that way is a cable to the Isle of Man.

3572. Is there no difficulty in applying the outer covering of marine glue to the cable after it has been made?—There appears none whatever. The contractors who applied this process at first attempted to do so by actually passing the cable bodily through a vessel of melted pitch. The result was that the first five yards that they passed through it contained a fault, and the general aspect of the thing was so frightful, that I begged of them at once to stop it, and to cover the cable in the way in which I originally recommended, which consists in revolving a tank of melted material round the cable as it rises up vertically, having bobbins of yarn revolving in the melted asphalte, paying off on to the iron cable as the whole mass revolves round it. In this way the temperature is never raised sufficiently to incur any risk of softening the gutta-percha in the interior of the cable. No difficulty, whatever, was found in the application.

3573. Have any faults been detected since?—No fault of any kind has appeared since; the cable was laid out with as much facility as any ordinary cable, and at rather a higher speed; in fact, at five miles an hour.

3574. Did you experience any difficulty from the sticking together of the cable in the coils?—No difficulty is experienced in that way; it is necessary to use wet sawdust if the material be asphalte, which would spontaneously adhere. With a material like marine glue such a precaution would not be necessary, for in its cold state marine glue would not adhere.

3575. What is your opinion of gutta-percha as an insulator?—I have given my opinion generally upon that subject in speaking of the faults which occurred in gutta-percha. It does not stand very high as an insulator, in the condition in which it is ordinarily manufactured; its insulating properties may be very much increased by more highly drying it. I have seen some gutta-percha that the company purchased from Messrs. Siemens, Berlin, which I believe was worked into an extremely dry condition, but I fear, from experience, that gutta-percha is liable to spontaneous decomposition, or to oxidation, to a much greater extent than it is in the ordinary state.

3576. Was it manufactured for a cable?—It was.

3577. In any peculiar manner?—I believe it was merely very highly dried in the process of mastication, but I have no positive ground for that belief.

3578. Was it equally ductile?—It was not quite so ductile as gutta-percha in general; it was rather harder and much blacker in character; gutta-percha, when at all exposed to the air, is like most of those substances, extremely perishable, and even when buried in dry ground the oxygen affects it most rapidly, and it soon becomes brittle. I have found that by immersing it in oil it appears to remain uninjured for an indefinite time. I have a wire which was laid in 1851, in a leaden pipe full of oil, which has undergone, apparently, no change whatever. I have also found that where covered in the manner adopted by Mr. Edwin Clark, which was that of covering it with tape and tar, that the change appears to be greatly arrested, though not entirely prevented. I have still further extended that system by embedding gutta-percha wire in melted asphalte, or any similar cheap compound, and I have found that in that way, so far as my experience at present goes, gutta-percha may be made to last for an indefinite time without change.

3579. Do you apply the melted asphalte in the same way that you do in covering a cable?—No; in some instances I have applied the asphalte by merely laying the gutta-percha wires in troughing and pouring in the melted asphalte in a nearly cold state round the wires.

3580. What temperature do you consider safe for the asphalte?—A temperature of 100°. In other instances I have placed the wires, which have been previously laid up in the form of a coiled cable, in a tank of iron made air tight, and then exhausted the air by an air pump so as to get a tolerably perfect vacuum, then I have admitted the asphalte in a melted state from another tank, and allowed it to flow in among the wires. I then lifted them out quickly, scraped off the surplus asphalte, and afterwards covered the wire with coarse tape so as to form a compound cable. I am now experimenting further on that principle, which I think promises to be very useful. The danger to be avoided is having the asphalte so warm as to destroy the centricity of the conducting wire.

3581. Have you pursued that system for underground wires only?—For underground wires, not for submarine cables.

3582. Would not there be great difficulty in getting at any wire in which there was a fault if you wished to take up the line?—It would be necessary probably to destroy all the wires in endeavouring to repair any one at that particular spot. I should mention that the asphalte I have used has always been partially fluid; even when quite cold it is never brittle.

3583. How do you prepare the asphalte?—I softened it either by grease, oil, or Stockholm tar, till it has the proper consistency.

3584. (*Professor Wheatstone.*) The French plan is to place naked wires in asphalte, keeping them about an inch apart. All the street work in Paris is done in that way?—There is a long account of that system in the "Annales Télégraphique," which gives me the impression that they have not yet found it perfect. I have endeavoured to adopt that plan myself in this country, but I find its expense very considerable. I was not successful in the experiments, and I am waiting to see the result of their experiments in Paris.

3585. (*Chairman.*) Would it not be a much cheaper mode of laying the wires?—Not so much cheaper as you may suppose. I think my own plan, which consists in using wires thinly covered with some insulator,

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such as gutta-percha or india-rubber, is preferable to the French plan of using naked wires, because it dispenses with the necessity for keeping them so scrupulously apart.

3586. (*Professor Wheatstone.*) It was found that at half an inch apart they did not succeed, but they succeeded perfectly if they were placed about an inch apart?—The quantity of asphalte required to place them an inch apart would be more costly than the price of ordinary gutta-percha covered wire.

3587. (*Chairman.*) Can you give the Committee any further information upon the subject of the failure of gutta-percha in land lines?—No, nothing strikes me at present.

3588. What is your opinion of india-rubber as an insulator?—It stands very high indeed as an insulator, and its specific inductive capacity is very low; therefore as a material it is peculiarly well adapted for use in submarine cables, and it has the great advantage that high temperature does not injure it. I have seen many pieces of it in a state of decomposition from a cause which at present I do not understand. By some it is attributed to the action of the copper, by some to the use of greasy wire in the manufacture, and by others to the use of an inferior india-rubber. I have had specimens which were laid down by my brother, Mr. Edwin Clark, in 1850, in several tunnels on the Lancashire and Yorkshire and North-Western railways. I have also examined specimens which were laid in the Box tunnel long prior to that time. All those specimens have cotton placed round the copper wire before the india-rubber is placed on it, and I find that no particular decay has taken place in these wires, and they give me the impression that india-rubber will eventually prove to be a highly permanent material.

3589. Provided it is separated from the copper wire by some other material?—Yes. I have also seen specimens which Mr. Siemens has brought from St. Petersburgh, in which the india-rubber was in contact with the copper wire without any effect of the kind I have spoken of having taken place. I am compelled to suspend my judgment as to the cause of that defect; if that cause can be discovered and the evil obviated, india-rubber will be found a very useful insulator indeed.

3590. (*Professor Wheatstone.*) Is it not the East India rubber which is so defective, the South American being much better. In the opinion of many persons the cure of the defect you have spoken of is to be looked for in a proper selection of the material?—Yes; I have found that the East India gum turns into the form of treacle when lying about in the office, but the bottle india-rubber, which we use for rubbing out pencil marks, never undergoes that change. I called Messrs. Silver's attention very earnestly to the defect when they first began to make public their process of covering wire with india-rubber, and they assured me that they were fully alive to the importance of using the very best gum, and that they never on any account used any but the very best Para india-rubber. But in wire manufactured by the same parties since that period I have seen that effect take place which leads me to suppose that it is not due to that cause, as their attention had been so earnestly directed to it.

3591. (*Chairman.*) Have you seen any plan which is perfectly satisfactory to you for covering wire with india-rubber?—The plan of which I should most approve is the plan adopted by Messrs. Silver, in which, after laying on the india-rubber in a spiral form, they submit it to heat. The union appears to be very perfect, and if the gum be not injured by too much mastication, I believe it will be found very permanent.

3592. Have all the specimens of wire which have been sent to you by Messrs. Silver been found perfect?—More or less perfect, and some remarkably so. I except, of course, the treacly change of which I have spoken. I think that a combination of india-rubber covered with gutta-percha would form a very excellent submarine cable.

3593. Would you put the india-rubber next the copper wire?—Yes; next the copper wire, so that in the event of an accidental elevation of temperature, there may be no danger of the wire coming to the outside, an evil that occurred to a large extent in the Atlantic cable and some other cables which I have seen. The low specific inductive capacity of india-rubber would make it a very nice material.

3594. Have you considered the properties of vulcanized india-rubber as an insulator?—I have seen a good deal of vulcanized rubber in two forms. One prepared by Mr. Daft and the other by Mr. Hooper Mr. Hooper's wire is made in several forms; one form is a perfectly elastic vulcanized india-rubber; another form is perfectly hard vulcanite; a third form is intermediate between those two, and seems to be much preferable to either of them. In all its forms its insulating power is extremely good, and its induction is as low as with india-rubber. The brittle character of vulcanite is a serious objection, because a violent kink would break it, but Mr. Hooper has endeavoured to obviate that by putting between the vulcanite and the conducting wire a stratum of india-rubber not vulcanized. I believe he has found much difficulty in arranging this.

3595. Does he put that next the wire?—Next the copper conductor; it is a stratum of simply spirally laid india-rubber. He does this in order to effect a secure joint, for at present it is generally stated that it is impossible to make a joint in vulcanized wire; but both Mr. Daft and Mr. Hooper assert that they can make perfect joints, and I have seen one joint made by Mr. Hooper in vulcanized wire, which appears to me a very complete and perfect joint. The effect of sulphur on the copper is very manifest in all the specimens which I have seen except two. One of those two is a wire prepared by Mr. Hooper in a manner which I do not understand; the other is a wire prepared by Mr. Daft, and is made of brass. Mr. Daft has observed that brass does not seem to be acted upon by sulphur in the same manner that copper is. Mr. Hooper is, at my suggestion, getting copper conductors coated with brass, in order to obtain that benefit from the use of brass. He has also, at my suggestion, tried the use of tin and zinc on the conducting wire, but I do not think they will have the effect of preventing the action of the sulphur satisfactorily.

3596. Do you expect much from vulcanized india-rubber on the whole?—I am sanguine that vulcanized india-rubber will some day be found very suitable for submarine cables, if they get over the difficulty of the action of the sulphur on the copper, and can make perfect joints. I think it is the finest material known for submarine cables; it is almost impossible to injure it by twisting or by breaking.

3597. (*Mr. Edwin Clark.*) Is not vulcanized india-rubber more liable to decomposition than pure gutta-percha or pure india-rubber?—I have seen specimens of valves, which have been used in steam engines for two or three years, in which no effect whatever has been produced by the sea-water; in the open air elastic vulcanized india-rubber seems to me very liable to decomposition. I think when it is embedded in sea-water, it will be found, as far as I am aware, very durable and permanent.

3598. (*Professor Wheatstone.*) In the course of a few years it becomes excessively rotten. I left in Paris, in 1845, a parcel of papers connected together with india-rubber bands; when I returned every one of them was rotten?—All compounds of gutta-percha and india-rubber appear to me to decay very rapidly, when exposed to the open air, especially to sunshine, when they oxidize rapidly. I have tried many experiments, by immersing them in water and various other liquids, and I find everything which keeps oxygen from them prevents any change, and I hope the sea-water will have the same effect on vulcanized india-rubber.

3599. (*Chairman.*) Have you observed that vulcanized india-rubber cuts or splits when it is brought in contact with sharp edges under water?—I have noticed the facility with which it can be split with a knife under water.

3600. Have you observed in valves that it has completely split to the core, upon the mere rectangular edges of the valve boxes?—Yes; that effect has occurred, after repeatedly binding upon the edges of the valve boxes, and I have noticed it; but even so split, it has appeared to me as strong and as good as when it was not split.

3601. Not as an insulator?—It would be useless as an insulator.

3602. Have you had any experience of Mackintosh's process of curing gutta-percha or india-rubber, by slightly vulcanizing it on the outside, so as to render it impervious to moisture?—I have seen specimens so prepared by Mr. Mackintosh, but I have not made any experiments upon them. I am not able to form an opinion upon them at present. Chloride of sulphur has the effect of vulcanizing in cold very effectively, and it is probable that it may be usefully employed in vulcanizing india-rubber cables; but I have no further knowledge of it.

3603. What is your opinion of Chatterton's compound as an insulator?—Chatterton's compound is a very high insulator indeed, and its specific inductive capacity is rather low.

3604. Is it more favourable than india-rubber?—About the same as india-rubber. I laid a cable down more than a year ago to Holland, and in order to test the use of Chatterton's compound, two of the wires were prepared with two coats of the compound, and two were perfectly plain gutta-percha; through all the testing of the various coats of wires used in that cable, I found uniformly the insulation of that wire which was prepared with Chatterton's compound was twice as good as that with plain gutta-percha. I found also that the discharge from it was about half as great; and now that the cable has been laid down, there still remains nearly the same proportion between the two wires; that prepared with Chatterton's compound has less induction and is a better insulator than that without it. In the course of experiments now being made by the Government, wire has been prepared with 10 layers of Chatterton's compound and 10 layers of gutta-percha laid on alternately, and its insulation is much higher than that of any gutta-percha that I have ever seen, and its induction very much lower.

3605. Does it rank as high as india-rubber?—Not quite so high as india-rubber, so far as my observations have at present gone.

3606. At what temperature has it been tried?—At present all our experiments have been made at ordinary temperatures; but all the wires are now having ice placed upon them, and in a few days the temperature will be 32°, and will be gradually raised up to a temperature of 95° or 96°.

3607. What is your opinion of Hughes' fluid as an insulator?—The insulation of the cables covered with Hughes' fluid is very excellent, and the induction extremely small, and therefore it seems a useful material. Its property of closing up any slight wound or defect in the gutta-percha would appear to me to be very valuable, and one very well worth consideration. If there be any truth in the belief entertained by some people, that water, under great pressure, can be forced into gutta-percha, the use of Hughes' fluid would entirely obviate that evil, because it would enter into the pores of the gutta-percha instead of water.

3608. You think it is a desirable coating for a gutta-percha covered core?—I think it well worthy of practical experiment. I have formed a very high opinion of it; it appears to me to give a very cheap cable of large diameter, very perfect insulation, and low induction. I have noticed that Hughes' fluid appears to me to improve the density of gutta-percha, and not in the least to injure it.

3609. Do you think you have had sufficient experience of Hughes' fluid to be able to form an opinion upon it?—We have had no practical experience of it; but from all I have seen I entertain a very sanguine opinion of its merits.

3610. Does it at all combine with the gutta-percha?—It does appear to combine with the gutta-percha; but in doing so it much improves its tenacity, and its insulating properties also; and therefore I think the combination will be an advantageous one rather than an injurious one. I have requested Professor Hughes to endeavour to find a fluid which will not change india-rubber; and he states that he has done so for an india-rubber covered wire; if it could be trusted in contact with any such fluid inside, a gutta-percha tube would appear to form a very nice cable; it would be necessary to be careful in making the joints to entirely separate the different portions of the cable into nodes, and not to allow free liquid communication through the whole length of the cable; at each joint I would make it of solid material. The manner in which Hughes' fluid closes accidental wounds in the gutta-percha is very striking.

3611. (*Mr. Edwin Clark.*) What is this liquid material?—It is a kind of tar, prepared in the distillation of a peculiar sort of anthracite coal or shale used in Scotland for making gas; it contains no naphtha.

3612. (*Chairman.*) What is your opinion of Mr. Allan's cable?—I do not approve of the principle.

3613. What are your objections to it?—Mechanically I think it objectionable to let the strain be transferred from the interior conducting wire to the insulator. You run great risk of crushing or injuring your insulator when any heavy strains are transferred through it to any extreme points of bearing.

3614. In paying out?—In paying out or in resting on rocks afterwards; secondly, I think electrically it is a great mistake to use such a material as steel, which has very low conducting power, while we have such an excellent conductor as copper available.

3615. Have you paid any attention to Herder's cable?—We are having specimens of Herder's cable made, in order to experiment upon them; but I do not think we shall find any useful results from them. I think the experiments on which he bases his opinions are rather fallacious.

3616. Have you examined De Berg's cable?—I have seen specimens of it only.

3617. Have you formed any opinion upon it?—I have formed rather a favourable opinion from what I have seen; it is a hemp cable, and I think it is the nicest of the parallel laid cables that I have seen. I should be afraid of it, because it is entirely a hemp cable; such a cable lying across rocks at the bottom of the sea, if it should happen to do so, would by the very slightest action of the current very soon saw itself in two, and it would be extremely likely to lie in cateraria across elevations on account of its great lightness, when a heavier cable would sink down and lie closer to the ground.

3618. Would it not be eaten by marine animals?—From the evidence I have heard given marine animals only occur at depths of 400 fathoms, at least, marine animals of such a character as could injure a cable, and down to those depths I would certainly carefully protect any cable by iron.

3619. What is your opinion of the size and proportions of the Atlantic cable?—Undoubtedly the copper conducting wire was much too small. At the early formation of the company, before it actually was a company, the projectors asked me to see some samples which they had prepared. One of them had five or seven very small wires, separated from each other in one bedding of gutta-percha. I pointed out to them how impossible it would be to use such a cable, and begged of them to use only one wire, and that a very thick one. They only partially accorded with my views, and adopted the present thin conducting wires. The experiments which led to the adoption of that small wire are well known, and I need not make any remarks upon them, because the

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error which caused that adoption is now very well understood. I think generally all experience shows that it is very advantageous to use a very thick conducting wire, and not so very thick an insulator. Speaking commercially, you gain more rapid speed by increasing the conductor than by increasing the insulator.

3620. To what error do you allude?—The error made by Mr. Whitehouse in supposing that small conductors would work more quickly than large ones.

3621. Have you made any experiments on the conductivity of copper wire?—None especially; but at the early formation of the Atlantic Telegraph Company, when it was first resolved to make the Atlantic cable, I wrote an earnest letter to the manager of the Company, Mr. Cyrus Field, and to the manager of the Gutta-percha Company's Works, Mr. Statham, begging of them to be careful not to use anything like brass wire, pointing out the very low conductivity of brass wire; I was led to do so from having accidentally wound a coil with brass wire instead of copper wire, finding it differ in an extraordinary manner from a similar coil wound with copper wire, I wrote to them earnestly to be aware of having a wire of bad conductivity; I believe it was that letter which led to the great attention which has since been paid to the subject.

3622. Have you had any experience of the great differences of the conducting power of copper wires?—I have frequently drawn pieces of copper wire taken from various cables, such as the cable laid in the Mediterranean, from our own Hague cables, and from the Atlantic cable, and compared them with the copper wire which is now used by the Gutta-percha Company, and I found an extraordinary difference in the conductivity; all our resistance coils, which were made to accord with the kind of copper then in use, are now entirely wrong, owing to the use of greatly improved copper by the Gutta-percha Company. But some specimens of copper which I received from the Gutta-percha Company as late as three weeks ago gave a conductivity of only 75 per cent., when pure copper would have given 100, so that even now their wire is not all uniformly good.

3623. Have you a standard for the conductivity of copper used by the Electric and International Company?—We have no standard in that Company, but Dr. Matthesou has lately been making some experiments on the various qualities of copper, and he has supplied me with a standard coil of pure copper wire, on which I now base the calculation I have made as to the conductivity of the Gutta-percha Company's wire at present.

3624. Have you made any experiments on the effect of temperature on the insulation of cables?—None, except that I have noticed generally how rapidly the insulation falls as the temperature rises, and in laying out submarine cables, in common with every one else, I have always noticed how strikingly the insulation improves as the cable goes out from the warm ship into the cooler sea.

3625. Have you studied the effect of different thicknesses of coating on submarine cables?—The induction between flat plates is known to vary directly as the distance between those plates, and, I believe, in submarine cables the same law prevails, but in measuring the spaces you must not take the surface of the conducting wire or the surface of the gutta-percha, but the mean of those two surfaces, and then dividing that by the thickness of the dialectric, you get, I believe, the law which rules that phenomenon; if you call C the circumference of the outer dialectric, a little c the circumference of the conductor, then if you add those two together, take half the difference, and divide that by T (the gutta-percha), which we will suppose to be the thickness of the dialectric, it gives you the law. The formula may be thus expressed:—

$$I = \frac{\frac{C+c}{2}}{T}$$

All these measurements, I think, are very much interfered with by that phenomenon which Faraday called penetration, and which Snow Harris called absorption, and which is so well known under the name of residual discharge in Leyden phials. I am endeavouring earnestly to ascertain the law of this effect, which seems a very interesting one.

3626. Does not it vary very much in different substances?—Yes, in the 20-coated cable to which I have previously alluded, it exists to an extraordinary degree; we find, if we charge that cable for a minute or two, and then at intervals of a minute, keeping placing it in contact with our galvanometer, it will continue to give about a degree or a couple of degrees of deflection for the space of 30 or 40 minutes, or even longer, continually creeping out a new charge every time. Other materials appear to have very little of that property, I believe. Advantage may be taken of that to find some suitable material.

3627. Has Chatterton's compound that property to a large degree?—Our experience has not yet informed us of that. I am carrying out a series of experiments with flat plates, by the use of frictional electricity, and tabulating all the materials.

3628. (*Mr. Edwin Clark.*) Have you tried sealing wax?—No. Shellac and bees-wax has it in a high degree, india-rubber in a low degree, and vulcanized india-rubber in a low degree. That effect does not at all agree with that of induction or insulation, and it forms quite a separate law of its own.

3629. (*Chairman.*) Have you noticed the effect of what are called false currents in coiled submarine cables?—Yes, very frequently; the effect is very apparent in cables coiled in small diameter. I have seen it in a coil of 20 feet diameter; but it is not at all apparent in large coils; nor do I think it a phenomenon of any importance in telegraphy, except while experimenting before the cables are laid down; it is the result of the action of one coil upon another, as originally pointed out by Faraday. It is very easily shown on small coils; but I think it is of no importance.

3630. Have you made experiments on the effect of earth currents in working through telegraph lines?—I have very often witnessed them, and have taken much interest in them. I have devised a machine for the purpose of balancing that force and counteracting the effects. The machine consists of any ordinary magnet or needle, which is surrounded by coils of thick wire in connexion with a single cell of battery. A handle moves horizontally on a table, and when that handle is in a central position it makes no contact with this cell at all; when it moves to the right hand, it causes this cell to work through the thick wire coil in one direction; and when it is moved to the left hand, it causes it to work through the thick wire coil in the other direction. If you move it further and further to the right hand or to the left hand, it throws into circuit a gradually increasing resistance coil, so that when it is worked to the extreme right it allows the current to work through the thick wire on short circuit, or when it is moved to the extreme left it allows the current to work on short circuit; when brought nearer and nearer to the central position the current has very little resistance from the small wire. In that manner you get the means of truly balancing the changing force of earth currents as they arise. I have noticed some extraordinarily violent effects at different times from earth currents; but they have never been so excessive as they were during 1859. We have even had them at such a pitch of high intensity that they caused visible sparks to pop between the terminals when touched.

3631. (*Mr. Edwin Clark.*) Have you applied this instrument?—Not yet, but I have one ready.

3632. (*Chairman.*) What is the strength of the earth currents as compared with battery power?—I have never myself been able to measure one that would overpower a stronger battery than one of 36 cells; but Mr. Varley has measured one as high as

70 cells; and on the occasion I speak of, when visible sparks occurred, which they did abundantly, both in this country and on the continent, the tension must have been equal to the power of many hundreds of cells.

3633. Is the weather peculiar in which earth currents are observed?—They are invariably accompanied by the aurora borealis. I have not traced out any law concerning them, but I have made preparations for measuring their intensity by the use of a Peltier electrometer the next time they occur.

3634. Is there any peculiar season of the year when they are observable?—Only so far as they depend upon the aurora borealis.

3635. (*Mr. Edwin Clark.*) Are earth currents uniform in their direction, or is there any general law concerning their direction?—I can trace no law as to their direction; at times they are most powerful east and west, at other times north and south; in five minutes they frequently change their direction to the right or to the left.

3636. (*Professor Wheatstone.*) The 28th of August was the great day on which almost all the telegraphs were stopped, not only in Europe as far as Rome and Naples, but also in Australia?—Yes.

3637. I believe a short line which goes from the House of Lords never was affected?—I have seen numberless instances of a line two miles in length being powerfully affected by these currents. On the occasion of which you have spoken, the short lines under the streets of London were powerfully affected by these currents.

3638. (*Chairman.*) Are the north and south lines affected more than east and west lines?—No, one storm will have its direction north and south, and another east and west.

3639. (*Professor Wheatstone.*) The inspector of the French telegraph says, that their lines were much more affected east and west, but that does not appear to hold good in this country?—No; I have urged on the Astronomer Royal the importance of the Government laying two or three wires radiating from the Royal Observatory, with copper plates at each end. I have pointed out that three wires will virtually give six points of the compass, four wires will give eight points, and by combining their effects you will get sixteen points of the compass. I believe on a line of 10 or 12 miles the effects of earth currents would be visible daily. I think that many of the effects that are now observed by means of a suspended magnet are really due to the passage of these currents. I have frequently written to Professor Airey to tell him that we had violent deflections of our needle, and he has invariably replied that he also has had violent deflections of his magnet.

3640. (*Chairman.*) Have you made any experiments on the comparative absorption of electro-magnetic currents and battery currents?—I have not had an opportunity of doing so, but I believe they are essentially similar in their character, and that any magnetic current may be insulated by a voltaic battery; I have charged a cable with an electrifying machine, raising it up to the same tension that would be given by a battery of, say, 100 pairs of plates, and I have found that the discharge from that cable is exactly the same in its force and character as that from 100 cells of voltaic battery; so that I believe there is no difference between frictional electricity and magnetic electricity or voltaic electricity.

3641. Are you acquainted with Professor Hughes's machine for printing in type through cables?—Yes.

3642. What is your opinion of it?—I think it may turn out a useful machine, it has the great advantage that it prints one sign with every wave, or with every current.

3643. What is the principle of its construction?—The principle of its construction is this: he keeps one wheel revolving at a certain rate and another wheel at the distant station revolving at the same rate carrying types; every time he sends a current from the rear wheel he arrests the distant wheel for a moment and causes it to print the character which is passing at that instant. In order to keep these wheels revolving synchronously, he employs an ingenious method of causing a tooth to descend, so that if his distant wheel has got a little in advance or is a little behind his own wheel, this tooth in its descent not only prints a letter but brings back his wheel to the right point, and in that way, practically by using a spring pendulum to control the velocity of his wheels, he can print for many hours without an error; I have seen him work for several days between London and Liverpool with that machine, though not without errors, still the errors were so few as to lead me to hope that it might give very good results. I have seen a letter from one of the companies in America which employs his machine very extensively, in which they speak in the very highest terms of its operation.

3644. At what speed will it work?—I have usually seen it work about 20 or 25 words a minute, but the letters I have seen from the telegraph companies in America state that they frequently work 35 words a minute and sometimes even 40.

3645. (*Professor Wheatstone.*) Is Hughes's machine superior in its action to the printing telegraph invented by M. Tayleur?—I think it is very superior to Tayleur's. Tayleur was the first, so far as I know, to employ the spring pendulum as the controlling power for the speed; but that machine rested at zero. It then automatically sends one current, which sets the machine in action, and the second current is given afterwards, which arrests the wheel at the moment the right letter is passing; after having printed it, the wheel revolves again to zero, and stands there until you send another current. In Hughes's machine the type wheel revolves perpetually; in fact, it is fixed to a heavy fly-wheel, and he does not arrest the type wheel at all; but he causes the paper to be brought up by a small pad in contact with the type wheel in running motion when that particular letter is passing the pad. He also makes very clever use of the auxiliary current to adjust the tension of the cable between the intervals when he is sending positive and negative currents. For example, if he has been sending a long negative current, he sends a momentary positive current automatically before the next current is sent, in the manner introduced by Mr. Varley. Also, at the further end of the cable he has a great gain, for if the cable has been receiving a negative current, he automatically, for an instant, puts a positive current into the cable, which sweeps out all the negative current from that half of the cable which would be some time clearing itself spontaneously at the near end; so that he assists the cable in clearing itself of the previous current automatically at each end.

3646. Has Hughes's instrument ever been tried on a submarine cable?—Not on a greater length than 240 miles; on that length it worked extremely well and very rapidly.

3647. Have you no doubt of its success on a long submarine cable?—I could not speak so confidently as to say that I have no doubt of its success, but I am very hopeful of its being found a most useful and rapid machine. Professor Hughes's machine acts, I think, with an almost weaker current than any other machine I know; he employs an electro-magnet, which is magnetized by a permanent magnet; the armature is down nearly in contact with the electro-magnet, which is a spring of so much force that it will almost overpower the permanent magnet; under these circumstances the feeblest current sent through causes the machine to print. I have seen Hughes's machine work when an ordinary galvanometer would give no indication.

3648. Will it work with as delicate a current as Professor Thomson's marine galvanometer?—I should say more, from what I have seen.

3649. With a more delicate current?—I am not able to say; it is a most instantaneous machine.

3650. (*Mr. Edwin Clark.*) Would not the inductive effect on cables of 2,000 miles be quite fatal to it?

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—No, it will act much slower. You must allow a certain margin of time for the current to have its effect on the magnet; it would act to the same degree faster than an ordinary machine as it would on a short cable. Of course it would not act so fast on the long cable, because you must allow a much greater margin for the current to make itself appear distinctly positive or negative.

3561. (*Chairman.*) Is there not great difficulty in causing the magnet to remain in the nearest position to the armature, or the armature in the nearest position to the magnet, if there is the least residual current in the cable?—Professor Hughes employs the residual current as one of the forces which keeps the armature down to the magnet, and it is the feeble current destroying this residual magnetism that really works his magnet; he really works on the residual current. It is the doing away with this residual magnetism which causes the magnet to fall back, and the machine to print. From the observations I have been able to make of the working of the machine, I am much more favourably impressed with it than I was disposed to be when I just examined it as a machine to be used in connexion with submarine cables.

3562. (*Mr. Edwin Clark.*) Does Professor Hughes's patent belong to the Electric Telegraph Company?—No, it does not.

3563. To himself alone?—I believe to himself alone.

Adjourned.

Friday, 9th March 1860.

PRESENT:

Captain Douglas Galton.
The Right Hon. J. Stuart Wortley.
Professor Wheatstone.
Mr. Edwin Clark.
Mr. Varley.

Captain DOUGLAS GALTON IN THE CHAIR.

Captain J. Washington, R.N., F.R.S.
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Captain John Washington, R.N., F.R.S., examined.

3564. (*Chairman.*) You hold the office of Hydrographer to the Admiralty?—I do.

3565. I believe you have paid considerable attention to the subject of deep sea sounding, and to the physical geography of the sea?—From the position I hold as Hydrographer to the Admiralty, all those soundings have been under my immediate direction.

3566. The Committee wish to obtain your opinion upon the relative practicability of three routes for laying submarine cables between the British Isles and America. The first is the route from Ireland or Scotland, by Iceland, Greenland and Labrador; the second is the route which the Atlantic Telegraph Company have taken; and the third the route by the coast of Spain, the Cape de Verde Islands, the northern coast of South America, and thence by way of the West India Islands to the United States. Will you first give your opinion upon the northern of those routes?—The route from Scotland by the Faroe Islands, to Iceland, Cape Farewell and Labrador, from the convenient length of the different steps looks very tempting; and were there no such difficulties to encounter, as ice and rapid currents, I should be disposed to recommend it. But looking to the knowledge which we have, confessedly rather imperfect, of those countries my present opinion is, that it is quite impracticable.

3567. Do you think there would be any difficulty with respect to the first step in that route to the Faroe Islands?—Not the least difficulty.

3568. Is there any ice found near there?—Probably not of any consequence.

3569. On the south coast of Iceland is there any ice?—On the south coast of Iceland you would encounter ice.

3660. Does the ice hang upon the south coast of Iceland?—No; during the summer the ice is away.

3661. Is there not a long ridge of basalt which runs out to some distance from the south coast of Iceland, which might be injurious to a cable laid upon it from the attrition?—Probably there is, but I am not immediately aware of it; by consulting our largest charts we shall be able to ascertain the fact.

3662. From Iceland to Labrador, would there be any strong current to encounter?—Not a strong current, I think, but there would be a great deal of ice; the east coast of Greenland is blocked up with ice, comparatively speaking.

3663. Both summer and winter?—Both summer and winter, the greater part of it.

3664. Is not it the case that there is always a considerable amount of pack ice on the south point of Greenland, and on the western coast?—I do not think the ice remains in the south-western fiords of Greenland during the summer. I have no practical experience, but from recollection it does not. You must have had much better evidence upon that subject from Sir James Ross and other Arctic navigators who can speak to the point; but my impression is, that the whole of the fiords are clear during the summer, and are blocked up during the winter.

3665. Would it be your opinion that the prevalence of ice during the winter along that coast would be injurious to a cable after it was laid?—Yes.

3666. From the grounding of the ice, I suppose?—Yes.

3667. The next step in that route is between Greenland and Labrador, how far do you think that would be practicable?—I do not think there would be any difficulty beyond, as I said before, partly the ice and partly the currents. We do not know the maximum depth of water that I am aware in Davis's Straits, but I do not contemplate that it exceeds 1,500 fathoms, and that is of no great importance.

3668. In none of those steps do you think that the depths of water would be any great difficulty?—I think not as far as we know; but the Committee will be well aware that we have very incomplete soundings indeed in that direction; in fact, I may say, that we are quite ignorant of the whole affair, and if it were proposed to lay a cable, the first step would be to sound it thoroughly; and without that being done, no opinion worth having could be offered in my judgment.

3669. We will now go the Atlantic Company's route. Of that I believe you have very complete soundings?—From the coast of Ireland to Newfoundland we have a complete line of soundings, and as far as my information extends at present, we know of no better line than that. If you will allow me, I should like to remark with respect to the former route to Cape Farewell, I think Sir James Ross stated the other day before the Committee, that he had made some soundings, which were deposited at the Admiralty. I have before me the chart on which those soundings are laid down, but in no case was bottom reached; and, therefore, I do not think the soundings are of any very great value; at any rate, they are not sufficient to do away with the necessity of thoroughly

sounding out the line in case the laying of a cable in that direction were likely to be attempted.

3670. (*Mr. Stuart Wortley.*) Do you know the greatest depth of soundings attained by Sir James Ross in that quarter ?—2,000 fathoms of line, and no bottom in one case ; that is about half way between Rockall and Cape Farewell. The others were chiefly 1,000 fathoms of line, but no bottom.

3671. (*Chairman.*) Do you know the nature of the bottom generally in those parts to which Mr. Stuart Wortley is alluding ?—The bottom was not reached in any case.

3672. (*Mr. Stuart Wortley.*) Then you have no knowledge of the nature of the bottom ?—Not the slightest. I have heard a report that a United States vessel has sounded the coast of Greenland, but I have never seen the results of the soundings.

3673. (*Chairman.*) Was that Colonel Shaffner's vessel ?—I believe it was.

3674. You have complete records of the bottom between Ireland and Newfoundland, I believe ?—Complete, and in no case is there any exception to the bottom being ooze ; therefore it is fair to presume that the bottom extending to Iceland and Greenland would be the same.

3675. The ooze, I believe, is small shells ?—Soft mud and very small comminuted shells ; so small as to be almost invisible.

3676. (*Mr. Stuart Wortley.*) There is a very sudden dip, is not there, about 200 miles from the coast of Ireland ?—There is a dip of 1,000 fathoms at 200 miles from the coast of Ireland.

3677. (*Chairman.*) In what length would that dip be ; it is almost impossible to say, I suppose ?—Yes. There is a section from which you may infer it.

3678. Were the soundings taken very near to each other at that part ?—The soundings were not taken to within a few miles of each other.

3679. (*Mr. Stuart Wortley.*) Do you mean a few miles longitudinally, or latitudinally ?—Longitudinally.

3680. Have there been any soundings, if I may so express it, latitudinally on the one side and the other of the Atlantic line ?—No.

3681. Would it not be advisable, before any new attempt were made, to see whether the dip is equally abrupt to the north and to the south ?—That might be done, but it would be a very troublesome affair.

3682. (*Chairman.*) That would be best done, I suppose, by means of three ships ?—Yes ; if it is to be done expeditiously it must be done in three ships. But I question whether the results would be worth the trouble. The only use of three ships would be to do it quickly ; they would not be in sight of each other. The lines must be taken out, I should say, 100 miles apart to get a result of any consequence. If you look at the chart of the Atlantic ocean you will see that the rock of Rockall lies about the same distance from the main that the dip on the Atlantic line lies. Now it is possible that the tail of the bank on which Rockall is situated might afford shallower water, but it would be going too much out of the straight line, and I do not think it would be at all worth the trouble. It would be going north-west, and then again south-west to get into the line, which would increase the distance considerably, and not be worth the trouble, because the deep water must be reached at last. When you speak of that sudden dip on the coast of Ireland, it appears sudden on all the sections that we have, but those sections are extremely exaggerated. But really that dip is not a bit steeper than any of our common roads. I do not think it is so steep as the road up Shooter's Hill.

3683. (*Mr. Stuart Wortley.*) The soundings show the dip, do not they ?—The soundings show it, allowing for the interval. There are no exact soundings on the spot to demonstrate it ; but, judging from the interval from one deep sounding to another deep sounding, the slope would not be very much more than the slope of Shooter's Hill.

3684. (*Chairman.*) You do not think there is any probability of its going down in a sheer precipice ?—Not the slightest.

3685. Then the detritus would have filled up anything of that sort ?—Yes. In fact there are no such abrupt cliffs in the bottom of the sea.

3686. (*Mr. Stuart Wortley.*) Have we knowledge enough of the bottom to ascertain whether that slope consists of a gradual slope or a jagged one ?—We have no means of ascertaining that ; it is extremely improbable, the bottom being ooze, that the slope should be anything but a gradual one. Ooze could not stand in abrupt cliffs in the same way as rock.

3687. (*Chairman.*) What appears in the soundings ?—In the soundings, at 550 fathoms, it is marked "rock or stone."

3688. From that you can bring up nothing ?—Merely the impression of the stone. I do not suppose that any stone was brought up actually.

3689. (*Mr. Stuart Wortley.*) Between the soundings at 550 fathoms, which is rock, and 1,750 fathoms, which is marked "shells," what distance would there be ?—My impression is that it would be 20 miles.

3690. (*Chairman.*) Do you think it would be desirable, supposing any further attempt were made to lay a cable there, to ascertain more accurately the character of the bottom ?—No ; I do not consider it at all necessary.

3691. You have entirefaith in those soundings ?—No soundings could be more carefully made than Commander Dayman's soundings were made. We have not a more careful man in Her Majesty's service.

3692. Even at that point you would not wish to have more soundings taken to ascertain whether there is any precipice ?—As a matter of curiosity it might be done.

3693. (*Mr. Stuart Wortley.*) Would it not be easy to take soundings between 550 and 1,750 fathoms where the rock changes to ooze, to ascertain the nature of the soil between those two points ?—There is not the slightest difficulty. It could be done in a very short time. A week of fine weather would settle the whole question upon that point ; and I think that it would be desirable to expend a week's labour upon it, if it were only a matter of curiosity ; but I do not think it would make any difference in the laying of the cable.

3694. If the whole of that interval between 550 fathoms and 1,750 fathoms turned out to be rocky bottom there might then be a question whether the whole of that part of the cable should not be stronger than that which is to be laid where there is ooze ?—It might be so, though it is extremely improbable.

3695. From that point where the dip occurs, across very nearly to the coast of Newfoundland, there lies what has been called Lieutenant Maury's plateau ?—Yes.

3696. That is, speaking generally, ascertained to be level, is it not ?—Speaking generally, it is so.

3697. Is it at all ascertained to what depth that level extends north and south ?—Speaking very generally, there are variations of 1,000 fathoms ; that is to say, a variation of nearly one-half of the whole depth of Lieutenant Maury's plateau.

3698. Does the plateau vary to that extent in several places or in only one ?—In one place.

3699. We do not know what the breadth of that plateau is, if it is to be called a plateau ?—No ; the word "plateau," I think, is misleading. I think it would be better to reject it, as Lieutenant Maury has done.

3700. Maury's line, we will call it ; we do not know the breadth of the line of soft bottom ?—No. In fact, we have a solitary line across the Atlantic ; it might be very desirable to run other lines, if there were any proposal for going further with the subject.

3701. I suppose there is no other view, with reference to the naval establishment of this country, which would render it desirable for the Government to make those further researches except for the sake of telegraphy ?—No, I think not ; so far as the science

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of navigation is concerned, there would be no use in it, but it is always a matter of legitimate curiosity to ascertain the physical features of the globe, as well under the waters as above them.

3702. Does the employment of Her Majesty's vessels in that kind of survey involve any great additional expense?—No great additional expense; very little more than the cost of the sounding line.

3703. Is not the taking of soundings a very good school for seamen and young officers?—Extremely good; and I may add that other nations are taking the initiative in this work. I believe the United States, if they have not already done it, are about to sound a line across the Pacific; they have sounded, I believe, a line from the coast of California to the Sandwich Islands; and I have heard also that they have continued a line on to Japan.

3704. Do you mean the Government of the United States?—Yes.

3705. Is the Government of the United States in the habit of publishing their soundings, or do they keep them secret?—They have published all the soundings taken in the Atlantic very liberally indeed; they were published at the expense of the Government, and the work has been distributed to all the public bodies in England.

3706. (*Chairman.*) Will you now give the Committee your opinion upon the southern of the three routes I have mentioned, which has the advantage of enabling the cable to be laid in much shorter lengths than the Atlantic route. The immediate route would be from some point on the coast of England or Ireland to Cape Finisterre, or near Cape Finisterre, passing down the coast of Spain, probably calling at Lisbon, Cape St. Vincent, and Gibraltar; thence passing from Cape St. Vincent to the Canary Islands, and thence to the Cape de Verd Islands; thence to St. Paul, and thence across either direct to the northern coast of South America, or to some island near that coast?—In the first step, as the Committee will be aware, between Ireland and Cape Finisterre, the maximum depth which we have ascertained is 2,600 fathoms.

3707. The bottom there is tolerably favourable, I believe?—The Committee have the soundings; mud and sand in all the cases in which the bottom is marked.

3708. Along the coast of Spain the bottom is equally favourable, I believe?—Yes; wherever marked, it is mud and ooze.

3709. In some parts it is marked "coral;" is that coral broken up?—It is all broken up.

3710. What information have you beyond Cape St. Vincent towards the Canary Islands?—None at all; not a single sounding.

3711. (*Mr. Stuart Wortley.*) What would be the distance from Gibraltar to the Canary Islands?—About 600 miles; from the Canary Islands to Cape de Verd, about 600 more; from Cape de Verd to St. Paul, 800 or 900 miles; and from St. Paul to the coast of America, about 1,200 miles.

3712. (*Chairman.*) On what parts of that route have you any information?—I am not aware that we have any soundings except immediately in the vicinity of the Cape de Verd Islands and approaching to St. Paul. We have soundings of 2,000 fathoms within 50 miles of the Cape de Verd Islands.

3713. (*Mr. Stuart Wortley.*) What is the nature of the bottom?—It is not marked.

3714. (*Chairman.*) Do you believe that there is any very great depth beyond 2,000 or 3,000 fathoms in that part of the Atlantic?—I do not think there is anything exceeding 2,500 fathoms in any part of it.

3715. (*Mr. Stuart Wortley.*) Has it been surveyed across?—No line has been sounded, but both the Canary Islands and the Cape de Verd Islands have been surveyed as groups of islands; but no connexion between them till we approach St. Paul, and there we have 2,000 fathoms and 1,000 fathoms at about equal distances up to the island of St. Paul.

3716. The Canaries are Portuguese, are not they?—The Canaries are Spanish.

3717. The Cape de Verd?—They are Spanish also.

3718. And St. Paul?—St. Paul, I believe, is Portuguese, if it belongs to any one; it is a mere rock.

3719. (*Chairman.*) Are you aware whether any impediments are likely to be met with in the way of volcanic islands?—I am not aware, but I think that it is not at all impossible.

3720. In the neighbourhoods both of the Cape de Verds and the Canaries?—Yes, it is quite possible.

3721. (*Mr. Stuart Wortley.*) Do you consider the line to which you have been referring within South America?—No; St. Paul's is exactly on the equator.

3722. Where do the trade winds first begin?—The north-east trade wind is always found in the latitude of 30°, say off Madeira.

3723. On that line you would have the advantage, generally speaking, of fine weather all the year round?—Yes, there is no doubt of that; there would be no difficulty in sounding that line.

3724. (*Chairman.*) And no difficulty in laying a cable?—No difficulty; I think the north-easterly trade wind is very fresh and strong at times, but it is a fair wind. I do not know that that is in favour of laying the cable; still it would be a fair wind, and, perhaps, rather against laying it.

3725. (*Mr. Stuart Wortley.*) It is an equal wind?—Yes; I think a wind ahead is rather better than a fair wind; I should prefer it in laying a cable if it were not very strong.

3726. It would not be liable to storms?—Not liable to storms certainly, but there is a strong fresh breeze and a fair amount of sea.

3727. When you approached the north coast of South America or the West Indies you would be liable to hurricanes, would you not?—Not so far south as you are going; no hurricanes extend there, I think, the hurricanes confine themselves to the West Indies, and all the latter part of the line would be laid in the region of calms, where you may ensure fine weather all the year round.

3728. You would avoid also the Gulf stream?—You would avoid the Gulf stream partly, but there is what are called the equinoctial currents setting in to the westward slightly; nothing of any importance. It might be desirable to go to St. Paul's, but it is making a very large angle if you went to the coast of South America, unless you went far south of the coast of South America.

3729. (*Chairman.*) Do you think there would be no greater depth between the Cape de Verd Islands and the coast of South America than there would be between the Cape de Verd Islands and St. Paul?—No, I think not; tho only sounding we have is 2,700 fathoms by Lieutenant Lee, of the United States' navy; I do not think it would exceed that.

3730. There is another route to which I have alluded, namely the route from England to the Azores, and from the Azores to the coast of North America; I observe that the route between England and the Azores has been sounded?—It has been sounded very carefully, and we know the greatest depth to be 2,375 fathoms.

3731. Have you any information as to the soundings between the Azores and the coast of North America?—Yes, for about 300 miles to the west of the Azores, where we have also 2,800 fathoms.

3732. (*Mr. Stuart Wortley.*) And 3,000 fathoms in one part?—No, there is no bottom there. There is a sounding of 3,000 fathoms and of 3,700 fathoms without bottom, but it does not follow that there would not be bottom.

3733. Between England and the Azores there is bottom all the way, is there not?—We ascertained the bottom at no greater depth than 2,500 fathoms; and generally speaking the bottom was ooze wherever it has been brought up.

3734. (*Chairman.*) Have you any information as to the soundings between the Azores and Bermuda?—

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Nothing further than I have mentioned, about 300 miles to the westward of the Azores; but the general belief is that that is about the deepest water in the Atlantic ocean; I do not know that there is any real foundation for it. It is the general supposition that a deep gully extends from Nova Scotia in a south-easterly direction from 3,500 to 4,000 fathoms deep.

3735. Is there a limit to the depth at which soundings can be taken with any degree of certainty?—I have no doubt that there is a limit; but as far as we have gone yet, I think there would be no difficulty in sounding in 4,000 or even 5,000 fathoms.

3736. But we have no soundings taken in that depth as yet?—Yes; if I remember, Sir James Ross made a sounding in deeper water, but I am not quite sure of its established accuracy; he believed it to have been accurate, but there are doubts, seeing that it was made with spun yarn, which we believe to be a very inefficient mode of taking soundings. We have almost reduced sounding to a system now. I have here an abstract of all the soundings that have been made, showing the line employed, the weight used, the mean time of sounding from 400 to 2,000 fathoms, and the extreme range of the intervals of time. Now the result of that table shows that using seine twine, which some persons advocate extremely, and an iron weight, we find that it occupied 36 minutes in sinking through 1,000 fathoms, that is from 400 to 1,400. Using albacore, which is nearly the thickness of a lead pencil, with a weight of nearly 2 cwt., we got the same depth in less than half the time, in 17 minutes. Now we assume that the more rapidly we can get the line down, the better for our purpose, therefore I may say there is no difficulty in deep sounding with a proper line and a proper weight. It involves the sacrifice of the weight and the line, but that is of very little importance compared with the accuracy of the sounding.

3737. Do you sacrifice the whole of the line?—No, part of it; we try to haul it up, but when it breaks we put up with the loss.

3738. Does the weight become detached on reaching the bottom?—Not in those experiments that I speak of, nor do I recommend it if accuracy is to be insured.

3739. What is albacore made of?—Hemp; it is merely rather stronger than the common sort of cod fishing line; it is commonly used in our dockyards, and has gone by that name; we can make any length of it, owing to the vertical machine used by Newall of Gateshead, which is a great advantage, because we get rid of the joints and the friction. The great object in sounding is to have the smoothest possible line on the reel, so that it will run freely, and as heavy a weight as it will bear, to make it run down as rapidly as possible, but sufficiently strong to allow it to be hauled up from the ship or boat, so as to ensure its being up and down. With care it can be done in a ship, and when the line is strictly up and down, we get the very best sounding; then we break the line and care nothing for it. If we wish to ascertain the bottom at any place, that is done by a separate operation, and we do not care about the depth; we take first a separate sounding for the depth. It is undesirable to try to do too much to bring up the bottom and ascertain the depth.

3740. Is the friction of the line sufficient to break it in coming up from the bottom, or do you manage to bring up a portion of the bottom?—It is not the friction, it is the weight added to the friction. When we want to ascertain the bottom, we care nothing about the depth; the line runs out, and we let it take its course. With a lighter weight it takes more time to go down, and it is more troublesome. We use a stronger line for the purpose of ascertaining the bottom, and bring it up. We make separate experiments for bringing up the bottom, and ascertaining the soundings accurately. The result of our experience is that that is the best course to pursue.

3741. (*Mr. Stuart Wortley.*) You take the depth and the nature of the bottom by separate operations?—Yes, by separate operations.

3742. Are not the Azores proverbially volcanic?—Yes; proverbially. It is notorious that off the Azores an island has appeared and disappeared. I should think it very undesirable, therefore, to select that route.

3743. Would not these soundings, if they were revised, vary from the last soundings?—Possibly in the immediate vicinity, but nothing material at a distance. I would strongly deprecate laying an electric cable in the vicinity of a known volcanic region.

3744. Can you tell the Committee the length of the leap from the Azores to Bermuda?—1,800 nautical miles, in round numbers.

3745. (*Mr. Varley.*) Is not the island of St. Paul volcanic?—Yes. I was asked the question whether I would go to St. Paul's. I would not recommend it as a step. On the contrary, I was about to say, that you not only make a very large angle, but you go into a volcanic region, which is undesirable.

3746. Is not Teneriffe volcanic?—Yes; the whole of this group of Islands. One of the Cape de Verd islands has been named by the Portuguese Fogo, or "Fire," evidently showing what it is.

3747. (*Chairman.*) Then of all the routes to America, the one which you think best adapted for the purpose of laying a submarine cable is the Atlantic Telegraph Company's route?—By far the best is that which has already been selected by the Atlantic Telegraph Company.

3748. (*Mr. Varley.*) In some of the old charts an island is shown a little to the south of the present Atlantic cable, about 500 or 600 miles from the coast of Newfoundland. Do you think that that island, which is stated to be very small, really exists, or that it is a mistake?—Certainly it does not exist. I observed, in a report made to the Atlantic Telegraph Company, as to picking up the cable in Bull's Arm Bay, on the coast of Newfoundland, it was stated that the cable had been very much injured by the rocky bottom. I think there must be some mistake about that. On seeing it in the paper, I sent for the original chart, on which the greatest portion of the cable is carefully laid down, and there nothing like rock appears. The soundings were made by Captain Otter very carefully, and I have no reason to doubt them.

3749. (*Mr. Stuart Wortley.*) The statement of Captain Kell to the Atlantic Telegraph Company is not that the bottom is continuously rocky, but that in the course of the 10 miles he picked up there were portions of the bottom that were rocky, and others that were soft ooze and sand. Wherever he found that there were rocks, the cable was greatly injured by friction or by chemical action, and wherever he found sand or ooze, the cable was as good as when it was first laid down. So that it does not necessarily imply that there is a continuous rocky bottom there, but that there are ribs of rock crossing the bottom at various points?—It is possible, but it is not shown in the soundings, as far as I recollect. I observe that there are stones marked in two or three places,—"stones and shells." There is "rock," I see marked in one place. I think, too, it was said that Bull's Arm Bay was not the best spot for laying the cable. Now, I must be permitted to say, that Bull's Arm Bay is a very sheltered spot, entirely free from ice; icebergs cannot get into it, whereas, if the line were carried in any part of the open coast of Newfoundland, which I think was recommended, there would be great danger of its being interfered with by icebergs.

3750. The suggestion made by Captain Kell, who examined the coast in the month of October, was that the cable should be taken to a place called New Perlicombe, which is not on the open coast of Newfoundland, but nearly opposite St. John's?—If any other equally sheltered spot can be found, I have no doubt it will do as well.

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3751. Do you think the Government would be disposed to make any soundings to the north and to the south of the spot where the Atlantic cable is laid to great depths ?—I am satisfied that the Government is so disposed to do everything in their power to advance telegraphy, that I am certain it would be granted.

3752. (*Chairman.*) Can you ascertain at all in sounding whether there is a very small depth of mud upon a rock or not ?—No ; we get merely the superficial character of the soil. We might send an iron spear possibly to the bottom, to a depth of one foot, if that would be of any service.

3753. Generally speaking, the soundings are surface soundings ?—Yes ; surface soundings generally.

3754. (*Mr. Stuart Wortley.*) Although soundings may have been taken which show that there is a soft bottom in Trinity Bay or Bull's Arm Bay, there may be rock a very short distance under the ooze ?—There may.

3755. If the cable lay there for some time, and it was subjected to any motion, would it not work its way down to the rock ?—Yes.

3756. That might reconcile, therefore, the two accounts ?—It might. In shallow water there would be no difficulty, I conceive, in driving a spear into the rock.

3757. What do you call shallow water ?—Under 200 fathoms ; beyond that I think it would be doubtful.

3758. (*Mr. Varley.*) Several portions of the cable recovered from Newfoundland show abrasion ; the wires have been worn completely through, and in some cases the ends have been driven into the gutta-percha ?—It is quite possible, as I have suggested, that there is rock underlying the superficial stratum of mud which we bring up by our soundings.

3759. (*Mr. Stuart Wortley.*) Are you aware of any sub-oceanic currents or anything that would cause a motion of the cable at that great depth of 200 fathoms ?—No.

3760. If a cable were broken and recovered from 175 fathoms, and apparently it had been subjected to friction, are you aware of anything that would cause motion at that distance in the water ?—I do not believe there is any current there. Is it possible that an iceberg can have touched it ?

3761. There are no icebergs in that part at all ?—I am not aware of any current, nor do I think that any current exists at that depth to affect the cable in the manner described.

3762. One is at a loss to understand what could give motion to the cable at that depth to affect it ?—Yes ; nor do I believe in the existence of currents at that depth of any consequence.

3763. (*Mr. Edwin Clark.*) Do you think that light mud would be deposited on the rock if there were any currents ?—It is highly improbable ; certainly not a sufficient stratum of light mud. The rock would be washed as bare as the face of a cliff. I may say that I am a general disbeliever in the strength of currents as has been asserted. I do not believe the currents ever had the slightest effect upon the Atlantic cable in any way or anywhere.

3764. (*Mr. Stuart Wortley.*) In your opinion, when you get to great depths, you would have absolute quietude and repose ?—Absolute quietude and repose. In illustration of that, I may mention that on one occasion Commander Dayman, in sounding at 2,400 fathoms, paid out purposely 300 or 400 fathoms of line more than the depth, and those 300 fathoms, by a great chance, came up in a tangled coil upon the top of the weight. So remarkable was it that we thought it right to give a drawing, which is given, and accompanies his report. I thought it a conclusive proof of the absence of currents, and such has always been my belief.

3765. Whereabout was that ?—About halfway across the Atlantic. I have no doubt his printed report is before the Committee.

3766. (*Chairman.*) Have you considered the soundings taken between Gibraltar and Malta by Commander Dayman ?—Yes, I have them all before me.

3767. Is there not some discrepancy between those soundings and the soundings previously taken both in the Straits of Gibraltar and in the line near Pantelaria ?—There is near Pantelaria ; but I should be disposed to reject the former soundings, and adopt the more recent ones.

3768. You do not think any volcanic action can have taken place there ?—No ; I think all the soundings above 150 fathoms formerly were very loose indeed, and not to be trusted. It is only very lately that we have learned to sound in great depths beyond 200 fathoms.

3769. Do you also think that the previous soundings taken in the Straits of Gibraltar should be rejected ?—I do.

3770. Where there is a difference of 400 fathoms ?—Yes ; reject them entirely, except those taken by the French, which coincide exactly with Commander Dayman's, except in one spot where they appear to have overlooked a remarkably shallow ridge, which runs, I may say, from Cape St. Vincent to Cape Spartel.

3771. Is not the bottom of the Straits of Gibraltar bare rock in parts of it ?—Apparently.

3772. Would not that rock interpose great difficulties to the maintenance of a cable ?—I think it might, and the notoriously rapid currents in the Straits of Gibraltar.

3773. What is the velocity of that current ; four miles an hour ?—That is the maximum, I think.

3774. Is not that surface velocity ?—Yes.

3775. Of course, at the bottom it would be less than that ?—Probably much less. When I spoke of currents previously, I alluded to the Atlantic Ocean. In any strait, like that of Gibraltar, or the straits in the immediate neighbourhood of the Shetland Islands or Iceland, the currents are very strong.

3776. (*Mr. Varley.*) Is not the current through the Straits of Gibraltar always in one direction ?—Always.

3777. Therefore the current would probably be less liable to interfere with a cable when it is down than if it changed in its direction ?—I do not think it would much signify afterwards ; the current must be either east or west. I do not think it would signify which way it was, or if it were both ways.

3778. The great fear from currents is where a cable lies across a rock, and the current is going backwards and forwards, see-sawing the cable, and ultimately cutting through the iron covering, even when very thick iron is used ?—In that case it would be better in the same direction ; but I have very great doubt of any current having that effect in any depth of water.

3779. Would not there be great difficulty in going over that shallow ridge which the French seem to have missed to get to Gibraltar ?—If it extends as far as Cape St. Vincent, but I am not aware that it does ; I do not think that there is any shallow ridge which you would have to cross of any importance ; I alluded rather to one very shallow spot of 45 fathoms, which the French have quite missed ; it is so near the coast of Africa, that it could be easily avoided in laying the cable.

3780. (*Mr. Stuart Wortley.*) In an early part of your evidence I understood you to say that you thought on the east coast of Greenland there was no current ?—I am not aware of any very strong current in the east coast of Greenland ; the ice is heaped up on the east coast of Greenland, but that may be as much by the wind as by the current.

3781. I find in an account given of the late expedition by Captain M'Clintock, Written by one of the officers of the "Fox," there is this statement, "When near "the meridian of Cape Farewell, by falling in with "the driftwood annually brought from Arctic Asia by "the great current known as the Spitzbergen cur-

" rent, the shattered and mangled state of these pine " logs bearing evidence of their long water and ice- " bound drift." Are you aware of the nature of the Spitzbergen current ?—Yes ; but I should say that less than half a mile is the maximum rate of it, if it is as much. When I spoke of a rapid current, I meant something much stronger than that ; it is not of a rapid nature at all ; there is a current setting out of Davis's Straits, we know that icebergs are brought down, and we know from the drift of ships, that their drift is only a few miles a day.

3782. I imagine of course when the writer speaks of a great current and the shattered and mangled state of the timber, the current must be very violent ?—I think we could tell the rate of that current from various experiments made with bottles thrown overboard.

3783. That is not an impediment to which you would attribute any importance with reference to laying a line of cable ?—No ; the mangled state of the fir trees might be caused by abrasion against the ice and the rocks. The experiments with bottles are very interesting, and the Committee will be glad to learn that the United States are taking it up in a very liberal manner. I am not aware that it is done by the Government, but Mr. Blount, a well-known chart seller at New York, is now distributing to all ships that go forth from that port, bottles to be thrown overboard in the current, with statements where they were thrown overboard ; and he has requested the Admiralty, amongst others, to receive the accounts that are brought in and to transmit them to New York. The English have done it for some time, but there is now another and very active labourer in the field.

3784. (*Chairman.*) Has the English Admiralty supplied bottles ?—The Admiralty has for a long time done that.

3585. You have mentioned you did not think that there was any volcanic action going on in the Mediterranean to the south of Pantelaria, do you think there is any volcanic action between Pantelaria and Sicily ?—I did not mean to say that these was no volcanic action ; but I do not attribute the difference in the soundings to volcanic action ; we know that Graham's bank has been thrown up within the last 30 years.

3786. That is to the north of Pantelaria ?—Yes ; all I mean to say is, that the difference in the soundings is not due to volcanic action, but is due to discrepancies in the soundings themselves ; I think it is a very dangerous piece, and the telegraph wires will be constantly getting out of order.

3787. Even if they pass to the south of Pantelaria ?—I think it is to be feared ; I think it is a dangerous line ; passing between Pantelaria and the coast of Africa, I think, would be better. You would be in the region of deep water as close to Cape Bon as possible.

3788. Are you aware whether there is any volcanic action near Cape Bon ?—None that I remember ; I think the further you keep out from Sicily, the safer your line will be ; I think you may look at Etna as the centre of the volcanic action.

3789. Near the Lipari Islands there is volcanic action, is there not ?—Yes ; but you should keep as far from Etna as possible, I should say.

3790. Are you aware whether there is any distinct volcano to the south of Pantelaria ?—Not that I recollect at this moment.

3791. With regard to the American sounding apparatus which brings up the bottom as well as tells the depth, called Brook's apparatus, has that any defects, do you think ?—I am not prepared to say that it has, but I prefer the mode I have mentioned by separate operations.

3792. (*Mr. Edwin Clark.*) In the mode of sounding you have mentioned, is the weight too heavy for the line to bring it back ?—Yes ; our great object is to run the line out as rapidly as possible, therefore we put about two hundredweight on to a cord line to get it down as rapidly as possible.

3793. By having a weak part at the bottom of the line, you only lose a small part of it ?—No, we make the line equal ; it just breaks where it pleases.

3794. (*Mr. Varley.*) Do you not haul in the line as far as possible ?—We haul it up till we get it up and down. One observation is, with reference to the interval of time, carefully watching the running out of the line every 100 fathoms. The second operation is to get the line up and down, that is an important circumstance.

3795. You could not do that if you used the American apparatus which detaches its weight ?—No ; because if the weight is detached we have no power of getting the line up and down, which we think more important, and more to be relied upon.

3796. Of course the line runs out for some time, simply by momentum ?—No doubt. It is a pity that we cannot do it by machine ; that Massey's machine is not sufficiently correct to enable us to register the soundings, but we do not find it sufficiently correct yet. The time may come when that machine may be made perfect ; but even that, with an error of some 300 fathoms in 2,700, is correct.

3797. (*Mr. Stuart Wortley.*) With regard to the soundings near Newfoundland, you are aware of the line which the Atlantic cable at present takes to the north of the banks ?—Yes.

3798. Can you tell us whether there is any deep water to the south of the banks, or rather to the south-west of the banks, so that it can run up to the coast of Labrador without going to Newfoundland ? Is there deep water through that channel ?—No ; we know the depth, 200 fathoms.

3799. That would be safe from fishing lines I suppose ?—Yes.

3800. And from anchors ?—Yes.

3801. There seems to be a passage by which a cable might be brought round to Nova Scotia ?—Yes ; if 200 fathoms would do, certainly it might be brought to about the Gut of Canso, and I think it might be well worth considering, because then the cable would be under the shelter of the banks of Newfoundland, from any drift ice that could damage it.

3802. You are not aware of any danger from ice that would be likely to preclude entering at that point ?—No, I think not.

3803. It would be an additional length, would it not ?—Yes ; an additional length of nearly 300 miles, and more perhaps ; in that case it would take the line generally more to the southward.

3804. (*Mr. Varley.*) Would you not be obliged to go a good deal to the southward to avoid the banks of Newfoundland ?—Yes, you must go 300 miles to the southward.

3805. (*Mr. Stuart Wortley.*) In coming from the Azores it would be possible to take that line ?—Yes, it would be the best line to take, and I think as short as the Newfoundland, because it would not do to cross the bank.

3806. (*Chairman.*) Are you aware of any process at the bottom of the sea, below depths greater than 100 fathoms which would be likely to injure a cable when once laid ?—Nothing below 200 fathoms.

3807. What would be likely to injure a cable down to that depth—animal life ?—No, I am not aware with regard to the northern line, icebergs or anything of that sort would probably affect a cable at 200 fathoms.

3808. (*Mr. Stuart Wortley.*) Do you know the greatest depth to which any known iceberg has extended ?—No ; I have in my possession a drawing of an iceberg 1,000 feet above water. I suppose that would be 5,000 feet below water.

3809. Is not an iceberg usually twice its depth under water ?—Five times, I think, is the proportion if it is what is called salt-water ice; of course it wil not be all salt water, because it is formed on land, but if there is a great deal of salt in it, I have heard that

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it is five times as much under water as it is above. I speak under correction.

3810. Do you mean that the submerged part would be as five to one?—I am not sure about that; it is more than three to one.

3811. I think we may collect, speaking generally, you are of opinion, that further investigation of the bottom of the Atlantic is desirable, with a view, at all events, to telegraphy?—I think it is.

3812. You think it would be a legitimate and worthy use of the national ships to obtain that information?—Quite so.

3813. (*Chairman.*) Do you think the northern route by Greenland offers sufficient chance of success to be worth going to any trouble, with respect to laying a submarine cable in that direction?—My individual opinion is certainly not; but it is right to say, that I have not studied the question as much as many others may have done; Sir James Ross and others, for example, and their opinion is far more valuable than mine. I am giving my impression rather than anything else, I have not had time to go into the question; but I must say that I think the ice and icebergs are very formidable matters to deal with.

3814. The ice is the only difficulty we have to fear in the northern route, and not the currents or the depths of water?—No. I think the depths would not be greater than you have already had in the Atlantic, if so great.

3815. (*Admiral Fitzroy.*) When you referred to Iceland, you were not aware of any soundings or examination having been made by any person to the south of Iceland, or of any survey having been executed within a few hundred miles of the island?—No, except the Danish survey of Iceland, which I have before me. I know of nothing else, except Sir James Ross's attempts at soundings, in which he did not get bottom.

3816. What I mean is, that no survey has been made by this country of that part?—None.

3817. Then that line, from the south of Iceland to the south of Greenland, is unexplored, so far as we know?—Quite.

3818. Then, again, from the south of Greenland to Labrador is also unexplored?—Quite so, as far as I am aware.

3819. With regard to currents in the ocean generally, when you say that there are no currents at the bottom of the ocean, you do not mean to include what we call soundings ordinarily at a depth of 100 or 200 fathoms; there may be currents over those comparatively shallow places, although there are none in the depths of the ocean?—Just so. I meant to limit myself to the depths of the ocean.

3820. The instance mentioned by Mr. Stuart Wortley, was 175 fathoms depth near the land; but your general opinion as to depths of the ocean would not include instances of that description?—No, certainly not.

3821. We know that there are currents passing up and down the coast, and particularly near projecting points?—When I spoke of currents, I think it was rather deep water currents, and Mr. Stuart Wortley alluded to superficial currents, about which there cannot be a doubt.

3822. But his question, as I understood it, had especial reference to the chafing of the cable at a depth of 175 fathoms?—I think it quite possible that it may have taken place; but I should hardly expect in a quiet place like Bull's Arm Bay, there should be any current passing up and down sufficient to abrade a cable at a depth of 175 fathoms; I think it improbable, but I will not say it is not the case.

3823. Captain Cochrane in his soundings in various parts of the Gulf stream, found evidence from the great change of temperature of strong currents from the north, the upper current running from the south at a depth of 500 to 1,000 fathoms; he found the lower stratum of water at a temperature entirely different from that at the surface, from which he in his soundings inferred a current below?—I quite admit that there is sufficient current to diffuse the temperature. When I spoke of currents, I rather meant strong currents which would affect a cable; that there is sufficient current to diffuse a stratum of water at a different temperature is quite certain.

3824. Why I refer to that particular point is, on account of the opinion entertained by some, that the polar current comes round Newfoundland, meeting the Gulf stream, and passing either by the side of it along the shore, or underneath it; some say that it passes down the coast, others say it goes under the Gulf stream. The reason for thinking that it goes under the Gulf stream is because at a considerably lower latitude, and even near the Florida Strait they find a very low temperature at a considerable depth?—I am quite disposed to think that it does pass under. There is quite sufficient current to diffuse the temperature, but not a current of any very great strength, I think.

3825. The other point to which I was desirous of adverting, was the American soundings, which have been made by Brook's sounding apparatus, as it is usually called, not only in the Atlantic, but in the Pacific. Are we not considerably behind the Americans in deep sea soundings? Are not the soundings that have been made by our ships in the last year or two, for instance those made by Captain Dayman, however good in themselves, few in comparison with what the United States Government have made during the last five or six years?—I am quite willing to admit that the British Government is in arrear as to the number of deep sea soundings made; but I think as far as accuracy is concerned, we are far in advance of any other nation.

3826. I understand that you decidedly prefer the method you have described, as having been adopted by our ships lately, to the American method?—Certainly.

3827. Do you remember how anxious your predecessor was some five or six years ago to have deep sea soundings taken in various parts of the world?—I well remember it.

3828. It was a fixed idea with him. You may not remember my having been spoken to by him to undertake some deep sea soundings of that kind, but I refer to it to show that the intention on the part of the Admiralty was to carry out such an exploration of the bottom of the ocean, two or three years at least before the United States Government took it up?—I am quite aware that that was the intention and wish of my predecessor Sir Francis Beaufort, and had his wishes been complied with, the British Government would have sounded out the Atlantic, and all the questions respecting telegraphy would have been set at rest, had only his foresight and his judgment been listened to.

The following document was handed in by Captain Washington:

3rd March 1860.

With the view of illustrating the experience gained by practical experiment in measuring the depth of the ocean, I have drawn out, in a tabular form, a series of results showing the periods of time occupied by various weights and lines in sinking from 400 to 1,400 fathoms, a space of 6,000 feet as found by American naval officers (see Lieut. Maury Sailing Direction, 8th edition) and myself.

The simple method of deep sounding by time with weights and marked lines, first successfully adopted by Rear Admiral Sir J. C. Ross in his expedition to the Southern Atlantic sea, appears to be the only trustworthy one yet devised, and will probably continue with any imperfections attaching to it until a mechanical contrivance for self-registering free from the defects of that invented by the late Mr. Massey shall be discovered.

The principal condition required to ensure accuracy and certainty in results obtained by these means is rapidity of descent, and we may in some measure judge of the relative value of each experiment by the regularity of descent shown in the gradual increase of the time intervals noted. These intervals with similar weights and lines should correspond

with each other in different experiments, but they have never been made to do so exactly, probably in consequence of unequal friction in the reels employed; in like manner the periods of time occupied by similar weights and lines in sinking through a given large space, such as 6,000 feet, are not always the same, and appear to be least discordant with each other when the periods of time are least.

The American officers employed very small and light lines and round shot, and sounded from boats as enjoined by Lieut. Maury.

My own soundings were taken from the ship when out of the influence of surface currents, and a comparison of the results may tend to increased confidence in this practice, which is less liable to interruption from wind and sea, and is not difficult with the assistance of steam.

Summary No. 1.

Time Intervals, Maury Sailing Directions, pp. 135, 138.

—	Lieut. Lee, U.S.N. 32 lbs. shot, small line, 400 to 1,400 fathoms.	Lieut. Berryman, U.S.N., 2, 32 lbs. shot, small line, 400 to 1,400 fathoms.	Lieut. Berryman, U.S.N., 2, 32 lbs. shot, small line, 400 to 1,400 fathoms.
	m. s.	m. s.	m. s.
1	26 45	25 12	25 32
2	27 46	24 03	24 31
3	28 51	26 12	28 17
4	27 24	26 11	24 30
5	19 09	26 06	24 55
6	19 30	28 10	26 55
7	26 49	25 25	26 56
8	29 30	24 35	26 08
9	28 45	24 40	25 45
10	26 06	25 41	23 20
11	26 59	31 57	26 03
12	29 44	27 48	24 15
13	23 47	24 20	25 33
Sums	341 05	341 20	333 00
Means	26 14	26 16	25 57
	Ext. range 29·44 19·9 = 10m 35s.	Ext. range 24, 20, 31, 57 = 7m. 37s.	Ext. range 23m. 20s., 28m. 17s., = 4m. 57.

Summary No. 2.

Time Intervals.

—	Lieut. Dayman, R.N., H.M.S. Cyclops, deep sea line 96 lbs. 400 to 1,400.	Com. Dayman, H.M.S. Gorgon, albacore line 188 lbs., lead 400 to 1,400 fathoms.	Com. Dayman, R.N., H.M.S. Firebrand, contract line 100 lbs., 400 to 1,400. fathoms.
	m. s.	m. s.	m. s.
1	26 54	18 31	21 3
2	23 17	18 30	18 12
3	26 06	17 13	21 23
4	26 11	17 42	19 20
5	23 43	18 21	17 15
6	26 58	16 48	17 45
7	25 04	17 20	20 13
8	28 05	17 07	15 58
9	24 56	17 04	18 46
10	26 57	17 04	17 12
11	28 07		18 53
12	29 43		16 44
13	27 16		19 15
14	27 23		
15	26 04		
16	25 18		
17	28 45		
18	29 10		
19	29 14		
20	27 48		
21	26 51		
22	28 38		
Sums	151 28	75 40	241 59
Means	26 53	17 34	18 32
	Ext. range 28,17, 29,43 = 6m. 26s.	Ex. range 16, 48, 18, 31 = 1m. 43s.	Ex. range 15, 58, 21, 03 = 5m. 05s.

In the following table, derived from the preceding and other sources, we have the comparative rapidity of descent of different weights with several kinds of line, and by different authorities, and also the extreme discrepancy in each series of experiments, from which it appears that the least velocity obtained was with seine, twine, and weights of 13 lbs., and the greatest with albacore line and 188 lbs. of sinker, the irregularities before noted between several results with the same means diminishing in proportion to the quickness of descent.

Captain J. Washington R.N., F.R.S.

9 March 1860.

Line employed.	Weight. Description.	Weight. In lbs.	Mean of times in sinking from 400 to 1,400 fathoms.	Velocity. Feet per minute.	Extreme range of intervals of time.	Authority
			m. s.		m. s.	
Seine twine	Iron head	13	36 03	5	7 40	Captain Sprett, R.N., C.B.
Dog line	Shot (iron)	68	29 47	2	1 10	Com. Maxwell, R.N.
Deep sea line	Iron	96	26 53	22	6 26	Lieut. Dayman, R.N.
Small line (American)	Shot	32	28 14	13	10 36	Lieut. Lee, U.S. navy.
Ditto	2 shot	64	28 16	13	7 37	Lieut. Berryman, U.S. navy.
Ditto	Do	do.	25 37	13	4 57	Do. do.
Contract line	Lead	100	18 32	13	5 05	Com. Dayman, R.N.
Albacore line	Lead	188	17 34	10	7 43	Do. do.

JOSEPH DAYMAN,
Commander R.N.

DEAR SIR, Admiralty.

IN returning you the enclosed proof which you were so good as to send me, I send you also a copy of an old memorandum, found among my ancient papers, relating to a submarine volcano off Iceland, a subject which I perceive has formed a branch of your recent inquiry. Should it be of no consequence, you will perhaps be at the trouble to return it to me at your convenience.

I have no doubt that there are others (eruptions) in the same direction, as it is immediately off that part of the coast whereon Hecla is situated, and although laid down in the Danish chart, the account of it may be interesting.

I see some of the evidence refers to Rockall. As this can never form a station, I am at loss to discover what benefit will arise by taking a cable there. There is a rise and fall there (according to Admiral Vidal, see N. M.) of 12 feet, and of course such a sea as might be expected, that I think would mutilate a cable without remorse. Notwithstanding what has been said about icebergs (and I see a good deal here and there in the proof), it is my humble opinion that all attempts to keep a cable in an efficient condition, on either of the coasts mentioned, subject to the visitations of icebergs, will end in disappointment. No doubt one may be laid down on a favourable opportunity, but how long will it be available? So long as ice above and volcanic fumes below let it alone: but such visits are ever uncertain.

You are aware that the neighbourhood of the Azores is a prolific district for volcanic display, especially east of Terceira. I might point out places where the ground has been disturbed, but I should think their well-known volcanic nature is sufficient to prevent a cable ever being taken there.

Between them and Newfoundland there is a shoal sounding of 100 fathoms, which has been so well authenticated by a naval officer as being on volcanic ground, that although it is not more than 10 or 12 miles from a sounding of 3,000 fathoms, and no bottom, of Captain Dayman, this cannot displace it. Teneriffe itself, over 12,000 feet above the ocean, has a sounding (without bottom) under 300 fathoms, at a similar distance. It is a very great pity the Atlantic is not better sounded than it is, especially between Newfoundland and Ireland.

I had much confidence in our early soundings, until the Americans showed us there were between 2,000 and 3,000 fathoms where we had laid down 6,000, so that I have no faith in those obtained by Sir Francis Beaufort's method, carried out by Sir James Ross and Sir E. Belcher, and even Captain Denham's deep sounding of 7,000 fathoms between Tristan d'Acunha and the River Plata, has been reduced, very properly, by Lieutenant Maury to 4,000; but most unfortunately not before it was quoted by the late Mr. Hallam, as the greatest depth known. But this was even made with American line, showing how necessary experience is in such matters, and throwing our ropeyarn soundings sadly into the shade. But pray excuse this long story from

Yours truly,

Capt. D. Galton, R.E. A. B. BEECHER,
Capt. R.N.

On the 13th of March 1830, a smoke was discovered rising out of the sea to the S.W. of Cape Reikianes, at the distance of 35 or 40 miles. As during the end of the

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month, the weather was calm and the sky clear, the smoke was seen rising to a prodigious height, and seen from Reikjavig, it appeared like a large cloud. This continued for a couple of months, and some individuals pretended to have seen it emit fire, which appears doubtful on account of the distance; but the volcano threw out vast quantities of burnt stones and brimstone, which were driven on shore. The volcano was seen again for some time during the next winter.

Mr. Gunlogsen attempted to determine the position of this volcano by angular measurement taken on shore. This operation assigns to it the same position as the submarine volcano which, in 1783, made a new island appear to the S.W., in about 63° 30′, and 23° 37′ W. of Greenwich; and where still exists the sunken rock called Blind Flugleskiar, upon which the sea was seen breaking, in 1829, from the Danish sloop of war "Najaden," Captain Kinch, when that vessel was passing very near it.

C. W. Siemens, Esq.

C. WILLIAM SIEMENS, Esq., further examined.

3829. (*Chairman.*) I believe you have some additional experience to detail as regards the superintendence and electrical conditions of deep sea cables. Since your last evidence was given you have acquired some information as to the Singapore line and the Red Sea line?—I have. With regard to the Singapore, Banca, and Batavia line I have received a detailed account from my representative who accompanied the expedition respecting its electrical condition before and after laying. I will give the important data respecting it. The line consists of two sections, the Singapore and Banca line, 264 nautical miles, and the Banca and Batavia line 286. The line throughout its length is in very shallow water.

3830. (*Mr. Stuart Wortley.*) What do you call very shallow water?—I believe the depth does not exceed 25 fathoms. Before the cable was submerged it was carefully tested, and having always adopted the system of expressing the insulating property of the gutta-percha covering in units of resistance and the opposition of the copper to the current in units of resistance, (in order to be free of all variations of batteries and of instruments), in measuring this cable we found in 100 miles of cable the copper had a resistance of 819 units and the gutta-percha a resistance of 291,529 units, that is to say for every one fraction of the current that passed through the gutta-percha 356 passed through the copper conductor.

3831. (*Chairman.*) That is on board ship?—Yes. The same test was made when the cable was laid, and the result was that the resistance of the copper was 824 and the resistance of the gutta-percha 190,000 or there was a loss of the current of 1 in 231 against 1 in 256, showing that the insulation had slightly decreased after it was laid, and the gutta-percha was slightly more conductive, but the total loss expressed in per cent. did not exceed ·43 or less than one-half per cent.

3832. Do you know what was the temperature of the water?—Unfortunately I have no correct data to go by. A fortnight after laying the same test was gone through again and the resistance of the copper was 837 and the resistance of the gutta-percha 181,840, showing a loss of 1 in 216 or ·46 per cent. These tests prove that the conductivity of the gutta-percha rather increased in and shortly after the laying. The Banca and Batavia line gave very similar indications. The copper had a resistance of 816, and the gutta-percha had a resistance of 346,933 before laying which represents a loss of 1 in 425. After laying the resistance of the copper wire was 883, and the resistance of the gutta-percha 259,584, which is a loss of 1 in 312. Showing that the loss in this case was less than before, but the per-centage of loss in both these cables is exceedingly small if compared to cables previously laid.

3833. Do you think there was any fault in this cable?—Not then, but after the line had been completed a fault suddenly appeared according to the calculation of my representative who was there 190 miles from Batavia. The cable was taken up at that spot and being underrun for one mile it was found to be broken by an anchor.

3834. Was not there a complete stoppage to the communication?—A complete stoppage. As an instance of ascertaining a fault to a great nicety that is an important fact.

3835. (*Mr. Stuart Wortley.*) The fault was calculated beforehand, and turned out to be correct within a mile?—The ship went to the spot indicated, and the fault appeared within a mile from that spot.

3836. (*Mr. Varley.*) Was the ship within sight of land?—I cannot say whether any land was in sight at that point. You may call the calculation nearly absolutely accurate.

3837. (*Mr. Stuart Wortley.*) What distance was that fault calculated?—190 miles, the total length being 286 miles. This fault having been just rectified, a second fault appeared suddenly at 112 miles from that spot by calculation. Measurements were taken to ascertain this fault, and they gave a result that it would be at 112 miles distance; but there was great difficulty in taking the indication, because there were very strong currents of polarization, and it was found when the cable was picked up that the fault lay 5 miles further on. It appeared afterwards upon examination that the cable had been again broken by an anchor, a great length of the copper wire had been laid bare, having been stripped out of the gutta-percha, and the oxidation of the copper had greatly interfered with the indications.

3838. (*Mr. Edwin Clark.*) Was the whole of the cable covered with iron?—Yes; it is nearly identical with the Red Sea cable.

3839. (*Chairman.*) Will not that cable be liable to great injuries?—It will always be liable to injury. It should be all shore end, the sea being so exceedingly shallow; otherwise there should be stringent rules enforced against anchors dragging there. I do not see how the cable can last for any length of time without requiring repairs again.

3840. (*Mr. Stuart Wortley.*) Where does the cable run from and to?—It runs from Singapore to Banca, an island off Sumatra, and from Banca to Batavia.

3841. Is it in shallow water all the way?—All the way in exceedingly shallow water. A final test before leaving the line was made, giving the resistance of the copper of this last section at 839 units, and that of the gutta-percha at 190,190, showing a loss of 1 in 231, or ·44 per cent.

3842. (*Chairman.*) It had very much deteriorated?—In exactly the same ratio as the other; the other gave ·46 per cent.

3843. The second line was very much less before it was laid?—The second time it was better.

3844. The second line was only ·2 to begin with?—The second line was ·24; it increased after laying to ·32 per cent.; and it increased after these two repairs had been made to ·44, showing an increase of leakage; but still the test is exceedingly perfect, the loss being less than ½ per cent. in 100 miles.

3845. (*Mr. Varley.*) When you say that the loss was ·44 per cent. a loss, as compared with what standard?—The standard is a column of mercury, one metre long and one millimeter square at a temperature of 20 degrees centigrade; therefore the gutta-percha in 100 miles length of this cable gives the same resistance which a mercury column of 190,190 metres long would give; whereas the copper conductor gives a resistance equal to a mercury column 839 metres long, of the same section, always reduced to the temperature of 75° or 20° of centigrade. My brother has lately written a paper, which has just been published in Poggendorfs Analin, setting forth the advantages of this standard.

3846. ·44 per cent. is the amount of current lost, compared with the amount of current the cable would conduct when one end is to earth?—Quite so.

3847. (*Chairman.*) How do you ascertain the conductivity of the gutta-percha?—It is by the current passing through the gutta-percha when the opposite end of the cable is insulated. The actual taking of these results requires a great deal of practical nicety, because the current passing into the cable is very strong at first, and decreases by degrees in an extraordinary ratio.

3848. Do not you find the conductivity of the gutta-percha by insulating one end, and then you fix it from the resistance of the copper?—No. The instrument we use is a modification of Professor Wheatstone's bridge. We compare two resistances, —a known resistance against an unknown resistance. In order to ascertain the resistance of gutta-percha, we produce an artificial resistance, which we can alter and make exactly like the resistance of the gutta-percha covering of the cable itself, or rather like a definite proportion of the same. In developing this principle my brother has had recourse to a system of decimal multiplication by mechanical stoppering, which enables us to read with great accuracy, resistances varying from $\frac{1}{10}$ part of a unit to a million of units.

3849. By insulating one end of the cable from the ground?—Yes; the cable is insulated at the other end, so that the current can only pass through the gutta-percha. When we take the resistance of the copper, we connect to earth, and compare the resistance of the copper to that of our coils.

3850. You deduct from that resistance the resistance previously ascertained from the gutta-percha?—Yes; but it is so slight a fraction that it may generally be neglected. The indications require a great deal of nicety, because the current passing into the cable shows a very different result at the first moment and after, say, two minutes. I have taken note of the diminution. The current decreases for two hours when it has reached its minimum; it decreases during that time to about one-third, and in the first fifteen minutes to one half the initial amount. In order to obtain comparative results, we take every observation after a certain lapse of time after the connexion has been made, and in this way we are enabled to obtain great certainty in the results. I am I am also in possession of some results of testing the Kuratske and Muskat line, which form a most interesting contrast to those of the Singapore line, both cables being of the same proportions. The copper gave in 100 miles a resistance equal to 810 units, and the gutta-percha a resistance of 480,000 units on board ship, whereas after it had been submerged the resistance of the gutta-percha increased to 840,000 units.

3851. The copper remaining the same?—Yes; it would slightly vary according to a known ratio, but the resistance of the gutta-percha is nearly doubled.

3852. In what depths was that line laid?—2,000 fathoms, showing that the insulation of the gutta-percha increased probably with the reduction of the temperature as well as by the increase of pressure upon it. We have found, I may mention here, that the Gibraltar core tests generally better under pressure than before it goes under pressure; it is indicated before the pressure comes on, and again after the pressure is on. The result is very uniform for the best coils.

3853. Of which are you speaking?—Of the Gibraltar core now. I am not in possession of more detailed information respecting the Indian Extension line.

3854. Have you similar diagrams showing the process of paying out to those which have been prepared in other cases?—Not yet; it will take some time to prepare them. Respecting the conductivity of the copper, I may mention that the Gibraltar core gives in 100 knots of copper a resistance equal to 377 units, which resistance varies in different coils within a limit of 20 per cent., the temperature being uniform.

3855. There is so great a difference in the copper?—In the conductivity of the copper.

3856. Some is very bad, then?—Relatively so. There may be slight differences in the weights of those coils.

3857. Then that difference of 20 per cent. in the conductivity that you have mentioned, may be partly attributable to a difference in the weight of the copper for some distance?—It may to some extent, but that variation could not amount to anything like 20 per cent. Being struck with this amount of difference, I submitted samples of the most and least conductive strands to Dr. A. Matthieson for analysis; and Dr. Matthieson informs me that in testing the conductivity of separate wires in the strands he found a difference of forty per cent. The copper contained in all instances traces of lead, but the differences in the amount of foreign matter were so small that they could not be determined quantitatively. Chemically pure copper proved, however, to be the best conductor.

3858. The insulation tests of the Gibraltar core give a resistance in 100 knots of 1,800,000 units, at a temperature of 80 degrees.

3859. What proportion would that be?—A proportion of 1 in 4,800, or a loss through the insulating medium of ·0021 per cent. in 100 miles. If taken at a temperature of 41°, the resistance of 100 miles of the gutta-percha is equal to 13,800,000 units, which proves that the insulation of gutta-percha is fully seven times higher at 41° than at 80° Fahr. The object of testing the core at one uniform temperature, and that temperature the highest that is in practice to be dealt with, is, in the first place, that every fault is increased sevenfold, because the conductivity of the gutta-percha is sevenfold, and, moreover, we have data for comparison; a slight variation of one or two degrees in temperature would obliterate slight differences in the conductivity of the gutta-percha, and render the measurements of short lengths of cables quite useless.

3860. You do not anticipate that a temperature of 80° is likely to produce any permanent injury to the core by an alteration in the centricity of the wire?—If it does, I think the core could not safely be laid in hot climates.

3861. May it not produce that eccentricity without your detecting it?—I could not say for certain whether an increase of temperature to 80° would ultimately produce any sensible alteration in the shape of the cable, but considering that the tests only last some hours, there is, I believe, not time for the copper wire to change its position inside the gutta-percha.

3862. What temperature have you found sufficient to cause a change in the position of the copper wire in the gutta-percha core?—I have suspended a core in a place where the temperature occasionally reaches to 100° and there I found the process active continually. After two months time there was a decided eccentricity produced, but it goes on at those temperatures by slow degrees. Whether at a temperature of 80° the action would go on more or less rapidly there is no experiment to show.

3863. You are aware that the water in the canal at the Gutta-percha Company's works is in summer often 80°?—Yes.

3864. Therefore if any injury occurred to the core from your experiment is it not more likely to occur from the water in the canal itself being hot?—Just so. There are two reasons for exposing it to this heat, the first is that it is a heat which we must expect it to be subjected to in summer in the hold of a ship, and unless the core is tested to what it has really to stand afterwards the test would not be conclusive of the goodness of the core. The second reason is, that in order to get correct comparative results all the tests should be made at a uniform temperature, and it is probable that some portion of this line or other lines must be tested at temperatures approaching 80°.

3865. Have you made any experiments with Armstrong's machine for the purpose of detecting holes in the gutta-percha?—Yes; I met with a practical difficulty, that is, in employing electricity of high tension the gutta-percha is not simply charged in the

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sense that you charge a Leyden jar but the charge is absorbed in the gutta-purcha, the current continues to flow in, and it is impossible to keep the static charge in a gutta-percha coated wire. If, on the other hand, the wire was charged suddenly there would be a risk of injuring it.

3866. (*Mr. Varley.*) If charged gradually it would escape faster than you produce it?—Just so. We therefore prefer to render the insulation tests under pressure and at elevated temperatures as accurate as it is possible to make them, and we have found by that means decided differences between different coils, some coils that are very perfectly covered improve under the pressure test whereas others remain constant, and others again have decidedly diminished in insulation.

3867. (*Chairman.*) Do you reject those coils which diminish in insulation?—Those that have diminished have been carefully examined, and if they have been found to contain air-holes they have been rejected.

3868. That pressure would not, however, be sufficient to detect air-holes in any part of the covering except the outer covering, I imagine?—The pressure is not sufficient to penetrate into the air-holes that are well covered.

3869. The core is not prepared to resist the great pressure to which it will be subjected at the bottom of the Atlantic. Would it not be desirable to subject the core to a test under pressure equal to that which it is eventually to bear?—It would, raising the temperature at the same time to the highest safe limit.

3870. Does working by reversed currents injure a cable?—That is a question which has been lately mooted. I am of opinion that working by reversed currents is the safest for the cable, independently of the advantages obtained by sending messages rapidly by it. In sending a positive current through the conductor, decomposition of the copper takes place probably throughout, but in a marked manner; whenever there is a slight fault, the oxide of copper continuing to form will increase the bulk of the conductor, and finally burst the gutta-percha covering; therefore it would be objectionable to work by positive currents continuously. Working by positive currents has an advantage in stopping up slight cracks, by the oxide of copper forming upon the outside of the conductor, and resisting for a time the action of the water; but the water penetrates the oxide so formed after a certain time, and a further coating of oxide of copper has to be formed till the entire conductor is eaten away. In working by negative currents only, an error of an opposite kind probably arises. The decomposition of the water in the gutta-percha will cause hydrogen to be deposited, or freed upon the surface of the conductor; this hydrogen will first condense, as we know hydrogen gas does condense upon metallic surfaces, but if it accumulates it must escape, either by forming a small channel through the gutta-percha, or by bursting it. Therefore that mode of working seems equally beset with dangers. The best mode of working is by alternate currents, first sending a negative current, which will form a film of hydrogen gas upon the surface of the conductor, and then, following the negative current immediately by a positive current, which, in discharging the negative charge that is accumulated in the conductor, reduces also the film of hydrogen gas already deposited upon the conductor. So that in working by alternating currents, the permanent condition of the insulating material is as fully maintained as it is possible to imagine. The discharging current is made of only half the intensity of the charging current, because one half of the charging current passes out through the receiving instrument, or a portion of it, at any rate. My brother has lately added at the receiving end a battery of polarization, which will produce an artificial return current into the cable at the receiving end, causing thereby more rapid discharges, and more rapid working. This battery of polarization consists simply of strips of platinum passing from one jar of acid to another, and from that into another jar containing acid, and so on. A current in passing through the instrument into this battery will cause hydrogen gas to be formed upon one lamina, and oxygen gas upon the other in each succeeding element, which gasses will condense upon these metallic surfaces. A grove gas battery is thus formed of only instantaneous power, because these gases have been formed upon the metallic surfaces in an extraordinary small film. But this current discharges the line wire at the receiving end, and produces more rapid working.

3871. You say that by using a negative current, a film of hydrogen is formed upon the copper. To form that film, water must have arrived at the copper, either from the gutta-percha itself or from the outside; is not that the case?—The gutta-percha contains water.

3872. But when the water in the gutta-percha itself has been all used up by the negative current, if there is a fault in the cable, that fault is caused by water coming from the outside?—Certainly.

3873. If you form a film of hydrogen upon the copper, that hydrogen will eventually acquire a pressure greater than the water outside?—Yes.

3874. And therefore if water can get from the outside to the copper, the hydrogen will escape from the copper to the water?—Yes.

3875. That being the case, what object can there be to work constantly with the negative current through a fault, when the fault itself gives you the means of discharging the hydrogen by the porosity of the gutta-percha?—The hydrogen escaping would probably increase the size of the aperture through which the water had forced its way by capillary attraction only.

3876. Would not the hydrogen escape in the same way by capillary attraction?—I think not.

3877. (*Mr. Varley.*) In practice it is found that the hydrogen does not blow the water out, the hydrogen itself escapes, it is not the hydrogen that enlarges the hole, but by bringing the water into the hole it enables the current to have a freer channel; the enlarging takes place from the electric current passing and destroying the gutta-percha?—Yes, that may be the mode of progression of a fault.

3878. Have you ever in any instance found a sufficient accumulation of oxide of copper to burst the wire, or do you merely speak from anticipation?—I have seen an instance in which it has burst the gutta-percha.

3879. (*Mr. Edwin Clark.*) In short you recommend the use of positive and negative currents alternately?—Yes.

3880. (*Chairman.*) Will you state your opinion as to the comparative merits of Wray's compound and gutta-percha as insulators?—Wray's compound possesses very extraordinary insulating properties. From the results I have given, there seems to be no ground for improving the insulation of gutta-percha; for all practical purposes it insulates well enough. The important question is to have a material of the least inductive capacity, and in that respect Wray's mixture is decidedly superior to gutta-percha. The proportion established by some experiments which I have tried, was 7 to 12 in favour of Wray's mixture. india-rubber appears, according to those experiments, to be very nearly as good as Wray's mixture. But in constructing a cable there are other necessary properties which we have to look to to make a safe cable; gutta-percha though it tests beautifully, is very liable to give way, unless it is very carefully dealt with.

3881. (*Mr. Varley.*) Give way from what cause?—From several causes, which I wish to allude to. In the first place, gutta-percha is put upon a wire and tests very perfectly, although we are not always certain of having excluded holes from the inside, which holes may ultimately fill with water and destroy the insulation. But a more fertile cause of the gutta-percha failing is if the core is exposed to tensile strain,

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Now, such tensile strain can be brought upon the gutta-percha covered core in different ways. In the machine for covering the strand or the core with the sheathing of wire, it is exposed to sharp pressure, because all the wires have to be bent upon the soft core, which, I believe, produces a slight elongation at the moment; and although the gutta-percha is sufficiently elastic to recover itself, a permanent elongation of the copper wire may be produced. Again, in submerging the cable, even if the iron sheathing is strong enough to resist the legitimate strain, it has been observed that after the cable has left the ship, it slightly untwists, caused by the partially free suspension of the wire; the great strain being nearest the ship, it elongates slightly there, and this elongation causes an untwisting of the covering, and consequently an increased elongation of the core. We know that after the core has been stretched, the copper wire will gradually try to be relieved from its state of compression, and the only way of its being relieved is by assuming a serpentine form or bulging in places towards the outside; hence the faults in sub-marine cables for the most part have consisted of places of eccentricity. The gutta-percha is peculiarly liable to this action, owing to its capacity of yielding to constant pressure, in the same way as pitch is known to yield very largely to pressure, even at the lowest temperature.

3882. How could an injurious extension of the core be guarded against?—I have to suggest some means of counteracting this stretching of the core. The first is to use a serving, possessing considerable strength, longitudinally, instead of the ordinary hemp serving, by putting on a covering of strings steeped in cement, such as marine glue, under some strain, and with a twist, in a sense contrary to the twist of the iron sheathing. A core so protected will resist not only elongation in the act of twisting the wires upon it, but it will also resist the tendency to untwist while it is being paid out, because the strain upon the strings will tend to make it twist in the opposite direction. For the same reason the tendency of the cable to form kinks would be greatly diminshed.

3883. How would you propose to prevent the rusting of the iron sheathing?—After the cable is sheathed, an external protection seems necessary to guard the wire from rusting, because it has been found that a cable, after having been submerged for some time, is very much rusted away. It occurred to me that, by laying saturated strings in between the wires, and then passing the cable through a heated die, a very perfect outer covering might be produced, which would not be liable to be rubbed off. Only yesterday I learned that Mr. Latimer Clark has suggested the same expedient, and so far I am confirmed in my views.

3884. How would you propose to construct a deep sea cable?—In constructing a deep sea cable, the conditions to be fulfilled are to get good insulation great security from external injury, and lightness. The material to be used immediately upon the wire should be one possessing very little inductive capacity, and one possessed of high insulating property. Perhaps gutta-percha, after we have learned something more of its capabilities, may be rendered a high insulator; but at present compounds, such as Wray's mixture, appears to possess decided advantages. There is one difficulty in using a compound of that description, namely, that they lose very much in quality by being exposed to heat in the covering machine. I therefore think it very desirable to coat the conductors without heating the material; and I have constructed a machine, which appears to accomplish the object very well. Here is some wire coated with Wray's mixture, which can be twisted about any how, and though it is single coated, it tests remarkably well. I should not be satisfied, however, with a coating of one material only, because an action similar to that we have observed in gutta-percha, namely, the core becoming eccentric, may arise; although the risk of eccentricities will be greatly reduced in avoiding heat in the process of covering, and the liability to air-holes will be entirely removed. Therefore I should always prefer to give one or more coatings of pure india-rubber. India-rubber possesses very remarkable mechanical properties, in addition to its high insulating property and low inductive capacity, it can be stretched to a great extent, and it possesses great continuity in itself. Therefore if any cavity should exist in the lower insulating medium the external pressure will tend to fill the same with india-rubber, and exclude the water thoroughly from the inside. It is very important to put on the external coating of india-rubber without the application of heat, because the heat will not only injure the india-rubber itself, making it sticky and oily, but it is certain to injure the coating below it, and therefore I have tried to accomplish that also without the application of heat. Although for insulation a very thin covering is all that could be desired so long as the insulating medium employed possesses very high insulating properties, it is necessary to add to its thickness in order to diminish the influence for induction. It is not necessary, however, that this additional covering should possess high insulating properties; we may therefore select a material combining great strength with moderate insulating properties. I propose for this purpose to put a number of hemp strands saturated in insulating cement, such as marine glue and shellac or any other compound, in several layers upon the insulated conductor, and to bind the whole together by a band of copper or brass in order to protect the saturated fibre from external injury. The copper should be either soldered up solid or it may be made so as to form a complete gripe joint. In this way a cable may be produced combining a very low inductive capacity with great relative strength and permanency. Its specific weight would be about = 1·4, and it would be capable of supporting sixteen miles of its own weight in sea water. Its absolute strength is above two tons, and, what is important, it breaks with little more than two per cent. of extension; whereas an iron covered cable extends above three per cent. The hemp fibre, being thoroughly impregnated and bedded solid in the elastic cement, can suffer no contraction or deterioration from the sea water, being thoroughly excluded. This cable has, moreover, no tendency to untwist or to form kinks. The copper sheathing is intended as a permanent protector against abrasion and marine animals. A cable constructed in this manner will not be more expensive than an iron covered cable. In shallow seas, and for shore ends, an additional iron or steel sheathing will still be required.

Mr. Siemens explained the mode of construction and working of his machine for covering conductors with Wray's compound or india-rubber to the Committee.

3885. (*Mr. Varley.*) Does Wray's compound unite as well as india-rubber?—No, not quite so perfectly. By heating it to 100° Fahr. it adheres perfectly, but I want to avoid that, because you cannot be sure in that case of uniform thickness. This compound is strengthened by being dealt with cold; and I consider it of great importance to maintain the degree of strength which it possesses. In crossing the longitudinal joints of succeeding coatings a high degree of cohesion laterally is also produced.

Adjourned to Wednesday next at Two o'clock.

Wednesday, 14th March 1860.

PRESENT:

Captain Douglas Galton.
The Right Hon. J. Stuart Wortley.
Professor Wheatstone.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

Colonel Tal. P. Shaffner.

Colonel Tal. P. Shaffner (Kentucky, United States of America), examined.

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3886. (*Chairman.*) I believe you have been acquainted with the subject of telegraphy for several years?—About 16 years.

3887. You have been chiefly occupied, I believe, with land lines in practice, have you not?—Both inland and submarine.

3888. For how long have you directed your attention to the question of telegraphic communication between America and Europe?—Since 1849 partially, and wholly since 1853.

3889. Perhaps you will give the Committee a description of your efforts for obtaining telegraphic communication between America and Europe? For example, you took steps to obtain a concession from Denmark?—Yes. In the early part of 1854 I united with parties in America for the purpose of consummating that object; and in the spring of 1854 I visited Denmark for the purpose of obtaining a concession of Greenland, Iceland, and the Faroe Islands, and obtained in August of that year a concession for 100 years for that purpose.

3890. Did not you always consider that that route was the one most likely to prove successful?—No, not until the retardation of the electric current in subaqueous conductors was clearly demonstrated. That discovery was made known by Professor Faraday at the Royal Institute in England.

3891. Had you made any attempts previously to 1854 with reference to a submarine telegraph between Europe and America?—Only in writings and in experiments in my own country.

3892. What were the terms of the concession which you obtained from the King of Denmark in 1854?—That I was, within 10 years, to construct the line, and the concession was to run 90 years thereafter, or for 100 years from the time of the concession. It also gave me exclusive rights in Greenland, Iceland, and the Faroe Islands, guaranteeing to me all the means in the power of Denmark to keep the line open for the impartial and free use and benefit alike of all nations, without any exclusive privileges on the part of Denmark. I believe that is the substance of the concession.

3893. You have subsequently renewed that concession, have you not?—There was a clause in the concession requiring me to expend 100,000 Danish dollars within three years. I expended more than double that sum, but there was a difference of opinion between the Danish Government and myself in regard to the manner or mode of proving that expenditure.

3894. What way was the money expended?—In diplomatic efforts in negotiating, in publications, and various other efforts for the promotion of the enterprise.

3895. The Danish Government did not admit that expenditure?—They did not deny it, nor did they admit it.

3896. (*Professor Wheatstone.*) Is your concession still in force?—Yes.

3897. What is the nature of the concession to Sir John M'Neil?—He has no concession.

3898. Do you know the nature of the agreement which has been made between the Danish Government and Sir John M'Neil?—He has the promise of a concession whenever I shall forfeit my concession.

3899. (*Mr. S. Wortley.*) As I understand, Sir John M'Neil's concession is subordinate to, or contingent upon, the failure of yours?—Yes.

3900. Is not there a certain sum of caution money that you have to deposit?—Yes.

3901. What is the amount of it?—100,000 Danish dollars.

3902. Does that represent 10,000*l.*?—£11,000 and a little upwards.

3903. Within what time is that deposit to be made?—It is to be made in a few days. I expect to leave this evening for that purpose.

3904. The limit has nearly arrived?—Yes, and I have the money to pay it.

3905. (*Chairman.*) Since you have obtained that concession you have, I believe, carefully examined the route between America and Denmark?—I have passed over the route. I chartered a ship out of my own means, and employed my own men; and being a man of limited means, I could not command the necessary appliances for the detailed survey that would be desirable. Hence my examination was not thorough—not so full as would be necessary.

3906. From what point on the coast of America would you propose to start?—Through Hamilton's inlet, latitude 54° 40′, or about that.

3907. Do you consider that that point is free from the rights of the Atlantic Telegraph Company?—After a careful examination of the matter as a question of law, I consider that we can pass through Hamilton's inlet without any infringement of the imaginary rights of the Atlantic Company upon the coast of Labrador.

3908. (*Mr. S. Wortley.*) You do not consider the shores of the inlet as the shores of Labrador?—Not beyond its territory—beyond the imaginary line.

3909. (*Chairman.*) That is to say, Hamilton's inlet runs inland beyond the imaginary line, which was granted to the territory of Newfoundland, by the former charter?—It runs many miles beyond the imaginary boundary of Newfoundland, upon the coast of Labrador; west, you may say.

3910. (*Mr. S. Wortley.*) Are you aware of any published map, which shows at once the line of the territory of Labrador, and the extent of the inlet, and showing the extent of the inlet to reach beyond the territory of Labrador?—I know of no authentic map that gives the boundary of Labrador; the Act speaks about longitude and latitude.

3911. (*Chairman.*) Are you aware upon what grounds the Government of Newfoundland claims a jurisdiction over Labrador?—Yes; I have not a memorandum of the various acts and re-enactments, backwards and forwards from the British Government and Canada to Newfoundland; there have been many Acts upon the subject.

3912. (*Mr. S. Wortley.*) There must have been an original charter, specifying the extent of the Labrador territory?—Yes; there was a charter by the King of England.

3913. I presume all the subsequent Acts of the Newfoundland legislature have been founded upon the charter; they can have no power beyond the limit of the territory granted by the charter?—A part of that territory has been transferred to Canada two or three times; it has been divided off, and first given to Canada, then brought back to Newfoundland, and then transferred back again. The alleged rights of the Atlantic Telegraph Company are bounded as follows;—starting at Anse Sablon, on the north coast of the strait of Belle Isle, latitude 51° 25′ north, longitude 57° 10′ west, draw a line due north to latitude 52°, and from thence to the mouth of Hudson's strait, latitude 60° 26′ north, longitude 64° 40′ west. This line will cross Hamilton's inlet, about 80 miles from sea. The inlet extends about 150 miles inte-

rior. The cable laid in the waters of this inlet, beyond the line above described, will not infringe upon the rights of the Atlantic Company on the Labrador coast. Besides, the line of jurisdiction strikes the ocean coast about latitude 56° 00′, longitude 59° 45′, and traverses the curve of the sea to near its northern point at the mouth of the Hudson strait, leaving some 210 miles of the Labrador coast beyond the line of jurisdiction before described, and westward of the Atlantic Company's rights. This sea coast is free and open to all who may wish to go there with a telegraph.

3914. (*Chairman.*) Assuming that you have power to land a cable in Hamilton's inlet, do you anticipate any physical difficulties in landing the cable there, and maintaining it?—Nothing more than is common to other parts of the ocean coast.

3915. What is the form of the entrance to that inlet? Is it protected on each side by ridges of rocks, or would icebergs be liable to ground in the centre of it?—I stopped some time at Hamilton's inlet, in the month of September. I examined the various bays, the coast, and penetrated into the interior, to study the character of the country and the people. The bottom of the bay is mostly a shelly formation, or shelly sand. I presume it is barnacle sand; it is a very white sand, and the mouth of the bay is very deep, running into the sea some 60 or 70 miles; it gets deeper and deeper; the width of that immense trench that enters the ocean I do not know, but the depth of it is over 350 fathoms. The mouth of the inlet is about 30 miles wide.

3916. When you say that it is a trench, you mean that it is shallower on each side for a certain distance?—Yes; above that trench there is a shelf of rocks projecting from the coast, probably about 25 miles to sea.

3917. Would that shelf effectually prevent the icebergs, which come down from the north, from touching the bottom of the trench?—I think so.

3918. That is to say, no iceberg could pass the shelf of rock?—I know of no iceberg that descends to the depth of the bottom of that trench.

3919. But assuming there is a shelf on each side of this trench, of course, though the iceberg could pass over the shelf, it could not touch the bottom of the deeper part below it?—That is true; I saw many icebergs aground, north of the entrance of Hamilton's inlet into the sea, and south of it. I sounded north of it, and found it to be shallow, 90 fathoms, while at this trench it is 300 and odd fathoms; but below was also shallow, so that icebergs grounding above the trench, melt there and become light enough to float, and, of course, will pass over the entrance of Hamilton's inlet into the sea, and ground again below. The current carries the ice south to the east coast of Newfoundland. This northern current is checked or thrown back by the superior power or force of the gulf stream, so that immediately east of Newfoundland the sea is composed of innumerable eddies. The iceberg and coast ice descends from the Davis strait, some passing into the gulf of St. Lawrence through the strait of Belle Isle, and the larger portion hugs or nears to the coast of Newfoundland, many times filling Trinity bay and the other arms of the sea penetrating the east coast of Newfoundland. The bay of St. John's is sometimes closed with ice for several weeks, and on this account the Atlantic steamers have been prevented from going to St. John's as a port of call. Navigators have seen large icebergs on or near the coast of Newfoundland, apparently stationary, and they supposed the bergs to be on the bottom, while, in fact, many times, they were only in the eddy of currents produced by the rebounding of the current from the gulf stream. The ice descending along the coast of Labrador does not, to a great extent, near the coast above the strait of Belle Isle. Between the ice and the coast there are some miles of clear water, along which vessels can navigate up and down, at least as far north as Hamilton's inlet, lat. 54° 40′ north. And the current descending from Davis strait does not immediately near the coast of Labrador, but is some 30 miles seaward. The tide current carries many icebergs nearer to coast, and many ground upon the shoals, which exist some 20 to 30 miles seaward. East winds sometimes force the bergs towards the coast, but the prevailing winds are from directions which prevent the bergs from coasting; and, besides, the water coming from the many rivers and bays of Labrador aid to keep them at sea. The shore or bay ice of Labrador cannot disturb an electric cable. This ice is not more formidable than the ice in the belts of the Baltic sea, where a cable has laid and been effective for the past seven years.

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I am confident that a telegraph cable, laid into any of the bays on the east coast of Newfoundland, will be more liable to injury from the ice than one laid into Hamilton's inlet, or any of the bays of the Labrador coast south of Hopedale, or latitude 61° north. I do not think there need be any fear of interruption by the ice upon either coast; it may be possible, however, that a cable laid in Trinity bay will be injured by icebergs; but on the other hand, the physical character of Hamilton's inlet is such that I cannot see any possibility to an interruption to a cable laid from the sea into that inlet.

3920. From your experience of American land lines, should you consider that there would be much difficulty in maintaining a line open during the winter from Hamilton's inlet to join the Canadian lines?—From what I could see of the country, there would not be near so much difficulty in maintaining a line there as in countries where I have maintained from 2,000 to 3,000 miles of lines, either as president or director, or in some other auxiliary position.

3921. Why not?—Because the country is a piney region, not subject to hurricanes and the great storms of wind that we have in the south-west, and not subjected to the sleets that we have, that tear our lines to pieces, nor to evil-disposed persons.

3922. Is that coast totally uninhabited, or are there Esquimaux?—The coast is uninhabited, but the interior is inhabited by Esquimaux.

3923. Would not the Esquimaux damage the lines?—No, not at all; judging from the general character that I obtained of the Esquimaux when I visited that country.

3924. (*Mr. S. Wortley.*) In what district do those heavy sleets occur?—We will take the entire country west of the Alleghany mountains. We have had at one time 1,500 miles swept from us almost in 24 hours, that is, 200 or 300 yards every mile or two.

3925. In the Western States, you mean?—Yes; westward of the mountains.

3926. (*Chairman.*) In Ohio, Kentucky, and Tennessee?—Yes; and North Missisippi, Missouri, and Illinois. The most westerly line that I ever constructed was from the Missisippi river to the western borders of civilization; that line I completed in 1852.

3927. Was that through Kansas?—Yes, it was before Kansas was settled; it was at that time all occupied by Indians.

3928. Assuming that a line could be satisfactorily laid out from Hamilton's inlet, to what point would you carry it on the coast of Greenland?—It would be improper for me to decide upon the precise place in advance of further examination.

3929. Or about what part?—About Julianshaab, latitude 60° 43′.

3930. I think you have taken some soundings between Labrador and Greenland?—Yes; I have sounded that part of the Atlantic. The depth of the water is from 1,800 to 2,090 fathams at the deepest part. I brought up bottom from various parts of Davis's Straits.

3931. Does the bottom consist of shells?—The nature of the bottom was somewhat similar to the bottom from Newfoundland to Ireland.

3932. Shells chiefly?—Yes. Not having examined the specimens with a microscope myself, I am not capable of giving the precise character of the bottom.

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3933. The specimens show that it was a bottom deposited under quiescent circumstances?—I have some of the bottom here (*producing a specimen*), this was taken about the middle of Davis's Straits.

3934. (*Mr. S. Wortley.*) What instrument did you use?—I used what we called Brooks's sounding apparatus.

3935. (*Chairman.*) Do you consider Brooks's sounding apparatus the best?—I am not capable of judging of that.

3936. (*Mr. S. Wortley.*) It is sufficient to bring up portions of the bottom?—Yes, it answered my purposes perfectly. I have many different specimens.

3937. Is that the instrument that Lieut. Maury used?—Yes; this is a late improvement.

3938. I suppose the bottom did not come up in that hard state?—No, quite soft.

3939. Was that specimen taken up at a great depth?—That was taken up at 1,840 fathoms, latitude 58° 20′, longitude 51° 50′.

3940. Did you find any current in Davis's Straits of any importance?—Yes; I found a northward current, about 80 miles west of latitude 61° Greenland coast. It would be as well to say Cape Desolation.

3941. Would not that current prevent the ice from accumulating on the coast?—Near the coast of Greenland I did not observe any current.

3942. Did you observe many icebergs on the coast of Greenland?—Yes; I observed in Davis's Straits perhaps a thousand icebergs, starting from the Gulf of St. Lawrence to my most northern point.

3943. Should you anticipate any difficulty in laying a cable from the presence of icebergs?—Not at all; those icebergs are from two to three or four miles apart, and sometimes 10 miles; we could run right along by them, and pay no attention to them.

3944. (*Mr. S. Wortley.*) In the month of September, when you were there, the coast was the freest from ice, was not it?—I do not know.

3945. Is not September the height of the summer in those regions?—No.

3946. When does the ice first melt and come away with respect to icebergs?—The icebergs are probably the thickest in January and February, and then they thin off until the return of that season again.

3947. Are you speaking from actual observation or from the result of inquiry?—Having only been there in part of September and October, I can only speak from inquiry.

3948. (*Chairman.*) When does the floe ice commence?—In January.

3949. Was there floe ice when you were there?—No.

3950. Was the coast perfectly clear?—There were a few icebergs, but no floe ice. I was informed that the floe ice was nearly entirely gone by the 1st of June. This floe ice does not go near the coast; it is generally some few miles distant from the coast, so that between the floe ice and the coast there is open sea.

3951. But still, if there is floe ice a little distance off the coast, it would be very difficult to lay a cable. I presume you would have to steer through the floe or be carried away with it?—With a steam vessel you can pass through openings in the floe ice in that latitude at nearly any time.

3952. But assuming you had had a cable on board at the time you were in Davis's Straits, could you have laid it with perfect facility from Hamilton's inlet to Julianshaab with a steam vessel?—With a steam vessel there would have been nothing in the world in the way.

3953. (*Mr. S. Wortley.*) What year were you there?—Last year, 1859.

3954. (*Chairman.*) You say that you would land a cable on the coast of Greenland, somewhere near Julianshaab. I presume in an inlet situated like Hamilton's inlet, with shelves of rock on each side, with a deep trench in the centre?—Yes.

3955. But the precise inlet you have not yet selected?—No, I have not. I saw many inlets that would answer. The bay at Julianshaab never freezes, and is clear of ice all the winter. Outside at sea in January and February there is much ice floating, so thick that sailing vessels cannot get into the bays. The temperature of the weather at Julianshaab is maximum cold, thermometer 16° Reaumur, average 10°.

3956. What is the minimum in winter?—I have not that down; it is never excessively cold; in summer 10° to 12° plus.

3957. Do you think there would be no difficulty from the anchors of fishing vessels or other vessels in the inlets near Julianshaab?—No; there are not many of them.

3958. Assuming that you land your cable at Julianshaab, would you take it overland to the next point?—That, in my opinion, is the best plan, upon present information.

3959. Have you been over that country to any extent?—I have penetrated further into the interior than perhaps ever had been done before north of Julianshaab.

3960. What description of country did you meet with?—It is mountainous perhaps for some 30 or 40 miles in the interior, covered with grass, green shubbery, and small spruce pine, but very dwarfish.

3961. Not a rich soil?—It is too mossy to be considered a soil at all. In the interior, northward, I traversed some very extensive ice fields; the ice was flat and easy to walk over.

3962. To what point on the coast would you direct your land line on the east coast of Greenland?—About the same latitude, or probably a little south of the latitude 61°.

3963. Do you anticipate that you would find a harbour there equally convenient for landing the cable?—I saw open bays there, and hills of the same character as those upon the west coast, and no ice.

3964. Did you meet with no ice along the east coast when you were there?—None.

3965. Has not the opinion generally been that there is always a large quantity of ice on that coast?—Yes, by persons uninformed as to the facts.

3966. Can you account at all for the fact of your not having found any ice?—Yes, because it is not common for ice to remain on that coast, and during parts of every year there is none there of consequence. I do not know that any vessels have ever approached there at that season of the year, nor is it common for vessels to go there; they imagine that it is icebound.

3967. How has the opinion arisen that it is icebound?—I suppose from the fact that in certain seasons of the year the floe ice runs from the north round by the Greenland current, and vessels in the Atlantic have observed this ice at the seasons of the year that ships frequent those latitudes, but I know of no vessel visiting the east coast within the past quarter of a century. There never was a steam vessel there.

3968. That is the spring I presume?—Yes, the spring of the year.

3969. Do vessels never attempt to go there in the autumn?—I never heard of one attempting to go there in the autumn.

3970. When was the Danish survey of that coast made?—Graah's survey was made about 1830; it commenced in 1828, I believe.

3971. Would not his vessels have remained both the autumn and spring there?—Graah went very little prepared for a survey; he had no vessels, and at the season of the year when it was desirable to be there the winds were against him. He had skin boats, which were wholly unfitted for such a service. I have travelled many miles in those skin boats, and know the danger that there is in going in the ice with them. Besides that, they are used by Esquimaux, and the Esquimaux are a timid and cowardly race, so far as such an enterprise may require their service.

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That there are difficulties in regard to the ice is possible for much of the year, but for some four or five months I do not apprehend that there is any danger or any difficulty in approaching the coast and the open bays upon the east coast of Greenland with a steamer, but it is very uncertain about getting there with a sailing vessel for want of wind.

3972. What is the reason of the difference?—The difference is that the seasons generally are against it; when the coast is free of ice, the winds are generally from the coast.

3973. Do the winds blow off the coast at that season of the year?—Yes, it did when I was there.

3974. In which direction does the current set?—The current sets from the northward, coming between Iceland and Greenland, and sweeps round Cape Farewell and Statenhuk, making a considerable eddy down to the southern end of Greenland.

3975. Where is Statenhuk?—Statenhuk is at the southern part of Greenland, near Cape Farewell, but east of Cape Farewell.

3976. And the current is northward from there?—Coming from the northward between Greenland and Iceland.

3977. It is a sort of eddy down the coast?—Yes, and there is an open sea between the east coast and the floe ice. The current sweeps round the coast, comes round Cape Farewell, and goes northward into Davis's Strait. What I have stated with regard to the ice on the east coast of Greenland, is only an opinion based upon reading and upon observation the short time I was there myself.

3978. When you were there yourself you found it perfectly free from ice?—Yes.

3979. And no icebergs?—No icebergs at all; this was in October 1859.

3980. Was the weather fine?—The weather was very pleasant; my family could be on deck every day.

3981. Is the water deep near the east coast of Greenland?—On the east coast I think the water is deep. The east coast of Greenland, south of latitude 65° north, forms a crescent to near Statenhuk, latitude 59° 40′ north. The sea within this crescent is eddy water, and floe ice is sometimes forced from the Arctic current by east winds into the eddy. The prevailing winds during the existence or presence of the floe ice in the Arctic current are from the west, north, or from between those points of the compass. These latter winds drive the ice, of whatever nature, from the coast eastward to sea, into the Arctic current, and it is then swept southward around the south end of Greenland. Within the crescent, on the east coast of Greenland, I am of the opinion that the water is not deep, and that the bottom is soft mud. Such is the case wherever an eddy exists. The bays of Greenland, both upon the east and west coasts, bring to sea more or less water, which has a milky appearance, caused by the fine particles of insoluble matter held in suspension. These particles are eventually deposited on the bottom of the bays, or at sea near their mouths respectively. It is natural to suppose that the bottom of the bays and the sea near the coast is deep mud, and that a telegraph cable laid thereon would not be disturbed. The currents are not strong enough nor deep enough to reach the cable, and there will be no ice there that can descend to the bottom. The Esquimaux traders living upon the east coast of Greenland from time to time go south to Stattinhuk, and return between the coast and the floe ice. Their boats are made of skin and very frail. They cannot be forced through even small ice, because of their tenderness and liability of immediately sinking. Capt. Graah had to depend upon these boats when he was exploring that coast 30 years ago. We can scarcely conjecture how much more he would have accomplished, could he have had the more modern improved small boats. I think the bottom of the sea gradually descends from the eastern coast to the Arctic current, say, about latitude 62° 06′ north, longitude 32° 21′ west, where I sounded, and found the depth to be 1,540 fathoms, and a very soft muddy bottom. The ascent from this place to the coast, some 200 miles, would be so gradual that it would be but little more than a plain.

3982. And the bottom is not rocky?—The bottom is very soft mud.

3983. Close to the shore?—As you get nearer to the shore it is more of this barnacle sand.

3984. How did you ascertain the depth of the mud?—From the striking of the mud by my sounding rod; the cord and the rod went perpendicularly into the mud, and the cord had mud on it. The soundings in latitude 62° 40′, longitude 29°, at 1,000 fathoms deep the mud was so soft that the ball got fastened to the rod, and we could not detach it, and we had to draw it back again into the boat.

3985. You brought up water upon that occasion?—I brought up any quantity of mud. A specimen of the bottom has been examined by Professor Ehrenberg, of Berlin, and I will read you a letter which I have received from him on the subject. He says, "I occupied myself at once with the "interesting bottom samples, and hope to finish "them in the course of January, and review the very "numerous forms of life-being therein. The small "numerous testaceous animals still show, almost in "every instance, in the shells, dried, weak bodies "of animals, and therefore ought to be existing alive "in the bottom in such a depth. Till now I have "been enabled to define with names about 50 species "of them. Some of them are new species I never "heard of before; but many are spread far over all "seas. In case you intend to send to me other "samples of the deep's bottom, derived by yourself "or under your direction, I shall most gladly exa-"mine them comparatively, only I must inform you "that I cannot finish such a review in a couple of "days. I want months for each locality and speci-"men, as a superficial naming is useless. Further, "I would request you to give me some special infor-"mation about the method of raising the mud from "the bottom of the sea, and the probable certainty of "the soundings, and the greatest depths noted par-"ticularly. Besides, should you be in connexion "with other ships which were further in the north, "in Baffin's Bay, and so forth, effecting considerable "soundings and raising bottom samples, such ones "would be very desirable to me. Your labour and "zeal, in raising and sending me specimens, it will "be my duty to acknowledge publicly in due time. "Individually, I would like to get, for necessary "comparison, some water samples of the surface of "the ocean there, not as water, but sediment, and "dried or evaporated water. The raised up not or-"ganic sand, containing very numerous organic and "some particular remains of sponge species, is re-"markable by its great quantity, so far as such "quantities are not found but on sand banks. The "sand itself has two decided characters. It is, first, "no rolling sand, but fragment sand, broken, dis-"solved stones of mountains. All the granules are "not round ones, but with acute sides. Secondly, "this granite sand consists of much glimmer and "quartz, with green crystal fragments, which might "be hornblende, were there particles of pumice-"stone, but which are not at all therein. One could "be inclined to judge is pyroxen that which appears "to be hornblende. It seems quite decidedly to be "broken, dissolved granitical mountains in that "depth." I expected to have learned that the depth of the sea there would produce evidences of volcanic elevations, but it seems that there is nothing to indicate that fact.

3986. And there is nothing in the bottom of the sea to indicate volcanic action, between Greenland and Iceland?—Nothing that I was able to find.

3987. Did you take many soundings between the two places?—I took several, but few are reliable, and only those few do I report.

3988. Did you take soundings on approaching Iceland?—Yes.

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3989. Did you find the same bottom?—I found the same character of bottom, but the bottom seemed to me to ascend up to Iceland.

3990. By a gradual inclination?—By a gradual inclination, commencing at 1,540 fathoms, latitude 62° 60′, longitude 32° 21′, coming up to Iceland.

3991. Then, according to that account, the descent would be very precipitous immediately from the coast of Greenland, and then by gradually shelving rise up to the coast of Iceland?—I cannot say that it would be so. It is possible that the approach to Greenland may rise gradually, as there is no current there. The bays penetrate into the interior, so that it might be possible that the ascent from the deep sounding of 1,540 fathom point might be very gradual; but that fact I do not know; I cannot say that it is so; I only say that it is possible.

3992. At what point of the coast of Iceland would you land the cable?—Somewhere near Reikiavik.

3993. In the harbour of Reikiavik, or adjoining it?—The exact point I am unable to determine.

3994. Do you consider that the bay in which Reikiavik is situated would afford complete protection from floating ice and icebergs?—I have seen many Icelanders who assure me that there is nothing to be feared on account of the ice in the bay of Reikiavik.

3995. Is the bay of Reikiavik frozen over in the winter generally?—Never; nor does the floe ice go into the bay of Reikiavik.

3996. Is there not a great headland on the north which protects it?—Yes.

3997. In which direction does the current set at that part of the sea approaching the bay of Reikiavik? Does it set towards the north, or is there any current?—The current is between Greenland and Iceland, nearly half way, descending southward.

3998. There is no current near Iceland?—There is no current near Iceland, except the interior current that runs out from the little streams. There is no ocean current.

3999. In landing a cable at Reikiavik you would carry it, I presume, over land for some distance in Iceland?—Yes; I think it will be better to carry it overland to somewhere about Portland.

4000. Portland is the southern point of Iceland, is it not?—Yes; it is on the south side of Iceland, about longitude 19°. The determination of these points, however, ought to be fixed by a more careful examination.

4001. Is the bottom of the sea near Portland rocky or soft and sandy?—It is sandy.

4002. Are you aware of any basaltic formation running out from the southern coast of Iceland for some distance into the sea?—I suppose that there is running out from the south-west part of Iceland, but to what extent to sea I have no conclusive proof.

4003. You have merely, I suppose, the Danish maps?—That is all.

4004. Those maps show shallow soundings along a certain part of the coast, and that part of the coast you avoid?—I avoid that entirely.

4005. (*Admiral Fitzroy.*) What size was the vessel you were in?—197 tons, American measurement.

4006. Was it a steamer?—A barque—a sailing vessel.

4007. Were your deep soundings taken from your vessel or from a boat?—Some were taken from the vessel, all of which I have had to reject. The only reliable soundings were those taken from a small boat with the men in it, and the oars at work to keep the boat in one position.

4008. Were you in the boat yourself?—Not in every instance, though at the most important I was in the boat myself.

4009. What sized line was used in the deep soundings?—I think it is the tenth of an inch in diameter (*producing a specimen*).

4010. (*Chairman.*) Was it supplied to you from the Navy yard of the United States Government?—Yes.

4011. (*Admiral Fitzroy.*) Is this the line recommended by the United States Government and generally used by sounding vessels?—It is one of the lines recommended. There are two lines I believe recommended.

4012. Is it the same kind of line that was used and recommended by Commodore Perry some years ago?—That I do not know.

4013. What weight did you use?—A 64-pound cannon ball, also obtained from the United States Navy department. All my apparatus was made by that department.

4014. Was your sounding apparatus similar to that used by Lieutenant Lee, by Lieutenant Berryman, and other officers in the United States Navy in recent sounding expeditions?—It was the same with a modification of the detaching lever.

4015. Brooks's apparatus?—Brooks's apparatus.

4016. The same which has been used recently to sounding across the Pacific between California and the Sandwich Islands?—The same.

4017. And since then from the Sandwich Islands towards Japan?—I do not know that.

4018. It has answered, as far as you are aware, under all the circumstances in which it has hitherto been employed as a sounding apparatus?—I cannot speak about others. In all my soundings from small boats it has answered perfectly.

4019. Did you in any instance use what I may call the old-fashioned method of sounding by a heavy lead or heavy weight which was to be brought up again attached to a large line?—I did, but I do not report any soundings taken by the old-fashioned method.

4020. If you were to set out on another sounding expedition, would you use Brooks's apparatus after having tried other methods, or any other method?—Brooks's apparatus having succeeded perfectly in my efforts, I would not be willing to change it for any other.

4021. Did the line part on any occasion?—It did once and only once within a few inches of the rod just as it was thrown over the boat; it had not got out of sight in the water.

4022. (*Mr. S. Wortley.*) That was from some accident to the line?—Yes, it was from carelessness; it had been run over a nail or something that cut it.

4023. (*Admiral Fitzroy.*) Speaking merely from estimation, about how many deep-sea soundings did you take between Labrador and Greenland?—Between Labrador and Greenland there are only three deep-sea soundings that I would put down as absolutely certain.

4024. Was the position of the vessel well ascertained at the time of taking those soundings?—Yes; clear days.

4025. In your opinion, is there any greater depth between Greenland and Labrador than about 2,000 fathoms, within 100 or 200 fathoms, more or less?—I think that a maximum depth was obtained by me of 2,090 fathoms. There may be 50 or 100 fathoms more taking in a little gutter in the sea. I do not believe there is any plateau or smooth floor at the bottom of the sea; it is all in ridges and cavities.

4026. About how many reliable soundings in deep water do you think were taken between Greenland and Iceland?—Certainly two.

4027. Were they equally divided as to distance?—Not exactly; all that I took on that part were satisfactory.

4028. And your impression from those soundings is that the greatest depth between Greenland and Iceland may be about 1,540 fathoms?—Yes.

4029. The east coast of Greenland that you visited was considerably to the south of that part of the coast of Greenland which is said to be blocked up during a great part of the year?—Very far south.

4030. That part of the coast which is generally blocked up by ice during the greater part if not all the year is the part where the colonies were supposed to have been destroyed in former centuries, is it not?—Yes, about latitude 65° or 67°.

4031. That part which has been generally referred to when speaking about the east coast of Greenland being blocked upwith ice, is to the north of the part of Greenland on the east side which you visited?—I speak of no part of Greenland north of latitude 61°.

4032. Referring to Iceland, are you aware whether any banks or comparatively shallow soundings extend from Iceland considerably towards the south, say 50 or 100 miles or more?—I am unable to say.

4033. When visiting the sea south of Iceland, did you find any strength of current to interfere with the management of a ship or with laying telegraph wires where you pleased?—We supposed that we felt the gulf stream current, but we had no satisfactory proof of it.

4034. At what strength might you have felt it in 24 hours, or at what rate was the current?—Not being a nautical man, I am unable to say, but it was very feeble. At that time we were in heavy gales.

4035. Your experience of Iceland seas, as far as you are personally aware, has left no impression on your mind of any current of importance?—None.

4036. (*Chairman.*) Upon what ground did you determine whether the soundings were successful or unsuccessful?—When the line went out we could feel when the ball had reached the bottom. Nearly every time, however, we had to shake the ball from the rod, because the mud getting between the ball and the rod made a jam, and we had to pull up the ball a few feet from the bottom, and jerk the cord to shake it off. The sea currents being very feeble and shallow in those seas, it was not difficult for us to realize the fact that the soundings were correct.

4037. In taking the soundings with Brooks's apparatus, did you ever haul the line taut before you let go the shot at the bottom?—Yes, we pulled it up perpendicularly, and let it down gradually and shook it off. We had to do that because the holes in the balls were not large enough.

4038. You could not have pulled the line up without the ball had shaken off?—We pulled a ball up once, and it is the only instance that I know of in which a ball ever was brought to the surface again from a depth of 1,000 fathoms; the rod, the ball, and the twine were covered with mud.

4039. From what depth was that?—That was 1,000 fathoms; in latitude 62° 40′, longitude 29°; that ball remained upon the rod; it could not be detached, and we brought it back again; it was a 64-pound cannon ball.

4040. As a rule, is it not the case that as soon as you pull the line taut, the lever detaches the ball?—No, when the line is slack.

4041. As a rule, you cannot haul the line taut between the bottom and the boat, because, as soon as the line reaches the bottom, the weight is detached, and therefore as soon as you begin to pull taut you cannot tell whether the weight is touching the bottom or not, at a great depth?—As long as the ball is on the rod, the line remains taut; the moment that it is detached, then the line becomes slack.

4042. The moment the weight touches the bottom the ball becomes detached?—Yes, except as before explained.

4043. Therefore you can only reckon from the sudden slackening of the line at the top that you have reached the bottom; you cannot make any fresh trial by moving the ball up and down, which you could do if the ball were tight upon the line?—That is very true, but we get accustomed to the going out of the line, so that we can tell.

4044. You cannot tell, I suppose, to within 20 fathoms?—Possibly not.

4045. Or 50 fathoms?—50 fathoms we can. If there is a slight current, there will be 10 or 20 fathoms of slack.

4046. (*Captain Washington.*) Did you keep a record of the intervals of the running out of each 100 fathoms by a watch?—By two watches and two men.

4047. Do the intervals occur at any regular proportions?—The first 100 fathoms ran out in one minute; the second 100 fathoms in one minute five seconds; 300 fathoms, one minute twenty seconds; and 1,000 fathoms, three minutes. *Colonel Tal. P. Shaffner.* 14 March 1860.

4048. Have you seen the report of Capt. Dayman's soundings in the Atlantic?—Yes.

4049. In his soundings it was observed that there were regular intervals in the running out of each 100 fathoms; have you observed the same in your soundings?—I have not compared them; the speed of the descent of the ball depends entirely upon the apparatus letting the ball out; sometimes, if the axles of the reel were well greased, it would go with greater rapidity than at others.

4050. Did you find any ice on the eastern coast of Greenland?—I did not see a piece of ice as large as your body. I was only at the south-east coast, I was not on shore.

4051. How far north did you go?—About 61°. I will tell you the reason why, possibly, there was no ice there this time; the season was too early for ice. There was not as much ice last winter as common. I am now speaking from information derived from the settlers; the winds from the west drive the ice, if any, into the ocean, and it is possible that in that manner the coast was free from ice at the particular time that I was there.

4052. But the ice does occasionally form there in very large quantities, does it not?—I do not know, but I think not upon the coast.

4053. If it did would that damage a cable?—That would depend entirely upon the extent of the ice; from all I could learn from the people, and from the fact that the trading Esquimaux come down to the south end of Greenland every year, it is possible to enter there at almost any season of the year with a steamer.

4054. (*Mr. S. Wortley.*) At what time of the year do you consider that coast the freest of ice, in the latitude you pass through from your inquiries?—I was informed that the months of August, September, and October were the months that were the freest.

4055. Not July?—July is often free.

4056. Because I found in a description of the late expedition of the "Fox," written by a gentleman who has appeared before this Committee, Mr. Allen Young,—he says, "On the night of the 13th of "July, we were becalmed, and on the following day "we steamed slowly to the north-westward, amidst "countless numbers of sea birds. At daylight, the "coast of Greenland showed out in all its wild mag-"nificence. Cape Farewell bore north 45° east, distant "25 miles; but from the peculiar formation of the ad-"jacent land the actual cape is difficult to distinguish. "Hitherto, we had not seen the Spitzbergen ice, and "we hoped that we might follow the coast round to "Frederickshaab without obstruction, but in the course "of the forenoon a sudden fall in the temperature of "the sea, with a haziness in the atmosphere to the "northward, indicated our approach to ice. Straggling "and water-washed pieces were soon met with, and in "the evening the distant murmur of the sea, as it "broke upon the edges of ice floes, warned us of our "being near to a pack. We made but little progress "during the two following days, the winds being from "the northward, and a dense ice fog rolling down from "the pack. On the 17th, Frederickshaab bearing "north 28° east, distant 50 miles, we determined upon "endeavouring to push through the pack, and after "being at times completely beset, and with a constant "thick fog, we escaped into the inshore water with a "few slight rubs, having been carried by the drifting "body of ice, nearly 30 miles northward of our "port." You did not in September see any body of floe ice like that?—I did not see any floe ice during the whole voyage. [From the evidence above quoted it will be seen that the coast at Julianhaab was free from ice. The floe seen by Mr. Young was north of that place.]

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4057. (*Captain Washington.*) You are speaking of the year 1859?—Yes.

4058. (*Mr. S. Wortley.*) Are you acquainted with the Spitzbergen current that is mentioned in that narrative?—I am acquainted a little with what is called the Arctic current.

4059. That comes down to the east coast of Cape Farewell and turns to the north?—Yes.

4060. According to that it is going round the south point of Greenland?—At the south end of Greenland there is an eddy, I think, that goes south about 10 to 15 or perhaps 20 miles. Ice gets in there, and cannot very well get out; it goes round and round and is broken up, and hence we saw some small pieces of ice there.

4061. What is the width of the mouth of Hamilton's inlet?—I think it is about 30 miles.

4062. Is there anything to prevent icebergs being driven into the inlet by a north-easterly wind?—A north-easterly wind could not very well drive them in.

4063. Hamilton's inlet extends from north-east to west, does not it?—It is rather in that direction, but that is not the exact point of the compass. Icebergs can be blown into that or any other bay, that is if the wind was to come with the tide, united together they could take a small iceberg probably into the bay. Ordinarily, I think, there is a current in the inlet running into the sea that would prevent ice from coming in there.

4064. There is a great current running out?—Yes.

4065. Is there any tide?—Yes, there is a tide.

4066. Do you know whether the tide runs up to the head of that inlet?—No, it does not run to the head, it affects the head, it is like the Mississippi river where the tide does not run as high as Vicksburg, and yet the tide below influences it there.

4067. (*Chairman.*) Did you take any soundings between Iceland and the Faroe Islands?—I could not take any reliable, it was at a time when there were those heavy gales which wrecked so many vessels upon the south-coast of Great Britain. I was in all those gales.

4068. At the time the "Royal Charter" was lost?—Yes.

4069. (*Capt. Washington.*) Where were you then?—We were on the 26th between Greenland and Iceland. I was there tacking about. During the other heavy gales in November, between Rockall and Iceland.

4070. (*Admiral FitzRoy.*) Did those gales reach you?—I was in them.

4071. (*Chairman.*) Do you anticipate any difficulty in landing a cable on the Faroe Islands?—None more than is common to any sea shore. There is a difficulty in landing a cable anywhere.

4072. But there is no ice?—There is no ice.

4073. (*Mr. S. Wortley.*) What is the character of those islands—are they mere rocks, or are they habitable?—Most of them are inhabited. I think the Strom Island is the best.

4074. (*Chairman.*) Do you anticipate any difficulty near the southern coast of Iceland from volcanic agency?—None where I would advise the landing of a cable.

4075. You think all difficulties of that nature can be avoided by selecting your place of landing?—Yes; it would not, I think, be advisable to land it at the south-west part of Iceland. I would come more east, where there is no likelihood of finding any volcanic action.

4076. Or east of Portland?—East of Portland or about Portland, longitude 19°.

4077. Do you think there would be any difficulty from the grounding of ice there in the winter?—There is no ice there at all.

4078. Is it entirely kept clear by the current from the gulf stream?—No ice can get there very well.

4079. Why could it not come?—There is nowhere for it to come from. It cannot come from the north.

4080. Does the current prevent its coming from the north? Is there a continual current from the south setting along that coast?—The current from the north takes the ice from Spitzbergen, for example, southward; it takes the ice between Iceland and Greenland. Sometimes it comes upon the north part of Iceland, and it has once or twice come down upon the north-east coast as far as Berufiord.

4081. (*Captain Washington.*) You are speaking of the summer, and not of the winter?—I am speaking of ice in the icy time.

4082. In winter would there be no ice off Portland?—No.

4083. Is that certain?—That is what I have been informed by Icelanders.

4084. Are you aware that it is in latitude 63½° north?—I presume, as I am looking at the map, I can see that there may be a little shore ice. When I speak of ice I mean ocean ice—ice floes or icebergs.

4085. How many miles off Iceland do you consider that ice forms in winter from the shore?—Not having been there I cannot say, only from information derived from others; there is comparatively no ice there at all.

4086. Does your information lead you to believe that the ice extends one mile off the shore?—No; I have seen many Icelanders who have lived there all their lives nearly. I have recently had an interview with the Governor of Iceland and with members of the Iceland assembly, the Diet, and I have taken a great deal of pains to see Icelanders in Copenhagen who have lived there for many years, who were familiar with the character of the sea about Portland and Reikiavik. I make a distinction between my own observation and that which I get from others.

[As additional evidence relative to the formation of ice upon the coast of Iceland, the following letters may be considered; viz.—

Letter from Arnljot Olafson, Member of the Icelandic Diet.

"I beg now to answer your questions concerning "the freezing of the sea at Reikiavik and at Portland (Dyrholar).

"As regards Reikiavik, I am well acquainted, "having sojourned there, attending its high school in "the years from 1845 to 1850. I can certify that the "sea does not freeze there a few fathoms from the "shore.

"I have never been to Portland, but I have spoken "with several respectable gentlemen, from that "country, amongst whom, a son of the magistrate of "that place, and they have asserted that the sea never "freezes on that coast. I have never heard of the "sea freezing on the coast of Iceland mentioned, "except in some very small bays.

"Reikiavik is situated 64 8′ north latitude; "average temperature +1·2, Reaumur +9·6 in "summer. The difference betwixt high and low "water, 12 feet."

Letter from Mr. Olaf Gunlogsen, Dr. of Philosophy at Copenhagen.

"In regard to your questions whether the bay near "Reikiavik and the sea outside of Dyrholar, commonly known as Portland, freezes in winter, I am "fortunate enough to be able to give you decisive "answers.

"Having been born near Reikiavik, and having "spent many years on the spot, I can with certainty "assure you, according to my personal experience, "that the bay of Reikiavik is never covered with "ice to interfere with navigation. There may, "perhaps, for a few fathoms from shore, form a little "ice-crust, but none of consequence. During a "single and an uncommon winter, occurring, perhaps, "once in a century, the inlets of the bay of Faxebay "have been frozen.

"In regard to the sea near Dyrholar (Portland) "I have learned from two students at the University "in Copenhagen—both reared in the district—that "the sea outside of Portland never freezes, and any "representation to the contrary is incorrect, and "without any foundation whatever. This statement "must be evident to any one acquainted with the "nature of the south coast of Iceland."

Letter from Mr. Gisli Brynjulfson, of Iceland.

"In answer to your questions whether or not the "bay of Reijkiavik and the south coast of Iceland "freezes during winter, I have the pleasure to inform "you as follows; viz.—

"From my own personal knowledge and experience I can assure you that the bay of Reijkiavik "has never been frozen to any extent to interfere "with navigation.

"In regard to Dyrholar (called by foreign sailors "Portland) I have never been there, but I have "taken some pains to ascertain the facts; and, from "all that I can learn, I have sufficient reason to believe that the sea around it never freezes, the "southern coast of Iceland being, as everybody knows, "always open to the Atlantic Ocean."

Letter from Mr. H. P. Targesen, and concurred in by E. Feverson, J. M. Christensen, and J. P. J. Bryd, Icelandic Merchants at Copenhagen.

"In reply to your favour I have the pleasure to "inform you that the sea from Portland and the south "coast of Iceland to Reijkiavik has never been frozen, "except perhaps in occasional places a few fathoms "from shore; and that the bay of Reijkiawik or Fax-"bay never has been frozen or filled with ice any "year. Very seldom has there ever been seen "Greenland ice off this part of the coast of Iceland, "and then only a few pieces."

4087. Are the people tolerably civilized in Iceland?—Yes; you will find as good society in Iceland and in Greenland as in any part of the world.

4088. Will you be good enough to give the exact position and depths of your deep sea soundings, in the form of a tabular statement?—Yes, but the time of descent of the cannon ball for each hundred fathoms can not always be the same. Circumstances are not common in taking deep-sea soundings. Each one requires the exercise of some new judgment. The rising and descending of the small boat on the waves required judgment in the application of the break to the reel. My apparatus was plain, like the ordinary well windless. In the 1000-fathom sounding the time of descent is greater than those given in the preceding; this was owing to the fact that I had the break applied, because I was confident the sea was not very deep at that place. When the spindles of the axle were well greased the ball could descend more rapidly. I do not think that the correctness of my soundings can be determined by the time of descent. We got so accustomed to the business that we could feel when the ball was on or off the sounding rod. Although I have made the subject of sounding the seas my study for some years past, yet I found that the realities of the service can only be appreciated by those that do the work.

I annex the time of descent of four soundings taken by me, two between Labrador and Greenland, and two between Greenland and Iceland. The following depths can be depended upon as correct, excepting perhaps the third (2,090), which may be less. We got bottom, but it was a rough sea.

Latitude	58° 20′	Longitude	51° 50′	1,840 fathoms
"	59° 12′	"	51° 45′	1,833 "
"	61° 05′	"	50° 27′	2,090 "
"	62° 06′	"	32° 21′	1,540 "
"	62° 40′	"	29° 00′	1,000 "

The positions of the ship were reckoned at noon some of the soundings were later in the day.

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Fathoms	Sept. 19th. Lat. 58° 20′ Lon. 51° 50′ 1,840 Fathoms.	Sept. 21st. Lat. 61° 05′ Lon. 50° 27′ 2,090 Fathoms.	Oct. 18th. Lat. 62° 06′ Lon. 32° 21′ 1,540 Fathoms.	Oct. 19th. Lat. 62° 40′ Lon. 29° 00′ 1,000 Fathoms.
	m. s.	m. s.	m. s.	m. s.
100	55	1.00	45	1.00
200	1.00	1.00	45	1.05
300	1.05	1.12	1.05	1.20
400	1.10	1.28	1.20	1.40
500	1.20	1.35	1.30	1.55
600	1.30	1.40	1.35	2.20
700	1.35	1.45	1.40	2.30
800	1.40	1.53	1.50	2.30
900	1.55	2.00	2.00	2.45
1,000	2.00	2.08	2.08	3.00
1,100	2.10	2.15	2.15	
1,200	2.20	2.18	2.23	
1,300	2.30	2.27	2.30	
1,400	2.31	2.37	2.40	
1,500	2.37	2.35	2.45	
1,600	2.42	2.40	1.15*	
1,700	2.50	2.50		
1,800	2.55	2.58		
1,900	1.05*	3.08		
2,000		3·20		
2,100		3·40		

4089. (*Chairman.*) Between Iceland and the Faroe Islands, do you anticipate any difficulties with regard either to the nature of the bottom, the currents, or any other causes?—I do not anticipate any difficulty, though I am not informed sufficiently on the subject.

4090. Are you aware of any reliable soundings having been taken between the Faroe Islands and Iceland?—There have been some soundings taken near those coasts, but I do not place much confidence in them.

4091. Were they Danish soundings?—I do not know.

4092. I believe you had not time to examine the Faroe Islands very closely?—I did not examine the Faroe Islands at all; my knowledge of the Faroe Islands is obtained from the governor of those islands, and members of the Danish Parliament, with whom I have been very much in communication during the last past winter.

4093. Are you at all acquainted with the geological formation of those islands?—No; but that is a well-known fact.

4094. From the Faroe Islands, what course do you take?—I think I would come to Scotland.

4095. Direct to Scotland?—Yes.

4096. Should you take a line also to Denmark straight, or through Scotland?—I would not like to answer on that subject just yet, as there may be some rivalry in the determination of the question.

4097. Would you prefer to take a line direct to the Faroe Islands and to the coast of Norway, or would you take it round through the Shetland Islands to go to Denmark, assuming that you were going to Denmark?—It is possible that we may take both, but the determination of that question properly belongs to the company hereafter.

4098. I assume that your concession would require you to unite America with Denmark in some way or other, and not only to pass through the Danish possessions of Greenland, Iceland, and the Faroe Islands?—The concession contemplates that we shall get to Denmark by some route either through Scotland or by Norway.

4099. Your examination of the route has confined itself entirely to the western portion of the route as far as Iceland?—Yes.

4100. Upon the eastern part of the route no great difficulties need be anticipated, I imagine, from the ice?—None from ice.

* Fractional.

Colonel Tal. P. Shaffner.

14 March 1860.

4101. (*Captain Washington.*) Are not the Faroes more in the line of the gulf stream than Iceland, and is not the south coast of Iceland blocked with ice in the winter months, and if not, why?—Yes, the gulf stream surrounds the Faroes. The south coast of Iceland is never blocked with ice. I cannot give any reason why the south coast of Iceland is never blocked with ice, except that it is not cold enough to freeze but little, and that there are no winds, nor currents to take foreign ice there.

4102. (*Chairman.*) The west coast of Norway does not freeze much, I believe?—I am not thoroughly informed upon that subject except from publications.

4003. Have you formed any opinion as to the class of cable you would use for a submarine line for this particular route?—My opinion is in favour of light cables.

4104. With a certain amount of protection on the shore ends?—Yes, always, whether it is rock shore or shingle shore, because the surf will soon wear a cable out among the shingles. I did not find that the Greenland Arctic current was very deep; I have reason to believe that it is quite shallow; that is the current running between Greenland and Iceland.

4105. (*Captain Washington.*) Setting in what direction?—It goes from the north-east to the south-west.

4106. Setting to the south-west?—Yes.

4107. At about what rate does that current run? how many miles a day?—I do not know; my information upon that point depends upon the opinion of the captain.

4108. Was the current sufficiently strong to make the captain think that the vessel was carried out of the position that he expected she would be in?—No.

4109. How deep do you think the current was?—The captain thought it was 30 or 40 feet deep.

4110. What reason had he for so thinking?—In putting out our lines for some time when we were in a calm.

4111. The boat was driven away to the south-west?—Yes, we were in that current in a calm, for instance, and then it was easy for us to see whereabout the current ran. I am no nautical man, and consequently the information I give is from hearsay. I judge about the depth because I put out plummets with boards on them, to see what effect it would have, and I could almost have them perpendicular; that was right in the centre nearly of the current. I may state that the inhabitants of Greenland are nearly all Esquimaux excepting the Government officials at Julianshaab. Julianshaab is a small town, and the governor of the district resides there; there are between 2,500 or 3,000 population in that district; the Esquimaux are civilized and under religious cultivation. They attend church, and many of them I saw reading their psalms and hymns in church, and we heard some preaching from the Esquimaux.

4112. I think Dr. Kane gives very much the same account?—He was very far north, and the state of intelligence in the south is far ahead of the north.

4113. Did not he call there on his way to the north?—I think he was not so far south. Everybody knows, of course, the state of intelligence of the people of Iceland, as being rather superior than otherwise. They are very good people, and there is nothing to be apprehended from them at all. I have taken all the precautions that I possibly could to investigate the character of the people, their habits, the cost of labour, and the probability of maintaining the line after it is down. I have gone into all these details, as well as into the specialty of laying the cable on all the coasts, except, I may say, the north of Scotland. I presume we shall have no difficulty in landing a cable there, let the coast be what it may.

4114. (*Chairman.*) Do you anticipate no difficulty in carrying the land line from Hamilton's inlet to the Canadian lines?—None at all.

4115. Is not that country very much uninhabited?—Yes, much uninhabited.

4116. Would not you be obliged to maintain a staff along that part of the line?—Not necessarily. I have had to run lines through regions of America where we do not find a house from hundred miles to hundred miles.

4117. Are you aware that the lines across Newfoundland were very difficult to make and still more difficult to maintain?—I am fully informed with regard to the mode of constructing the Newfoundland line, and very well informed in regard to the character of that country. That is a marshy country, full of rivers, bogs, fens, and everything that can be disadvantageous to the maintenance of a telegraphic line.

4118. That is not the character of the country through which you propose to make your line?—It is not the character of the interior of Labrador.

4119. (*Chairman.*) Is it a rocky country?—There are some rocks; it is a sandy, piney country; it is an entirely different country from Newfoundland. The Newfoundland line was constructed by men of no experience in telegraphy; they were merchants, who entered into it as a speculation. None of them knew anything about telegraphy. I speak of it in that light, knowingly.

4120. Are there any points connected with your scheme upon which you would like to offer further information?—The principal reasons that have induced me to believe in the complete practicability of this route at first was the electrical advantage that it was composed of, circuits which could be commercially available; that long subaqueous circuits I have not believed for some years past, and do not believe now, to be possible for commercial telegraphy. It may be, as a philosophical fact, possible to telegraph through the 2,000 miles between Ireland and Newfoundland, or any other circuit of that extent, but I do not believe it is possible by any contrivance to make it answer as a commercial enterprise, nor do I believe that it could be done if the line were on poles. Air is a much better non-conductor than any substance that we can get. This northern route is broken up into short circuits, but the longest of those circuits will present difficulties fully sufficient for the greatest amount of scientific knowledge that we have in telegraphy to overcome.

4121. What is the length of your longest line?—It would be about perhaps 600 miles.

4122. Where would that be?—Between Labrador and Greenland. The distance from Labrador to Greenland is not so great as that; but I estimate the slack of the cable according to the ratio of percentage of the Atlantic cable from Ireland to Newfoundland; it possibly may be more.

4123. Do you mean statute miles or nautical miles?—Statute miles.

4124. (*Captain Washington.*) Of 1,760 yards each?—Yes; it is not the exact distance, because the precise length would depend upon the point of starting and landing in those bays; we might go further than that perhaps.

4125. (*Chairman.*) What do you consider the greatest length of commercial circuit, if I may use the term, which could be used?—I think it is possible that 1,000 miles might be made commercially available.

4126. Are you aware that a line of cable now exists of 750 miles in length in water of a temperature of 70°, through which 10 or 12 words a minute can be worked without difficulty?—I am not informed upon that subject as to the details; I have heard about it. I also read officially in the report of the board of directors of the Atlantic Telegraph Company that they had demonstrated beyond a doubt that they could send 10 words a minute through the cable; but from the efforts made upon that line after it was laid, we had no such evidences. [At the correction of the proof of this examination further information has been received as to the working of the 750 miles of line above referred to. The maximum speed is seven words per minute.]

4127. But if it were proved to you that a line has existed for four or five months 750 miles long which is daily sending messages at the rate of 10 words a minute, would that change your opinion as to the length of a commercial circuit?—No; because I have just admitted that a commercial circuit might be as great as 1,000 miles. It depends entirely upon the conducting medium. It is possible that by increased sized conductors an additional facility for transmission might be attained.

4128. Or a decrease in the speed of transmission?—Possibly; that is a question that I have not experimented upon myself.

4129. In fact your opinion would come to this, that 1,000 miles of cable working at five words a minute would not be commercially remunerative?—That would depend upon the expense of administration and the capital invested.

4130. I assume the capital to be *pro rata* of the distance, that if you have a greater distance in circuit you have *pro rata* a larger capital, not in direct proportion but in a certain proportion, and therefore I assume that if you have 1,000 miles of cable that will work at five words a minute, assuming it possible, because we have 750 miles of cable that works at 10 words a minute, that would not be commercially remunerative?—I think that an existing line capable of transmitting only five words a minute, whether 1,000 miles or one mile long, is surrounded by so many difficulties in the transmission that it could not be made a successful commercial enterprise. Any line that cannot transmit more than five words a minute cannot be depended upon, according to my experience in the administration of telegraph lines for many years past, as a commercial enterprise.

4131. What number of words per minute is the minimum number which you consider should be transmitted to make a line commercially profitable?—To answer that question I should require to make some estimates, which I could do if I had a little time for the purpose. I have always found that in a bad working line (a five-word line would be a bad working line) there is a large per-centage of the time required for the offices asking each other to repeat—they do not get what is sent, and for the correction of what they do send, so that a great amount of the five words that are sent per minute are words used for ascertaining what is sent. We find that the case on air lines when we are interrupted with atmospheric electricity. Many times we cannot get more than a few words per minute, and many of our lines in the south-western States have been thrown into bankruptcy on that account.

4132. What per-centage of the number of messages sent do you consider is required for office purposes or purposes of interrogatories, and so forth?—That depends entirely upon the working of the line; on a good line it is reasonable to calculate at least five per cent., that is on the very best working lines, and it increases in proportion to the non-working of the line.

4133. What speed of transmission are your estimates founded upon for the North Atlantic line?—I have not made any estimates upon that subject because the speed will very much depend upon the character of the cable adopted and the length of the circuits. I have not made up my mind what kind of cable will be best for that purpose.

4134. My question refers to this. You say that a line from Newfoundland to Ireland would not be, in your opinion, a commercial line, because the capital expended in obtaining a fair working speed would be so large. What do you consider to be a fair working speed in order to secure a remunerative return?—I would still have to estimate the cost and all the capital invested in a given circuit to ascertain that fact. Not being aware that that would enter into the subject of examination, I have made no preparation for it. I should be very happy to do so if it is desired. On the line from Newfoundland to Ireland there was a capital of half a million sterling expended. Now that line must do work enough to pay for the expenses of management, and a fair interest upon the capital invested as well as something to meet the hazard involved.

Colonel Tol. J. Shaffner.

14 March 1860.

4135. What is your capital for the North Atlantic line?—We have not adopted any amount of capital yet, but that, again, will depend upon the cable.

4136. You have not made any estimate?—None at all. I have kept my mind free from making any estimates in regard to the working of the line, the number of words it would transmit, the capital to be invested, or the charge to be made for the messages, or anything of that sort. I have kept my mind as a blank sheet of paper, to be open to further investigation.

4137. That being the case, how can you give a decided opinion that your line will be a commercial line, and that the Newfoundland and Ireland line will not be a commercial line, the whole question being one of cost and return?—Because a cable laid on the longest circuit of this northern route, even at the greatest expense that has been incurred for the construction of cables, will not be greater than the capacity of the working of a given circuit commercially based upon actual experiments on lines of equal length, subterranean or submarine.

4138. Will you be good enough to attach to your evidence a short estimate, founded upon transmitting ten words a minute, as to the difference of the cost of your line and the old Atlantic line?—I would be willing to give a presumed estimate upon that subject, relative to the line that I propose; but I would not like to make it comparative with the other.

4139. Had you ever any idea of carrying a telegraph from Europe through Asiatic Russia?—No. I did go to Russia, having in view the getting of a concession, or not exactly the getting of a concession, but to getting such an action on the part of the Russian Government as would prevent that route from ever being brought up as an inflated balloon against the more practicable route between Europe and America. I do not think that a line can be made commercially between the two countries by that route, and such was the decision of the late Emperor. Whenever the Government should be disposed to make it a commercial line, I expect it to be accorded to me, as promised by His Majesty, the late Emperor.

4140. The overland route?—The overland route; the principal objection would be the internal regulations, and the political character of Russia.

4141. Would you have more difficulty in maintaining a land line through Siberia than you would in maintaining the land line which you propose from Hamilton's Inlet?—Yes; especially when you come on the north-western coast about Sitka and Cook's Inlet, for instance. I do not think it would be possible to establish a line by way of Behring's straits; it might be possible along the Aleutian Isles. Some years ago I published a scheme of a great world girdle telegraph, but I only intended that this part should be completed now, and the remainder hereafter, when made practicable.

Answer to Question 4138.

COMPARATIVE WORKING of the NORTH ATLANTIC and the OLD ATLANTIC TELEGRAPH PROJECTS.

To answer this question fully would require much time and many pages of print, I will group together a few facts based upon the present known sciences, and leave the reader to draw his own conclusions.

There are about 108,000 miles of telegraph lines in operation in the world, traversing different zones and under influences of different temperatures and other physical circumstances. My observations have been extensive on both hemispheres, and I have, from time to time, noted the developments manifested in the manipulation of practical or commercial telegraphy.

Colonel Tel. P. Shaffner.

14 March 1860.

Air-lines traversing warm regions are much interrupted by atmospheric electricity, by incompleteness of insulation, and by conductive physics of the air, such, for example, as heat, fog, &c. Submarine lines are interrupted by incompleteness of insulation and by the retardation effects resulting from the Leyden jar developments, common to the transmission of electric currents through subaqueous conductors.

Throughout the whole range of the 108,000 miles of telegraph lines, I do not believe that there is to be found in operation a commercial circuit of 1,000 miles. The lines are divided into circuits, the length adopted for each depends upon the extent of the interruptions above mentioned—submarine and air-lines respectively. Air-line circuits are usually 300 to 500 miles long, and but seldom longer. It is usual to overcome distance by coupling two or more circuits together with an apparatus, called a "translator" or "repeater." Without these instruments the distance cannot be overcome, except by re-writing with the hand. Sometimes, however, auxiliary batteries are placed at different parts of the same circuit, but in submarine telegraphy we can have batteries only at the ends of the cable, and the cable can only be charged by a battery at the station of transmission. We cannot have co-operating batteries at both ends at the same time. On submarine lines the circuits are arranged as thus described, but the speed of transmission is dependent upon the length of line and the retarding force of the Leyden jar. On the many thousand miles of air-lines on the American continent we have no circuit as long as 1,000 miles. In submarine telegraphy there is no circuit of that length.

The interruptions occasioned to air-lines are but equals to those common to submarine telegraphs, excepting with this difference, that on air-lines the electric force is taken from the conductor or increased in charge, and on submarine lines it is held in suspension. The results, however, are the same. The electric force cannot be scaled to intelligible measurement. The pulsations of the electric current must be even, regular, and known as equal, at both ends of the line respecting duration. If the current is irregular the signals transmitted will be questionably received. According to a written opinion given me some time since by Mr. Charles T. Bright, now Sir Charles, the retardation of the electric current transmitted through a subterranean line of copper wire No. 16 was at the rate of one third of a second for 500 miles, and one second for a distance of 1,000 miles; and, according to that progression, it would be about nine seconds for 2,000 miles, if that length of circuit can be brought to commercial telegraphy. It is possible that a larger conductor and a greater thickness of insulation will lessen the retardation. To what extent this latter organization of a cable would prove effective, on a long submarine circuit, remains a theory; and every practical telegrapher knows that theory in the mysterious workings of the electric telegraph cannot, in all cases, be depended upon. In the application of theory to practice new developments in physics appear, and, in many instances, wholly defeat that which theory seemed to demonstrate as certain results.

The old Atlantic line was 2,050 miles long, in one continuous circuit—the longest in the world. It was laid on the 5th of August 1858, and, at the time, it was announced as a success. Herewith I give the working of that line from the 10th of August to the 1st of September inclusive. From the 5th to the 10th of August is allowed for the perfection or arrangement of the apparatuses at the respective stations. The correctness of the working herewith given has been evidenced before the United States Consul in London, by one of the electrical staff of the Company that was in Newfoundland, and by one of the staff that was in Ireland.

I produce the whole working of that line as the best evidence in substantiation of my opinion, that a circuit of that length cannot be made commercial.

MANIPULATION OF THE ATLANTIC TELEGRAPH LINE,

From August 10th to the 1st of September inclusive.

FIRST DAY.

August 10, 1858.

Newfoundland to Valentia.

Time sent.—"Repeat, please."
Sent 2.23 A.M.—"Please send slower for the present."
Sent 10.20 A.M.—"How?"
Sent 10.50 A.M.—"How do you receive?"
Sent 11.35 A.M.—"Send slower."
Sent 11.50 A.M.—"Please send slower."
Sent 12.20 P.M.—"How do you receive?"
Sent 1.56 P.M.—"Please say if you can read this."
Sent 2.31 P.M.—"Can you read this?"
Sent 2.47 P.M.—"Yes" (supposed).
Sent 3.43 P.M.—"How are signals?"
Sent 5.40 P.M.—"How are signals?"
Sent 6.10 P.M.—"Do you receive?"
Sent 8.16 P.M.—"Please send something."
Sent 8.45 P.M.—"Please send V.'s and B.'s."
(Adjusting signals.)
Sent 10.38 P.M.—"How are signals?"

SECOND DAY.

August 11, 1858.

Newfoundland to Valentia.

Sent 2 A.M.—"Please send B.'s"
Sent 9.37 A.M.—"Send alphabet."
Sent 11.50 A.M.—"Send alphabet."
Sent 12.56 P.M.—"Please send V's."
Sent 1.25 P.M.—"Send V slowly."
Sent 2.38 P.M.—"Send V slowly."
Sent 3.58 P.M.—"Little better; send V."
Sent 5.37. P.M.—"Can't read. Adjust your key."
Sent 5.43 P.M.—"Send alphabet."
Sent 7.23 P.M.—"Please tell me how signals are."
Sent 7.50 P.M.—"Alter your key to send slower."
Sent 8.15 P.M.—"Send alphabet."

THIRD DAY.

August 12, 1858.

Newfoundland to Valentia.

Sent 2.50 P.M.—"Send alphabet."
Sent 3.55 P.M.—"If this received send battery current in one direction five minutes."

Valentia to Newfoundland.

Sent 4.02 P.M.—"Sent constant current five minutes."

Newfoundland to Valentia.

Sent 4.22 P.M.—"Received."
Sent 5.35 P.M.—"Laws, Whitehouse received five minutes signal Coil signals too weak. Work relay. Try drive slow and regular. I have put intermediate pulley. Reply by coils."

[This was the first complete message in form received from Newfoundland through the Atlantic cable.]

Sent 10 P.M.—"Do you receive?"

FOURTH DAY.

August 13, 1858.

Newfoundland to Valentia.

Sent 12.38 A.M.—"Send word 'Atlantic.'"

Valentia to Newfoundland.

Send 12.51 A.M.—"Atlantic."

[A note by Professor Thompson, in Valentia diary, says:—"This was the first word read in Newfoundland."]

Newfoundland to Valentia.

Sent 1.04 A.M.—"All right, Thomson's galvanometer. Send some other words."
Sent 3.07 A.M.—"We read all your signals. Repeat 'Atlantic.'"

Valentia to Newfoundland.

Sent 3.37 A.M.—"Atlantic."

Newfoundland to Valentia.

Sent 3.49 A.M.—"Send ten V's, and repeat words."
Sent 5.13 A.M.—"Send two words."
Sent 5.52 A.M.—"Sent 'alphabet' and two 'understands.'"
Sent 7.23 A.M.—"Send alphabet."
Sent 7.51 A.M.—"Did not receive. Send again."
Sent 8.19 A.M.—"Send word 'Valentia.'"

Valentia to Newfoundland.

Sent 8.32 A.M.—"Valentia."

Newfoundland to Valentia.

Sent 8.40 A.M.—"Yes, yes. Send word 'Thomson.'"
Sent 8.55 A.M.—"Repeat. Send 'Thomson.'"

Valentia to Newfoundland.

Sent 9 A.M.—"Thomson."

Newfoundland to Valentia.

Sent 9.18 A.M.—"After long blank send word 'Atlantic.'"

Valentia to Newfoundland.

Sent 9.30 A.M.—" Atlantic."

Newfoundland to Valentia.

Sent 9.40 A.M.—" Received last. Send any word."
Sent 10.26 A.M.—" Send a few words slowly."
Sent 10.53 A.M.—" Send alphabet."
Sent 12.2 P.M.—" We receive currents, but can't read you. Send V."
Sent 12.30 P.M.—" Can't read. Send V."
Sent 1.10 P.M.—" Please send dots for twenty-five minutes."

Valentia to Newfoundland.

Sent 1.40 P.M.—" Sent dots."

Newfoundland to Valentia.

Sent 1.59 P.M.—" Dots right. Send slowly for fifteen minutes, please."
Sent 2.34 P.M.—" Please send V's."

Valentia to Newfoundland.

Sent——" Sent V's."

Newfoundland to Valentia.

Sent 3.24 P.M.—"Receive perfect. Will you take message?"
Sent 5.6 P.M.—"See to key. I think your blank key does not short circuit."
Sent 5.45 P.M.—"You must slower, as some of your dots do not show on most delicate detectors."
Sent 6.42 P.M.—" We get scarcely any of your currents. Our receiving instruments are as delicate as it is possible to get them."
Sent 8.15 P.M.—" We receive but few currents. Can you try other coils or only one?"
Sent 9.30 P.M.—" Using new large double keys, but have put on treddle. What have you on? Our relay and recorders in good working order. Begin with attack."
Sent 10.58 P.M.—" We get your currents, but so irregularly that we cannot read them. Will you examine your key well? If you understand this send the letter "H" four times.—De Sauty."

Valentia to Newfoundland.

Sent 11.03 P.M.—"Sent four H's."
Sent 12.18 P.M.—" Repeated four H's."

FIFTH DAY.

August 14, 1858.

Newfoundland to Valentia.

Sent 12.43 A.M.—"Received your few signals, but very bad. Repeat."

Valentia to Newfoundland.

Sent 12.53 A.M.—" Sent currents at six seconds intervals."

Newfoundland to Valentia.

Sent 1.08 A.M.—"Better, but imperfect."

Valentia to Newfoundland.

Sent 8.16 A.M.—"Sent four H's."

Valentia to Newfoundland.

Sent 1.27 A.M.—"Better. Try again."

Valentia to Newfoundland.

Sent 1.34 A.M.—" Repeated four H's."

Newfoundland to Valentia.

Sent 1.44 A.M.—H's received. Try words."

Valentia to Newfoundland.

Sent 1.53 A.M.—"Send faster."
[These were the first words recorded in Newfoundland.]

Newfoundland to Valentia.

Sent 2.55 A.M.—"Understand. Send faster. Now try message. We get your signals on delicate detector by tapping and marking the paper with pencil for the time the needle is held over on either side."

Valentia to Newfoundland.

Sent 4.10 A.M.—" Altered—weak still."
[This is all that could be made out of a message sent to Newfoundland in answer to theirs of 2.55.]

Newfoundland to Valentia.

Sent 4.25 A.M.—" Please work slowly. Your currents do not move needle half degree."
Sent 6.32 A.M.—" Only few currents strong enough to deflect needle."
Sent 7.53 A.M.—"Signals not readable. Repeat."
Sent 9.49 A.M.—"Signals too irregular to read."

Valentia to Newfoundland.

Sent 10.08 A.M.—" Sent signal 'understand:'"

Newfoundland to Valentia.

Sent 10.50 A.M.—" Could only read the signal 'understand' and the word 'to.'"

Valentia to Newfoundland.

Sent 11.40 A.M.—"Thomson's galvanometer and relay."
[The only intelligible words of a message sent.]

Newfoundland to Valentia.

Sent 12.01 P.M.—" We try to read your signals on galvanometer. They too weak for relay."
Sent 3.35 P.M.—" What is occurring? If you understand " give H four times, and if you authorize statement give V four " times."
[This only received at Valentia of the following message:—
" To Saward.—Intense excitement in United States. Impression.
" We cannot work through the cable. Shall I state what is " occurring?" &c., &c., &c.]

Valentia to Newfoundland.

Sent 2.48 P.M.—"H" four times.

Newfoundland to Valentia.

Sent 3.37 P.M.—"Your 'understand' received. Send something."
Sent 4.22 P.M.—" Please send something."
Sent 5.49 P.M.—" Please send alphabet."
Sent 8.07 P.M.—"Now please say something."
Sent 10.20 P.M.—"Newfoundland. Saturday. Saward—E. " M. Archibald, New York, telegraphs, instructed by Honourable " Directors Atlantic Telegraph Company, and Directors New " York, Newfoundland and London Telegraph Company, to " state that unexplained delay injures interests both companies. " I replied—'cause not passing messages—that instruments " require great care and adjustment. Doing fast possible. You " should not look on cable as on ordinary short line, as we " encounter many little difficulties, but think all soon overcome.' " DE SAUTY."

SIXTH DAY.

August 15, 1858.

Newfoundland to Valentia.

Sent 2.38 A.M.—" Can't read signals. Send alphabet."

Valentia to Newfoundland.

Sent 3 A.M.—"Wait."
[This in answer to repeated requests from Newfoundland to acknowledge message De Sauty to Saward of over night.]

Newfoundland to Valentia.

Sent 3.29 A.M.—"Wait" (and signal "understand").
Sent 4.30 A.M.—" Can't read your signals. Send V's."
Sent 11.42 A.M.—" We get nothing from you. Send."
Sent 2.52 P.M.—" Will you try and work with sawdust " battery and reversing key, making long contacts?"
Sent 5.3 P.M.—" It is possible that you have * * * * cable " near you. We disconnect half hour for you to test."
[The above was all that was received of the following message:—" It appears to us that you receive our signals. Is it " possible that you have fault in cable near you?" &c., &c., &c. The asterisks mark the omission of the words "fault in" which was caused by making joint on the beach at Valentia.]
Sent 5.43 P.M.—" Have you tested?"
Sent 8.25 P.M.—" Send dots. Send slower."

SEVENTH DAY.

August 16, 1858.

Newfoundland to Valentia.

Sent 12.4 A.M.—"Send V's."

Valentia to Newfoundland.

Sent 12.20 A.M.—"Sent V's."

Newfoundland to Valentia.

Sent 12.34—" Please send alphabet."

Valentia to Newfoundland.

Sent 2.27—"Sent alphabet."
[This alphabet arrived perfect at Newfoundland, excepting the letters "P, V, W," and "Z," and these were the faults of the operator.]

Newfoundland to Valentia.

Sent 2.57.—"All right but three letters. Please ask some question, but much faster."

Valentia to Newfoundland.

Sent 3.35 A.M.—" Understand. Can * * * *"
[The rest of the message, which was as follows, was unintelligible:—" You take a message?" These words came afterwards, forming the twentieth message.]

Newfoundland to Valentia.

Sent 3.36 A.M.—"Please after care. Yes, at same rate."

Valentia to Newfoundland.

Sent 4.56 A.M.—" You must repeat each sentence in full."

Newfoundland to Valentia.

Sent 5.15 A.M.—" Repeat word before 'in.'"

Valentia to Newfoundland.

Sent 5.40 A.M.—" Sentence."

Newfoundland to Valentia.

Sent 5.55 A.M.—" I said—'send your message.'"
Sent 5.40 A.M.—" Received, but not intelligible. Try again, " commencing with four V's."
[The words sent were, "Your signals received, but not intelligible, &c. &c." This was in answer to the Director's message, which, at the first transmission, could not be read. N.W. Irwin on duty in Newfoundland at the time.]

Colonel Tal. Shaffner.

14 March 1860

Colonel Tal. P. Shaffner.

14 March 1860.

Valentia to Newfoundland.

Sent 11.12 A.M.—"Directors of Atlantic Telegraph Company, "Great Britain, to Directors in America:—Europe and America "are united by telegraph. Glory to God in the highest; on "earth peace, good will towards men."
Sent 11.32.—"Repeat back faster."

Newfoundland to Valentia.

Sent 11.44.—"Directors.—All right. Will you receive "one?"

Valentia to Newfoundland.

Sent 12.36 P.M.—"No. Repeat all back, faster. Queen's "next."

Newfoundland to Valentia.

Sent 1.16 P.M.—Repetition—"Directors of Atlantic Telegraph "Company, Great Britain, to Directors in America;—Europe "and America are united by telegraph. Glory to God in the "highest; on earth peace, good will towards men."

Valentia to Newfoundland.

Sent 1.39 P.M.—"Can we send faster? You can."

Newfoundland to Valentia.

Sent 1.45 P.M.—"'Repeat before 'send.'"
Sent 2.08 P.M.—"You may go little faster. We receive on "galvanometer. Relay won't work."

Valentia to Newfoundland.

Sent 4.15.—"Wait repairs to cable."
[These words were sent after Valentia had sent as far as "greatest interest" in the Queen's message, and it was in consequence of their making this interruption, without signifying that the despatch was not finished, that the unfortunate error took place of forwarding on that communication before completion.]
Sent 6.29 P.M.—"The Queen desires to congratulate the "President upon the successful completion of this great inter- "national work, in which the Queen has taken the greatest "interest. The Queen is convinced that the President will join "with her in fervently hoping that the electric cable, which "now connects Great Britain with the United States will prove "an additional link between the two nations, whose friendship "is founded upon their common interests and reciprocal esteem. "The Queen has much pleasure in thus directly communicating "with the President, and in renewing to him her best wishes "for the prosperity of the United States. Repeat very fast."
[The Queen's message was not completely finished until 6.48 A.M., of the 17th August. During the time the repetitions in answer to questions from Newfoundland were being made, the Valentia signals, as observed in Newfoundland, were very much embarrassed, probably by earth currents. It was raining very hard in Newfoundland during the whole time.

H. W. IRVIN.]

Newfoundland to Valentia.

Sent——"Repeat from beginning of message to desires."
Sent——"Words between 'deepest' and 'convinced.'"
Sent——"All from 'esteem' to 'end.'"

EIGHTH DAY.

August 17, 1858.

Newfoundland to Valentia.

Sent 2.32 A.M.—"After 'and in renewing.'"
Sent 3.40 A.M.—"After 'and in renewing.'"
Sent 5 A.M.—"After 'and in renewing.'"
Sent 6.28 A.M.—"Go on after 'United States.'"
Sent 7.53 A.M.

[Repetition of Queen's message.]

"The Queen desires to congratulate the President upon the "successful completion of this great international work, in "which the Queen has taken the greatest interest. The Queen "is convinced the President will join with her in fervently "hoping that the electric cable which now connects Great "Britain with the United States will prove an additional link "between the two nations, whose friendship is founded upon "their common interest and reciprocal esteem. The Queen has "much pleasure in thus directly communicating with the Presi- "dent, and in renewing to him her best wishes for the prosperity "of the United States."
Sent 8.5 A.M.—"Now take one."

Valentia to Newfoundland.

Sent 8.12 A.M.—"Sent correct. Go faster."
Sent 9.10 A.M.—"Send fast."
[This was after requesting a repetition.]

Newfoundland to Valentia.

Sent 10.10 A.M.—"Newfoundland, Monday. Directors, "Atlantic Telegraph Company:—Entered Trinity Bay, noon, "fourth. Landed cable at six, Thursday morning. Ship at "once to St. John's two miles shore cable, with end ready for "splicing. When was cable landed at Valentia? Answer by "telegraph, and forward my letters to New York.

"CYRUS W. FIELD."

Valentia to Newfoundland.

Sent 10.20 A.M.—"Acknowledged. Go on."

Newfoundland to Valentia.

Sent 10.34 A.M.—"Have you more? Always give the signal "'cleared out' when you finish."
Sent 1.21 P.M.—"Ward to Whitehouse:—Mr. Cunard wishes "telegraph McIver Europa collision Arabia. Put into St. John's. "No lives lost. Will you do it? Stay anxiety non-arrival De "Santy."
[The word "Ward," with which this message commences, was intended for Saward; but the first two letters of the name were not intelligible.]
Sent 3.30 P.M.—"Receive nothing intelligible. Is your end "cable right? Disconnect half an hour for your test."
Sent 6.45 P.M.—"Can't read. Signals irregular. Repeat."
Sent 9.41 P.M.—"Please send a succession of alternate dots "and dashes for ten minutes. I shall have President's message "soon."
Sent 10.25 P.M.—"President's message here. If you can "receive it send current in one direction five minutes. When "have received and correct, send current five minutes again. "We have not read a single word from you since 3.10 your "time."

Valentia to Newfoundland.

Sent 10.31 P.M.—"Sent current five minutes."

NINTH DAY.

August 18, 1858.

Newfoundland to Valentia.

Sent 2 A.M.—"Did you get my last message? President's "waiting."
Sent 5.15 A.M.—"Can you take message?"
[Notwithstanding both stations making every exertion to restore communication by alternately sending and putting in instruments to receive, nothing more was read at Valentia and not one word in Newfoundland during the whole day. It is proper to state, however, that from 6.40 A.M. until 9.08 P.M., operations which took place at Valentia prevented the possibility of transmission or reception. From 6.40 till noon Mr. Canning was engaged lifting the cable in the harbour. From noon until 1.30 P.M. efforts were made to communicate, and from this time till 9.08 P.M. operations obstructed electrical working.]

TENTH DAY.

August 19, 1858.

Newfoundland to Valentia.

Sent 12.03 A.M.—"See to adjustment. Can you receive "President's message? Been here since yesterday."
Sent 12.43 A.M.—"We can't read."
Sent 1.42.—"Currents too weak to read."
Sent 2.12 A.M.—"Very good currents, but can't read. Send "B's."

Valentia to Newfoundland.

Sent 3.35 A.M.—"C. C. G. Faster."
[This is answer to Newfoundland's request to send C's for adjustment.]
Sent 7 A.M.—"Send message fast."
[These words came in very good signals. The deflections on galvanometer very strong.]

Newfoundland to Valentia.

Sent 7.05 A.M.—"Have you received message for McIver? "Send acknowledgment."

Valentia to Newfoundland.

Sent 7.06 A.M.—"No."

Newfoundland to Valentia.

Sent 7.58 A.M.—"D. C. McIver, Liverpool. Arabia in col- "lision with Europa. Cape Race, Saturday, Arabia on her "way. Head slightly injured. Europa lost bowsprit, cutwater; "stern sprung. Will remain in St. John's, Newfoundland, ten "days from sixteenth. Persia calls at St. John's for mails and "passengers. No loss of life or limb. Cunard. New York, "August 17."

Valentia to Newfoundland.

Sent 8.04 A.M.—"Cunard. All right. Go on."

Newfoundland to Valentia.

Sent 10.25 A.M.—"Washington City. To Her Majesty Vic- "toria, Queen of Great Britain:—The President cordially "reciprocates the congratulations of Her Majesty the Queen, on "the success of this great international enterprise, accomplished "by the science, skill, and indomitable energy of the two "countries. It is a triumph more glorious, because far more "useful to mankind, than ever was won by conquest on the "field of battle. May the Atlantic telegraph, under the "blessings of heaven, prove to be a bond of perpetual peace "and friendship between the kindred nations, and an instrument "designed by Divine Providence to diffuse religion, civilization, "liberty, and law throughout the world. In this view will not "all the nations of Christendom spontaneously unite in the "declaration that it shall be for ever neutral, and that its com- "munications shall be held sacred in passing to the place of "their destination, even in the midst of hostilities?

"JAMES BUCHANAN."

Valentia to Newfoundland.

Sent 10.40 A.M.—"President's all right."

Newfoundland to Valentia.

Sent 11.20 A.M.—"Your current much stronger, but cannot " read your signals. Repeat."
Sent 12.02 P.M.—"Received. Send a few words."
Sent 12.56 P.M.—"Your currents very weak. Repeat."

Valentia to Newfoundland.

Sent 2.01 P.M.—"How now—can you read?"

Newfoundland to Valentia.

Sent 2.23 P.M.—"Understand. Better than ever. Please " always commence by attack and give final signals, as we " receive on galvanometer. Relay won't work."
Sent 2.28 P.M.—"To Whitehouse. Please send large circular " galvanometer."

Valentia to Newfoundland.

Sent 3.19 P.M.—"Can you take message for Field?"
[At eight P.M., Valentia had finished message to Mr. Field. Currents were strong, but very irregular, and only the last five words were readable. This was the message commencing "Directors have just met."]

Newfoundland to Valentia.

Sent 3.48 P.M.—"Strength of your current constantly varies. " Send Field's message."
Sent 3.12 P.M.—"Repeat all from beginning to 'tariff.'"
Sent 3.56 P.M.—"You should never send more than a dozen " words at a time in long messages. Repeat all of last message " before 'tariff.'"
Sent 9.57 P.M.—"Can't read. Send dots and dashes."
Sent 10.19 P.M.—"After 'can you.'"

Valentia to Newfoundland.

Sent 10.30 P.M.—"How now."

Newfoundland to Valentia.

Sent 10.34 P.M.—"Better. Repeat message to Field."

Valentia to Newfoundland.

Sent 10.36 P.M.—"Try read on galvanometer."

Newfoundland to Valentia.

Sent 10.57 P.M.—"Yes. Repeat Field's message."
Sent 11.28 P.M.—"All to word 'the.'"
Sent 11.46 P.M.—"Understand after 'met.'"
[The last ten messages refer to the following message to Mr. Field, which although commenced at 3.55 P.M. on the 19th, was not finished until 1.45 A.M. of the 20th.]

ELEVENTH DAY.

August 20, 1858.

Newfoundland to Valentia.

Sent 12.11 A.M.—"Now from 'Valentia' to 'tariff.'"
Sent 1 A.M.—"I. End. Understand."

Valentia to Newfoundland.

Sent 1.45 A.M.—"C. W. Field, Newfoundland, August 17, " 3 P.M.—The directors have just met. They heartily congra- " tulate you on success. Agamemnon anchored at Valentia at " 6 A.M. on Thursday, August 5. We are just on the point of " chartering a ship to lay shore end. No time will be lost in " sending them out. All your letters have been posted to New " York. Please write to me fully about tariff and other working " arrangements."

Newfoundland to Valentia.

Sent 2.24 A.M.—"C. W. Field understand. C. W. Field, " Newfoundland. August 17, 3 P.M. The directors have just " met. They heartily congratulate you on success. Aga- " memnon anchored at Valentia at 6 A.M. on Thursday, August 5. " We are just on the point of chartering a ship to lay shore end. " No time will be lost in sending them out. All your letters " have been posted to New York. Please write to me fully " about the tariff and other working arrangements."
[This message was repeated from Newfoundland to Valentia, to show that it was understood.]

Valentia to Newfoundland.

Sent 2.35 A.M.—"Can we send faster?"

Newfoundland to Valentia.

Sent 2.43 A.M.—"Yes. I have two messages since morning. " Can I send one?"

Valentia to Newfoundland.

Sent 3.20 A.M.—"Send faster."

Newfoundland to Valentia.

Sent 3.14 A.M.—"How do you receive now?
[The two last messages exhibit a discrepancy in point of time as kept at the two stations.]

Valentia to Newfoundland.

Sent 3.38 A.M.—"Splendid on Thomson's galvanometer and " print on Morse key. How do you?"

Newfoundland to Valentia.

Sent 3.44 A.M.—"We work with Morse key and detector. Will " you take message?"

Valentia to Newfoundland.

Sent 4 A.M.—"We can't unless on Company's service."

Newfoundland to Valentia.

Sent 4.45 A.M.—"New York, August 18. Directors of Atlan- " tic Telegraph Company, London:—The directors of New " York, Newfoundland, and London Telegraph Company desire " to express to the directors of the Atlantic Telegraph Company " their joy and gratitude for facilities and privileges on coming " into closer union and fellowship with them and our fellow men " throughout the world. May the success which has crowned " our labours secure to the nations of the earth a perpetual bond " of peace and good fellowship."

"PETER COOPER, President."

Valentia to Newfoundland.

Sent 6.10 A.M.—"Directors all right."

Newfoundland to Valentia.

Sent 7.33 A.M.—"New York, 18th. Directors Atlantic " Telegraph Company, London:—Niagara arrived to-day. All " well. Full reports by mail. I drew on you from St. John's " at three days sight 750*l.* sterling, to pay labourers on Niagara. " Great rejoicing all over country. Successful laying cable. " Please request Admiralty to permit the Gorgon, Capt. Day- " man, accompany Niagara. New York, C. W. Field."

Valentia to Newfoundland.

Sent 8.25 A.M.—"Received acknowledgment of message, Field " to directors."

Newfoundland to Valentia.

Sent 8.31 A.M.—"To Whitehouse. Please send out a large " circular galvanometer, and another relay or two as soon as " possible."
Sent 9.03 A.M.—"If you have anything, go on."

Valentia to Newfoundland.

Sent 9.25 A.M.—"Bartholomew to De Santy.—Whitehouse in " London. Your message about galvanometer, gone there."

Newfoundland to Valentia.

Sent 9.31 A.M.—"De Sauty understand. Go on with the " message."

Valentia to Newfoundland.

Sent 9.38 A.M.—"Cleared out."
[The Newfoundland diary had in it the following words:—"First rate signals from Valentia this morning." This was the first time the cleared out signal was given by both stations at same time. All communications had been accurately got off to their destinations from both sides of the Atlantic.]

Newfoundland to Valentia.

Sent 10.20 A.M.
[The clerks devoted a considerable length of time to practising. The Newfoundland clerk was speaking of instruments in use there. About twenty words were passed over during this time.]

Valentia to Newfoundland.

Sent 10.45 A.M.
[Practising. Replying to Newfoundland clerk; informed him we read on Thomson's land galvanometer, and requested him to try and send faster.]

Newfoundland to Valentia.

Sent 11.10 A.M.
[Practising at higher speed. Newfoundland reports superintendent and staff quite well, and Bull's Arm Station henceforth called "Cyrus Station." About twenty words were sent altogether.]

Valentia to Newfoundland.

Sent 11.30 A.M.—"Bartholomew to De Santy. Wait half " hour—repairs."
[Joint of cable at edge of the water being repaired.]
Sent 2.38 P.M.—"Send alphabet."
[This was repeated four times. The cause of its not being answered at first was owing to the fact that they were repairing key at Newfoundland. The alphabet was afterwards sent.]

Newfoundland to Valentia.

Sent 4.25 P.M.—"Will you take message for Saward?"

Valentia to Newfoundland.

Sent 4.31 P.M.—"Yes, yes."

Newfoundland to Valentia.

Sent 4.40 P.M.—"Saward. Two cable splicers and gutta- " percha jointer here waiting to make a splice in shore end. " All well.—DE SAUTY."

Valentia to Newfoundland.

Sent 4.56 P.M.—"We will send coil currents. Say if you " will receive."

Newfoundland to Valentia.

Sent 5.20 P.M.—"Signals very good. Will you receive from " Mayor Halifax to Lord Mayor of London."
Sent 5.52 P.M.—"Have you received my last service mes- " sage?"

Valentia to Newfoundland.

Sent 6.15 P.M.—"We can't take message."

Colonel Tal. P. Shaffner. 4 March 1860.

Newfoundland to Valentia.

Sent 6.38 P.M.—"The two messages are from Mayors Halifax and Toronto."

Valentia to Newfoundland.

Sent 6.52 P.M.—"We have asked London. Wait."

Newfoundland to Valentia.

Sent 7.50 P.M.—"Well, have you asked those messages of De Sauty ?"

Valentia to Newfoundland.

Sent 9.3 P.M.—"*Bartholomew to De Sauty — Whitehouse away. Directors* order no message but Government and companies' to be sent yet. Hope. You men."
[The words in italics were not received in Newfoundland.]

Newfoundland to Valentia.

Sent 9.19 P.M.—"Have you message ?"

Valentia to Newfoundland.

Sent 9.21.—"No."

Newfoundland to Valentia.

Sent 9.31.—"Was message about 'Europa' made use of ?"

Valentia to Newfoundland.

Sent 9.48 P.M.—"Yes; it was sent for publication."

Newfoundland to Valentia.

Sent 9.55 P.M.—"What weather have you ?"

Valentia to Newfoundland.

Sent 10.8 P.M.—"Very fine. Yours."

Newfoundland to Valentia.

Sent 10.18 P.M.—"Mosquitoes keep biting. This is a funny place to live in—fearfully swampy."
[All messages cleared out again from both stations.]

TWELFTH DAY.

August 21, 1858.

Valentia to Newfoundland.

Sent 12.32 A.M.—"We wish you to send coil currents. Are you ready ?"

Newfoundland to Valentia.

Sent 12.40 A.M.—"Ready."

Valentia to Newfoundland.

Sent 12.45 A.M.—"We send V.'s."
[Good dots and V.'s were received at Newfoundland.]

Newfoundland to Valentia.

Sent 5.46 A.M.—"Yes, we read well."

Valentia to Newfoundland.

Sent 5.58 A.M.—"Can you read ? Send fast."
Sent 6.6 A.M.—"I will send V.'s by coil currents. Say if you get them. Are you ready ?"

Newfoundland to Valentia.

Sent 6.12 A.M.—"Yes, quite ready."
Sent 11 A.M.—"Shall be glad to hear from you."
Sent 12.9 P.M.—"Your signals not readable."

Valentia to Newfoundland.

Sent 12.20 P.M.—"How now ?"

Newfoundland to Valentia.

Sent ——.—"Better."
Sent 1.10 P.M.—"De Sauty to Bartholomew :—Ask Saward send 35 feet ⅞ gutta-percha tube and two gutta-percha syphons with tops, and fifteen pounds gutta-percha, ⅞ sheet."

Valentia to Newfoundland.

Sent 1.22 P.M.—Bartholomew to De Sauty :—Understand. How signals now.

Newfoundland to Valentia.

Sent 1.30 P.M.—"Not very good; repeat my last."

Valentia to Newfoundland.

Sent 1.33 P.M.—"Bartholomew to De Sauty.—Understand. Can you take message ?"

Newfoundland to Valentia.

Sent 1.50 P.M.—"Yes, but repeat figures of my last."

Valentia to Newfoundland.

Sent 1.51 P.M.—"Thirty five, three eight, two fifteen, three eight."

Newfoundland to Valentia.

Sent 2.2 P.M.—"Understand. Send message."

Valentia to Newfoundland.

Sent 4.44 P.M.—"Thomson to De Sauty.—Order Macfarlane to arrange a mirror galvanometer for receiving. Use the innermost coil and power steel magnetic adjustment. Say when will be ready."

Newfoundland to Valentia.

Sent 5.55 P.M.—"Now ready. Go on."
Sent 6.10 P.M.—"Land galvanometer and circuit. Signals beautiful."

Valentia to Newfoundland.

Sent 6.22 P.M.—"Is this first time it is in circuit ?"

Newfoundland to Valentia.

Sent 6.27 P.M.—"No. Send thirty-five words as fast as you can."

Valentia to Newfoundland.

Sent 7.8 P.M.—"Use hundred Daniel's, with reversing key, in your next. We are going to test the cable, and only wait for a specimen of your battery signals, after receiving which we shall explain what we want."

Newfoundland to Valentia.

Sent 7.18 P.M.—"Understand. What reversing key do you mean? We are getting ready Daniel's. If you send again send faster."
Sent 7.30 P.M.—"That again at — speed.
Sent 7.58 P.M.—"Can't read."
Sent 9.12 P.M.—"Shall we send battery currents since everything ready ?"
Sent 10.19 P.M.—"Please send something."

THIRTEENTH DAY.

August 22, 1858.

Newfoundland to Valentia.

Sent 2.5 A.M.—"Can you receive message ?"
Sent 3.4 A.M.—"How do you receive ?"
Sent 10.36 A.M.—"Do you receive this ?"

Valentia to Newfoundland.

Sent 10.50 A.M.—"Can you read this ?"

Newfoundland to Valentia.

Sent ——.—(Signal understand.)
Sent 10.56 A.M.—"Can you take message now ?"

Valentia to Newfoundland.

Sent 10.57 A.M.—"Yes."

Newfoundland to Valentia.

Sent 2.15 P.M.—"August 21st, New York. Right Honourable Sir Walter Carden, Lord Mayor of London.—I congratulate your Lordship on the successful laying of the Atlantic cable, uniting continents Europe and America, cities London and New York, Great Britain and the United States. It is a triumph of science and energy over time and space, uniting more closely the bonds of peace and commercial prosperity, introducing an era in the world's history pregnant with results beyond the conception of a finite mind.—DANIEL F. TIEMANN, Mayor."

Valentia to Newfoundland.

[Acknowledgment of Mayor's message received.]
Sent 2.47 P.M.—"Insulate for two hours and say when you commence. Repeat this."

Newfoundland to Valentia.

Sent 2.56 P.M.—"Understand. Cable insulated."
Sent 5.43 P.M.—"Have you finished testing ?"
Sent 7 P.M.—"Repeat. Send much slower."
Sent 7.55 P.M.—"Signals are good, but your sending comes very bad. Repeat all."
Sent 8.51 P.M.—"Please repeat service."
Sent 9.30 P.M.—"Repeat service message."
Sent 10 P.M.—"Repeat service now."

Valentia to Newfoundland.

Sent 10.19 P.M.—"Thomson De Sauty.—Put delicate detector in circuit and note weak currents from us, thirty minutes each way. Say if ready."

Newfoundland to Valentia.

Sent 11.2 P.M.—"Repeat word after 'us.'"

Valentia to Newfoundland.

Sent 11.8 P.M.—"Thirty."

Newfoundland to Valentia.

Sent 11.12 P.M.—"Understand. Ready now."

FOURTEENTH DAY.

August 23, 1858.

[It rained very hard at Newfoundland from midnight until 6 A.M.]

Newfoundland to Valentia.

Sent 8.09 A.M.—"Your currents very irregular. Repeat."

Valentia to Newfoundland.

Sent 10.52 A.M.—"Signals weak. Will you take message from Lord Mayor ?"

Newfoundland to Valentia.

Sent 10.57 A.M.—"Yes, send little faster and better."

Valentia to Newfoundland.

Sent 11.18 A.M.—"Give 'understand' every twenty words."

Newfoundland to Valentia.

Sent 11.18 A.M.—" All right. Send much faster."
Sent 11.52 A.M.—" Understand faster."
[Testing nearly the whole of the day at Valentia.]

FOURTEENTH DAY.

August 24, 1858.

Valentia to Newfoundland.

Sent 1.57 A.M.—" The Lord Mayor of London to the Hon. " Daniel F. Tiemann, Mayor of New York :—The Lord Mayor " of London most cordially reciprocates congratulations of " Mayor New York upon the success of so important an under- " taking as the completion of the Atlantic Telegraph cable. It " is indeed one of the most glorious triumphs of the age, " and reflects the highest credit upon the energy, skill, and " perseverance of all parties entrusted with so difficult a duty; " and the Lord Mayor sincerely trusts that, by the blessing of " Almighty God, it may be the means of re-uniting those kind " feelings that now exist between the two countries.

" R. W. CARDEN, Lord Mayor."

Newfoundland to Valentia.

Sent 1.04 A.M.—" Repeat between duty and sincerely."
Sent 1.40 A.M.—" Repeat between 'duty' and 'sincerely,' " then after 'feelings.'"
Sent 1.59 A.M.—" Message all right—understood."

Valentia to Newfoundland.

Sent 2.44 A.M.—" Thompson to De Sauty :—On what instru- " ment do you receive? How many divisions deflection do " you get? What battery? How many cells? What key? " Directors desire you send news public interest, but none com- " mercial. Have you any now?"
Sent 3.20 A.M.—" What you want?"
[In an explanatory note it is said that this is " evidently only " a part of a message."]

Newfoundland to Valentia.

Sent 4 A.M.—" Repeat after 'news' in last service."
Sent 4.32 A.M.—" Repeat after 'none.'"

Valentia to Newfoundland.

Sent 7.22 A.M.—" We have long service to send."

Newfoundland to Valentia.

Sent 7.30 A.M.—" Signals better. Repeat after 'we have " 'long.'"

Valentia to Newfoundland.

Sent 7.42 A.M.—" Service message to send."

Newfoundland to Valentia.

Sent 7.45 A.M.—" Acknowledge message Thompson to De " Sauty."

Valentia to Newfoundland.

Sent 8.03 A.M.—" Can you take a long message?"

Newfoundland to Valentia.

Sent 8.06 A.M.—" Go on."

Valentia to Newfoundland.

Sent 8.19 A.M.—" Saward to De Sauty :—Answer by tele- " graph what length shore end you require, and if any small " cable as a reserve. How much surplus had Niagara, and " where is it? Have you splicers and jointers enough? " We have chartered the Bilboa to lay the end. Telegraph full " particulars, and if you require anything beside gutta-percha " articles I will send them by the Bilboa."

Newfoundland to Valentia.

Sent 8.55 A.M.—" Repeat from 'shore end' to 'small.'"
Sent 1 P.M.—" Acknowledge Saward to De Sauty."

Valentia to Newfoundland.

Sent 2 P.M.—" C. W. Field :—We desire you to place in " America about fifteen thousand pounds unappropriated twenty " pound shares, authorized February last. Reply by telegraph. " Soonest. " C. M. SAMPSON."

Newfoundland to Valentia.

Sent 2.05 P.M.—" Acknowledged message."—Sampson to Field.

Valentia to Newfoundland.

Sent 2.50 P.M.—" Answer each question about instruments."
[Refers to Thomson's message 2.44 A.M.]
Sent 3.30 P.M.—" Your currents vary much. Very weak to- " day. How ours to-day?"
[This is in answer to Newfoundland's attempt to send message.]

Newfoundland to Valentia.

Sent 7.27 P.M.—" To Thomson :—Receiving on your gal- " vanometer thirty divisions. Galvanometer seven degrees. " Print local circuit by hand. The large Smees.—White- " house's reversing key. News Persia. " DE SAUTY."
Sent 9.42 P.M.—" Have you received message right? Service " for Saward. Will you take it?"

Valentia to Newfoundland.

Sent 10.20 P.M.—" Yes, and prepare after to receive " Thomson's compensated currents."

Newfoundland to Valentia.

Sent 11.29 P.M.—" De Sauty to Saward :—Two miles shore " and ample. Have half a mile small cable; plenty. It is " stowed on beach. Two splicers and jointer here. Six gallons " naphtha required. Please send authority to draw on Brooking. " 100*l.* required immediately for labourers' house in a wilder- " ness. Roads to make and woods to cut down and clear. " Ought to have some more relays. Have only one great diffi- " culty in sending letters from here. Have written fully."

Valentia to Newfoundland.

Sent 11.30 P.M.—" Understand. Give news of Persia. Also " public news for morning papers. Send much faster."

Colonel Tal. P. Shaffner.

14 March 1860.

SIXTEENTH DAY.

August 25, 1858.

Newfoundland to Valentia.

Sent 12.16 A.M.—" Persia takes Europa's passengers and mail. " Great rejoicing everywhere at success of cable. Bonfires, fire- " works, *feu de joies*, speeches, balls, &c., &c. Mr. Eddy, the " first and best telegrapher in the States, died to-day. Pray give " some news for New York, they are mad for news."

Valentia to Newfoundland.

Sent 1.09 A.M.—" Understand. Sent to London for news. " Meantime take Thomson's currents. Say when ready."

Newfoundland to Valentia.

Sent 1.31 A.M.—" Splendid sending. We are quite ready. " Understand."
Sent 2.24 A.M.—" Murray to Thomson :—Signals very weak. " Send stronger acid."
Sent 6.50 A.M.—" Can you receive?"

Valentia to Newfoundland.

Sent 8.23 A.M.—" Thomson to Macfarlane :—Where are " keys of the glass cases and drawers in the apparatus " (room)?"

Newfoundland to Valentia.

Sent 8.40 A.M.—" Macfarlane to Thomson :—Don't re- " collect."

Valentia to Newfoundland.

Sent 8.55 A.M.—" Saward to De Sauty :—Are American " wires broken or working?"

Newfoundland to Valentia.

Sent ——.—" Working."

Valentia to Newfoundland.

Sent 11.15 A.M.—" Field :—I send my warmest congratula- " tions on the success of the Atlantic telegraph. God be " praised. " GURNEY."
[This was acknowledged by Newfoundland.]
Sent 11.52 A.M.—" North America, with Canadian, and the " Asia with direct Boston mails, leave Liverpool, and Fulton " Southampton, Saturday next. To-day's morning papers have " long interesting reports by Bright. Indian news. Virago " arrived at Liverpool to-day. Bombay dates, 19th July. " Mutiny being rapidly quelled."

Newfoundland to Valentia.

Sent 11.30 A.M.—" Sampson, London :—I will attend to your " request. Have no doubt I can do what you require. " CYRUS W. FIELD."
[This was acknowledged by Valentia.]
Sent 7.40 P.M.—" Signals very weak. Repeat after 'north.'"
Sent 8.16 P.M.—" Repeat after 'the north.'"
Sent 9.38. P.M.—" Mails understand."
Sent 10.13 P.M.—" Repeat from 'Southampton' to 'papers.'"
Sent 10.27 P.M.—" 'Bright.' Go on."
Sent 11.05 P.M.—" Repeat from 'arrived' to 'mutiny'—then " 'after quelled.'"
Sent 11.34 P.M.—" Repeat from 'Bright,' then after 'quelled.'"
Sent 11.57 P.M.—" Signals weak. Repeat one word after " 'Bright.'"

SEVENTEENTH DAY.

August 26, 1858.

Newfoundland to Valentia.

Sent 12.57 A.M.—" Why don't you give 'finis' (signal) when " you have done?"

Valentia to Newfoundland.

Sent 11.15 A.M.—" Received nothing from you since one."

Newfoundland to Valentia.

Sent 11.39 A.M.—" We have had heavy storm, with thunder. " Cable put to earth one hour twenty-five minutes."
[Professor Thomson testing from 9 A.M. till 11.30 A.M.—On the morning of Thursday, August 26, there was a storm of heavy rain, accompanied by thunder and lightning, in Newfoundland. It commenced at 2.30 A.M., and at 3.05 A.M. the lightning was so strong that the end of the cable was put to earth for protection. At 4.30 A.M. the storm ceased, and at 7.15 A.M. the weather is noted as having been since very fine.]

Colonel Tal. P. Shaffner.

14 March 1860.

Sent 1.17 P.M.—"Can't read your signals. Send slower, "and repeat all."
Sent 2.36 P.M.—"Your signals too weak to read."
Sent 3.19 P.M.—"Your signals very weak. Have twenty "messages for you. Will you take them?"
Sent 3.58 P.M.—"Your signals better. Repeat."

Valentia to Newfoundland.

Sent 5 P.M.—"Try only one galvanometer in circuit. We "must understand stoppage of Monday night, and learn how to "manage."

Newfoundland to Valentia.

Sent 5.15 P.M.—"What do you mean by stoppage of Monday "night?"
Sent 7.36 P.M.—"Understand. Go on."

Valentia to Newfoundland.

Sent 8.23 P.M.—"Saward to De Sauty:—T. H. Brooking & "Co. authorize an advance on your order by Hepburn to extent "of one hundred pounds."

Newfoundland to Valentia.

Sent 7.58 P.M.—"After 'De Sauty.'"
Sent 8.28 P.M.—"Understand."
Sent 8.41 P.M.—"After 'Hepburn.'"
Sent 8.51 P.M.—"Understand 'Saward.'"

Valentia to Newfoundland.

Sent 11.11 P.M.—"Thomson to De Sauty:—Your signals "were very weak Monday night at 1.12 Greenwich time. "None came from 2.44 to 4.06. Then very weak indeed. "Improved later. At 8 good. We sent one message eight "times from 2.45 till 5.12. Had no reply. Stoppage again "from 2 to 6 in the afternoon. Can you explain why? Tell us "each time."

Newfoundland to Valentia.

Sent 11.13 P.M.—"Understand. New York, 25th August. "Samuel Gurney, Esq., London. Many thanks for message "received. Americans wild with joy. Great celebration "throughout the land. September 1 and 2.
"C. W. FIELD."

EIGHTEENTH DAY.

August 27, 1858.

Newfoundland to Valentia.

Sent 12.47 A.M.—"Have you received message to Gurney."
Sent 1.27 A.M.—"Please take message Thomson."

Valentia to Newfoundland.

Sent 1.37 A.M.—"No; must send long press messages. Are "you ready?"

Newfoundland to Valentia.

Sent 1.43 A.M.—"Understand. Signals weak. Send ten "words at a time."

Valentia to Newfoundland.

Sent 9.10 A.M.—"George Saward, Secretary Atlantic Tele- "graph Company, to Associated Society Press, 50, Beaver "Street, New York. News for America by the Atlantic Cable. "Emperor of France returned to Paris Saturday. King of "Prussia too ill to visit Queen Victoria. Her Majesty returns "to England 31st August. Petersburg, 21st August. Settle- "ment of Chinese question. Chinese empire open to trade; "Christian religion allowed; foreign diplomatic agents admitted; "indemnity to England and France. Alexandria, August 9. "—The Madras arrived at Suez 7th inst. Dates, Bombay, "19th, Aden, 31st. Gwalior insurgent army broken up. All "India becoming tranquil."

Newfoundland to Valentia.

Sent 2.09 A.M.—"Your signals give scarcely two degrees. "Repeat."
Sent 2.34 A.M.—"Changing rapidly (galvanometer). Please "repeat."
Sent 2.59 A.M.—"'Associated.' Understand."
Sent 3.18 A.M.—"Understand."
Sent 3.37 A.M.—"Saturday."
Sent 3.59 A.M.—"Understand 'August.'"
Sent 4.25 A.M.—"'Question' faster."
Sent 6.09 A.M.—"Repeat after 'allowed.' Work slowly."
Sent 6.41 A.M.—"'France' understand."
Sent 7.01 A.M.—"Understand."
Sent 7.35 A.M.—"Repeat from 'Bombay' to 'Gwalior.'"
Sent 8.36 A.M.—"Repeat after 'Bombay.'"
Sent 11.10 A.M.—"Send slower. Repeat."
[These messages were sent at the end of every ten words of message from Valentia.

Valentia to Newfoundland.

Sent 6.30 P.M.—"Can I send?"
Sent 9.50 P.M.—"De Sauty:—Till not troubled by stoppage, "every day I shall send, and you receive, from 12 to 1, from 2 "to 3, from 4 to 5, &c. You send from 1 to 2, from 3 to 4, &c. "Never stop during hour. Give reversals when not speaking."

Newfoundland to Valentia.

Sent 10.62 P.M.—"Is this Greenwich time? If so, give us "one o'clock."

Valentia to Newfoundland.

Sent 10.20 P.M.—"Yes; we begin now and to 11, if you "understand."

Newfoundland to Valentia.

Sent 10.24 P.M.—"Understand."

NINETEENTH DAY.

August 28, 1858.

Newfoundland to Valentia.

Sent 1.06 A.M.—"To the Directors:—Take news first Saward. "Sir William Williams, of Kars, arrived Halifax Tuesday. "Enthusiastically received. Immense procession, welcome "address, feeling reply. Held levee; large numbers presented. "Niagara sailed for Liverpool at one this morning. The Gor- "gon arrived at Halifax last night. Yellow fever in New "Orleans, 60 to 70 deaths per day. Also declared epidemic "Charlestown. Great preparations in New York and other "places for celebration to be held 1st and 2nd Sept. New "Yorkers will make it the greatest gala day ever known in this "country. Hermann sailed for Fraser's River; six hundred "passengers. Prince Albert sailed yesterday for Galway. "Arabia and Ariel arrived New York; Anglo-Saxon, Quebec; "Canada, Boston. Europa left St. John's this evening. Splen- "did aurora. Bay Bulls to-night, extending over eighty-five "degrees of the horizon. "DE SAUTY."

Valentia to Newfoundland.

Sent ——.—"Understand 'per.' Send faster."
Sent ——.—"Repeat 'Sept.' to 'day ever.'"
Sent 1.16 A.M.—"Take one from Thomson. Can you read?"

Newfoundland to Valentia.

Sent 1.20 A.M.—"Understand 'yes;' understand."
Sent 2.57 A.M.—"After 'fifty yards' repeat."
Sent 4.15 A.M.—"We received badly all day Monday; weak "signals on after fifty yards."
[Messages 252 and 253 referred to some parts of a message which they could not make out in Newfoundland, and which they did not succeed in obtaining.]
Sent 9.27 P.M.—"Nothing received."
Sent 10.28 P.M.—"Say if you have received Thomson's."

Valentia to Newfoundland.

Sent 10.40 P.M.—"No; send it."

Newfoundland to Valentia.

Sent 11.58 P.M.—"Shall repeat message Thomson."

TWENTIETH DAY.

August 29, 1858.

Valentia to Newfoundland.

Sent 2.31 P.M.—"How are my signals?"
[No communications from Newfoundland to-day.]

TWENTY-FIRST DAY.

August 30, 1858.

Newfoundland to Valentia.

Sent 12.25 A.M.—"Can you read?"

Valentia to Newfoundland.

Sent 1.25 A.M.—"Yes, we can read you. Send news slowly. "Saward *asks where Kells is. How are my signals? Persia "arrived Saturday.* Receive *on one galvanometer only, fault "signals, indicted* currents from coil *of your larger galvanometer.*"
[None of the words italicized were read in Newfoundland.]

Newfoundland to Valentia.

Sent 2.45 A.M.—"Can read some of your sending. Take this "message:—New York. The Directors Atlantic Telegraph "Company, London. Parties pressing upon us messages for "Europe. When will line be open for business. Has Mr. Mor- "gan sailed for New York? Early in the morning of Sept. 1, "please send me message that I can read at the celebration that "day, and another on the 2nd I can read at dinner that even- "ing. "C. W. FIELD."

TWENTY-SECOND DAY.

August 31, 1858.

Valentia to Newfoundland.

Sent 1.30 P.M.—"Can you read? We have two Government "messages. Will you take? Reply direct."

Newfoundland to Valentia.

Sent 1.37 P.M.—"Try, but send."

Valentia to Newfoundland.

Sent 3.41 P.M.—"The Military Secretary to Commander-in- "Chief, Horse Guards, London. To General Trollope, Halifax, "Nova Scotia:—The Sixty-second Regiment is not to return to "England."
[This message and that which will be found further on in re- gard to the Thirty-ninth Regiment, saved to the British Go- vernment the sum of fifty thousand pounds (250,000 dollars), by avoiding the shipment and transportation of troops.]

Newfoundland to Valentia.

Sent ——.—"This received:—The Military Secretary to "Commander-in-Chief, Horse Guards, London."

Sent ——.—"'Trollope,' understand. Go on after 'Scotia.'"

Sent ——.—"Is it finished after 'England?'"

Valentia to Newfoundland.

Sent 4 P.M.—"Yes. Now take another. Are you ready?"

Newfoundland to Valentia.

Sent 4.5 P.M.—"Yes, send."

Valentia to Newfoundland.

Sent 9.3 P.M.—"The Military Secretary to Commander-in-"Chief, Horse Guards, to General Officer commanding, Mon-"treal, Canada:—The Thirty-ninth Regiment is not to return "to England."

Newfoundland to Valentia.

Sent ——.—"I want you to repeat 'Canada.'"

Valentia to Newfoundland.

Sent 10.5 P.M.—"Can't read. Try 'Daniel's."

Newfoundland to Valentia.

Sent 12 P.M.—"Repeat from 'Canada' to 'return.'"

TWENTY-THIRD DAY.

September 1, 1858.

Valentia to Newfoundland.

Sent 12.20 A.M.—"Canada—The Thirty-ninth Regiment is "not to return."

Newfoundland to Valentia.

Sent 12.28 A.M.—"Understand. Will you take a service?"

Valentia to Newfoundland.

Sent 12.30 A.M.—"I will try. Slow."

Newfoundland to Valentia.

Sent 8.30 A.M.—"We have received nothing since you "repeated last."

Valentia to Newfoundland.

Sent 11.27 A.M.—"Can you take message?"

Newfoundland to Valentia.

Sent 11.31 A.M.—"Yes."

Valentia to Newfoundland.

Sent 12.49 P.M.—"C. W. Field, New York:—The directors "are on their way to Valentia to make arrangements for open-"ing wire to public. They convey through cable to you and "your fellow citizens their hearty congratulations and good "wishes, and cordially sympathize in your joyous celebration "of the great international work."

Newfoundland to Valentia.

Sent 12.52 P.M.—"Forty-eight words. Right, right." [These were the last words received at Valentia.]

Valentia to Newfoundland.

Sent 12.54 P.M.—"Right (signal understand)."

[Some portions of the following message were received in Newfoundland]:—

Sent 1.30 P.M.—"*C. W. Field, New York, please inform* "American *government we are now in position to do best to forward* "their government messages to England. Saward, London." [The words italicized in the message were received in Newfoundland, and they were the last received from Valentia.]

A SUMMARY showing the NUMBER of MESSAGES and the WORDS and LETTERS they CONTAIN sent through the ATLANTIC CABLE from VALENTIA to NEWFOUNDLAND.

Day and Date.	No. of Messages.	No. of Words.	No. of Letters.
Friday, Aug. 13	9	7	47
Saturday, Aug 14	10	14	76
Sunday, Aug. 15	1	1	4
Monday, Aug. 16	12	169	835
Tuesday, Aug. 17	4	9	43
Wednesday, Aug. 18	—	—	—
Thursday, Aug. 19	8	30	125
Friday, Aug. 20	22	198	967
Saturday, Aug. 21	10	126	626
Sunday, Aug. 22	5	38	188
Monday, Aug. 23	2	14	70
Tuesday, Aug. 24	14	301	1,475
Wednesday, Aug. 25	6	99	490
Thursday, Aug. 26	4	119	595
Friday, Aug. 27	5	166	823
Saturday, Aug. 28	4	19	83
Sunday, Aug. 29	1	4	15
Monday, Aug. 30	1	13	65
Tuesday, Aug. 31	8	85	411
Wednesday, Sept. 1	3	62	310
Total, 20 days	129	1,474	7,253

A SUMMARY showing the NUMBER of MESSAGES and the WORDS and LETTERS they CONTAIN sent through the ATLANTIC CABLE from NEWFOUNDLAND to VALENTIA.

Colonel Tal. P.-Shaffner.

14 March 1860.

Day and Date.	No. of Messages.	No. of Words.	No. of Letters.
Tuesday, Aug. 10	16	54	231
Wednesday, Aug. 11	12	41	182
Thursday, Aug. 12	5	43	167
Friday, Aug. 13	27	221	1,103
Saturday, Aug. 14	17	249	1,238
Sunday, Aug. 15	8	58	285
Monday, Aug. 16	18	129	623
Tuesday, Aug. 17	10	268	1,335
Wednesday, Aug. 18	2	12	60
Thursday, Aug. 19	21	352	1,740
Friday, Aug. 20	23	415	2,054
Saturday, Aug. 21	17	121	565
Sunday, Aug. 22	14	140	700
Monday, Aug. 23	4	18	90
Tuesday, Aug. 24	12	154	764
Wednesday, Aug. 25	14	129	640
Thursday, Aug. 26	13	109	546
Friday, Aug. 27	13	67	335
Saturday, Aug. 28	7	170	850
Sunday, Aug. 29	—	—	—
Monday, Aug. 30	2	76	375
Tuesday, Aug 31	7	39	187
Wednesday, Sept. 1	4	20	98
Total, 23 days	271	2,885	13,968

For CYRUS W. FIELD, ESQ.

Correct diary of messages sent through the Atlantic cable 1858.

July 4, 1859.

CYRUS W. FIELD, ESQ.:—

I hereby certify this document to be a faithful and complete record of the communications and messages sent and received through the Atlantic telegraph cable during the period of it working—viz., from August the 10th to September the 1st inclusive—extracted from and collated with the diaries kept at the respective stations, at Valentia, Ireland, and Cyrus Station Bay of Bull's Arm, Newfoundland.

HENRY W. IRVIN,

Of the Electric Staff in Newfoundland.

London July 7, 1859.

CONSULATE OF THE UNITED STATES OF AMERICA, LONDON.

I, Robert B. Campbell, Consul of the United States of America for London and the dependencies thereof, do hereby certify that on this 6th day of July, in the year of our Lord one thousand eight hundred and fifty-nine, before me personally appeared and came Henry W. Irvin, to me known, who signed the following certificate in my presence, and then and there acknowledged the same to be his free act and deed. In testimony whereof I have herewith set my hand and affixed my seal of office at London, this day and year above mentioned, and in the eighty-third year of the independence of the said United States.

[SEAL.] ROBERT B. CAMPBELL.

This is to certify that I was on board the Agamemon during the laying of the Atlantic cable, and employed on the electrical staff from the time the cable was landed, August 5, 1858, to the 4th of November 1858, inclusive, and that the annexed is a correct account of messages sent and received through the Atlantic cable from August 10 to September 1, 1858, both inclusive.

EDWARD BULL,

Of the Electrical Staff at Valentia, Ireland.

I have not had an opportunity to sift the correctness of the preceding statement. A practical telegrapher can see that there are many explanations necessary to harmonize the respective parts. It will be liberal to admit that all the *words*, and all the *letters* composing the words represented in the statement, were actually *sent* and *received* through the cable, and not in any manner abbreviated or represented by arbitrary signals, common in practical telegraphy. The secretary of the Atlantic Company published a statement that 99 words had been transmitted through the cable in 67 minutes, being less than 1½ words per minute. Suppose that 99 words were actually received through the old cable in 67 consecutive minutes, or at the rate of 90 words per hour, the result was not favourable. The line was then capable of sending but 1,800 words per day, 18,080 messages of twenty words each per annum, which a

Colonel Tal. P. Shaffner.

14 March 1860.

2*l.* would amount to but 36,160*l.* I do not think that such an income could however be realized on a line of that circuit and conduction. The capital of that company was over 400,000*l.*, and the interest upon that capital and expense of administration would have required an income of at least 45,000*l.* I have now explained what was the result of the old Atlantic line, and what would probably have been realized. This too was after the company had assured the world that ten words per minute could be expected through the cable, and that such a result had been conclusively demonstrated.

The North Atlantic route, viâ Greenland, will be composed of four submarine sections, each composing an independent electrical circuit, as follows, viz. : from Scotland to the Faroe Isles, about 250 miles of cable, from thence to Iceland, about 350 miles ; from Iceland to Greenland, about 550 miles ; and from Greenland to Labrador, about 600 miles of cable. One line of cables laid from Scotland, via., Faroe Isles to Iceland can be so constructed as to transmit equal to three cables, or lines of communication, laid across the other two sections of the route, and if the two longer sections of the cables be constructed conformably to fixed and demonstrated laws of commercial telegraphy, twenty words per minute can be realized through the cable, seventeen of which may be considered as commercial. It remains a financial problem whether the cables shall be constructed so as to commercially transmit ten, fifteen, or twenty words per minute. On short circuits, like those of the northern route, the solution lies, side by side with our present experience in submarine telegraphy, while on the other hand, no such results can be attained on long circuits, such for example, as from Ireland to Newfoundland.

The maximum speed of transmission of messages over telegraph lines may be estimated according to the following data :—A line that transmits 40 words per minute may be considered as perfect, and not subjected to any hindrances whatever. One word in forty must be allowed for the line, leaving 39 words as commercial. But, besides, in a day of twenty-four hours, at least one-sixth of the time must be allowed for administrative affairs, such for example as "paid," "not paid," "repeat," "get answer," "don't understand," "send letters," "write slower," "put on more battery," &c., &c. There are many explanations that have to be sent over the line from time to time, during the day. A line that can transmit but 30 words per minute, I allow two for the line, leaving 28 commercial words. A line that can transmit but 20 words, I allow three for the line and 17 for commercial words ; a line that can transmit but 10 words per minute, I allow 4 for the line and 6 for commercial words ; and a line that can transmit but 5 words per minute, I do not estimate any commercial result, though a few intelligible signals might be received. The preceding scale of manipulation is based upon observations and actual telegraphic experience extending over both hemispheres and many years of toil.

I have already explained the telegraphic result that was effected by the old Atlantic line, and from the record of that company demonstrated that it never had a probability of commercial success. On the northern route, cables of required commercial capacity can be laid and maintained. If a cable, capable of transmitting but 10 words per minute be laid, giving 6 commercially, the result will be, 360 words per hour, 7,200 per day, 360 messages of 20 words, which at 2*l.* each will produce 720*l.* per day, or 224,640*l.* per annum ; and deducting 20,000*l.* for expense of administration, there will be a nett income of 204,640*l.* A line capable of producing this result can be laid for a less sum than the capital of the old Atlantic line, which was about 400,000*l.* But a line can be constructed on the northern route, that will produce 20 intelligible words per minute, giving 17 commercial words, and 1,020 per hour, 20,400 per day, 1,020 messages of 20 words each per day, which at 2*l.* each will produce an annual income of 636,480*l.*

If light cables can be made to subserve the purpose of ocean telegraphy, such as recommended by Lieutenant Maury, the expenditure of the same amount of money as before mentioned, by means of an increased conduction, an income equal to 1,900,440*l.* per annum can be realised, and the cost of administration not materially augmented.

In electric telegraphy, as in all other commercial affairs, I have long since learned the impropriety of expending money upon speculative theory ; never go beyond demonstrated practicabilities. The capacity of every circuit that will be on the northern route is known by their equals in length, already operating in deep seas. We have nothing to experiment upon ; nothing to invent or discover.

Adjourned.

Saturday, 19th May 1860.

PRESENT :

Captain DOUGLAS GALTON. | Professor WHEATSTONE.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

Mr. C. C. Forde.

19 May 1860.

Mr. HENRY CHARLES FORDE examined.

4142. (*Chairman.*) You are a civil engineer ?—Yes.

4143. Have you had considerable experience in telegraphic engineering ?—Yes, during the last four years.

4144. You have lately been concerned in laying the Red Sea telegraph, have you not ?—I have, between Aden and Kurrachee, and with Mr. Gisborne in the whole of the line.

4145. You personally superintended the line between Aden and Kurrachee ?—Yes.

4146. And the other portion, I think, was superintended by Mr. Gisborne ?—Yes.

4147. Is the cable between Aden and Kurrachee of the same construction as the cable between Suez and Aden ?—It is in every respect the same.

4148. What are the depths in which that cable is laid ?—The depths vary from the minimum to 2,000 fathoms the maximum.

4149. Whereabouts is the greatest depth ?—The greatest depth is across the Gulf of Oman, from the coast of Beloochistan across to Muscat.

4150. Had you soundings taken previously to laying the cable ? — Yes, H.M.S. "Cyclops" took the soundings.

4151. Did you experience any difficulties in laying the cable across the deeper portion ?—The only difficulty, or rather anxiety, that we experienced was about the paying out machinery, which to a certain extent gave way ; it never quite broke down, and therefore we succeeded in laying the cable.

4152. What part of the paying-out machinery gave way ?—The paying-out machinery principally consists of an iron drum, with break strap round it ; this iron drum has 12 spokes, six on each side, and 10 spokes out of the 12 broke.

4153. And fell off ?—No ; they did not fall off, but some were parted several inches, and that part of the

deep portion of the cable was laid, with a broken drum for about 70 miles; but there was no difficulty in laying it.

4154. Is that cable laid and in perfect working order now?—As far as I know it is in perfect working order; it was when I left Aden, one month after it was laid.

4155. Then there is no flaw in the cable between Aden and Kurrachee?—As far as I know there is none between Aden and Kurrachee. The section between Aden and Kooria Mooria was reported out of order one week, and the next it was contradicted from Aden. We have not yet had the particulars.

4156. Do you attribute the defect to the instruments?—We suppose it to be the instruments, or some connexion in the office; but we have not yet received any definite information as to that line, so that we hope it is in good working order.

4157. Is it not the fact that one link of the portion between Aden and Suez is not in working order?—Yes; they are repairing the line now. I hope it is repaired by this time.

4158. Have you any records of the electrical condition of the line during laying?—There is the electrical log, which has not yet been sent to us from Berlin; it has to come through Messrs. Siemens and Halske.

4159. Do you expect to have it soon?—Yes.

4160. Can you let us have the drawing, as we have of the other portion?—Certainly, when it is plotted. I should think there can be no objection to give it to you. I have none.

4161. Were there any peculiarities observed during the laying of the cable?—We commenced at Kurrachee, and we laid the whole line to Muscat with only one check. The electricians thought there was some fault in the cable. It was in connexion with the earth plate, which got deranged, and it showed a difference upon the instrument used, which was a particularly delicate one; but the difference was not observed upon the ordinary tangent galvanometer. The earth plate was examined, and it was found that the cable was in every respect perfect.

4162. Was the weather very hot for laying the cable?—No; the temperature was from 76° to 80°.

4163. Did you observe the temperature of the water?—No, I did not observe the temperature of the water; it was taken, but I do not know what it was. The detailed information is not yet in my hands, and any information I could give you would be incomplete.

4164. Did the insulation improve when the cable was being paid out?—It improved rapidly. I may say that it was twice as good when in the depth of 2,000 fathoms as it was on board ship; that is to say, the resistance which the gutta-percha offered to the escape of the electricity was twice as great, speaking in round numbers. I have not got the exact figures, but that was about what I ascertained when I inquired into it.

4165. The part of the apparatus which got out of order was not the cones but the paying out drum?—We did not expect more than 1,000 fathoms of water; the paying out machinery is made as light as possible, and when we had to pay out in over 2,000 fathoms of water it seems that the strain was excessive for the thickness of metal in the drum.

4166. Did you measure the strain as the cable was going out?—It was measured, but there was no indicator on the machine to denote the exact strain on the cable; there is a moveable arrangement with a lever and wheel at the end of it, which is weighted, in proportion to the depth of water, and it is just kept moving to insure as far as possible that no undue strain may come on the cable.

4167. Was the electrical department under the contractor?—Under the contractor principally; the contract was to lay the cable, and give over the line to the company, in good working order, 30 days after it was laid, therefore the electricians were in his employ.

4168. Had you no electrician on your part?—We had Mr. Brunton, who is the company's superintendent and electrician.

4169. To what do you attribute the improvement in the insulation when the cable was paid out?—I attribute it principally to the coolness of the water, and to a certain extent to pressure; as the gutta-percha got cooler the insulation increased: we have always found from our tests that this is the case.

4170. You said that you did not expect more than 1,000 fathoms. I suppose you had not had soundings taken of the whole line?—We did not get the soundings until we arrived at Kurrachee, and there we met the Cyclops. The soundings had been taken previously to our arrival, but we had not received them in England.

4171. Would you have any hesitation in laying a cable of that size (*the Gibraltar cable*) made of steel and hemp in 2,000 fathoms?—Not the slightest.

4172. Do you think there would be advantages in laying an iron covered cable over an iron and hemp cable?—The advantages are in favour of hemp and iron, or hemp and steel.

4173. Even if the iron cable is sufficiently strong to be laid in that depth?—Yes.

4174. Why?—In the first place one great danger to be avoided is a broken wire; if an iron covered cable is very heavily strained the wires are liable to break and get entangled in the machinery, which might cause a jerk and break the cable.

4175. Is not the maximum strain on the cable after it has passed round the drum?—After it has passed round the drum the maximum strain is from the drum to the bottom of the sea.

4176. Would broken wires interfere with the machinery?—Yes; because a broken wire before it comes to the drum might get entangled with the machinery and cause a jerk on the cable.

4177. That is your principal reason for preferring a cable of hemp and iron, or hemp and steel?—That is one reason. Another reason is, that by a combination of hemp and steel you get greater strength in proportion to its weight; and therefore it is less liable to break; you have a larger margin to go upon.

4178. Would you use a hemp and steel covered cable at moderate depths, anything over 300 fathoms?—From 300 to 1,000 fathoms I should not.

4179. Why not?—Because I think perhaps it is more expensive to make, and I do not think it is required for those depths. It is more trouble to make. No doubt the weight of the steel is much less.

4180. Do you consider it desirable to have iron in the outer covering for the purpose of protecting the cable from insects after it is laid, or merely to give it strength during the process of laying?—Principally to give it strength during the process of laying. I can scarcely believe that the iron will protect it much from insects at the bottom. After it is stretched in the paying out it is liable to get into kinks at the bottom, or, at all events, it is lying at rest and it is almost certain that at some point the wire opens sufficient to admit insects. It is, to a certain extent, a protection against animals.

4181. Do you consider a hemp covering desirable as protecting the iron against oxidation, in case you might want to raise the cable again in parts that are not in great depths, but in moderate depths?—I do not know how far it would prevent oxidation. If it did prevent oxidation, I think it would be an improvement. In case of repairs you require strength, of course, in a cable of moderate depths.

4182. You do not attach great importance to the protection of iron in moderate depths?—No, I do not. Where anchors are likely to come across the cable of course you must have as much strength as possible to protect it.

4183. Do you attach great importance to protecting the outer covering of the cable from oxidation in moderate depths?—Yes if it can be accomplished in a good and practical manner.

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4184. Then, for the greater depths you consider that a cable covered with hemp, combined with steel, is the best that can be laid?—I do, so far as I know at present.

4185. You prefer it to a cable entirely covered with hemp?—Certainly.

4186. What are your reasons for that preference?—Because the hemp covering to a cable gives no support to the core. The core is very liable to be stretched. I have found that is the case in experiments. The hemp is also liable to contract and strangle the core when immersed in water.

4187. Suppose a cable is made of hemp, upon which a weight would produce a very moderate degree of extension, would you object to that?—If you can make a cable in that way, probably it would be a good form of cable; but the cable must have weight enough to pull itself out of the ship. The contraction of the hemp is a serious objection.

4188. You attach no importance to the outer protection for the iron, as I understand, if the cable is laid, or very little importance in great depths?—I do with reference to repairs in moderate depths should the outer covering prevent corrosion, but in great depths that is not of much importance.

4189. Then you would be perfectly satisfied with a cable, so that you could get it paid out covered with hemp?—If you could prove to me that it would not stretch in the paying out nor shrink afterwards. There is another thing safer in an iron-covered cable: in coiling it up in a ship the iron cover prevents anything in the shape of weight pressing on the core, or mechanical injury from falling weights.

4190. The iron prevents the core flattening?—Yes.

4191. Would that be equally effective with a cable of iron and hemp?—I think so.

4192. Because the core does not rest against the iron, but has a soft hemp cushion where it is liable to get squeezed?—I think that would protect the core sufficiently, because there is not much weight in a cable coiled up in a ship; but still it is desirable, in case of anything falling on it, to have it protected to a certain extent. I think the hemp and steel as designed for the Gibraltar cable would be quite sufficient for that purpose.

4193. Have you made any experiments upon hemp-covered cables?—I have tried three or four.

4194. Do you remember the names of the inventors of those specimens that you tried?—There was no inventor; they were made according to a specification sent to Newall and Company, and were simply covered with hemp. Some were covered with hemp and iron, and others with hemp and steel.

4195. What you are describing as a hemp-covered cable would be a cable with strands of hemp instead of strands of iron or steel;—Yes.

4196. Have you tried De Burgh's cable?—No, we never got one sufficiently long.

4197. Or Sinnot's?—No, I do not think that was tested.

4198. Or Hall and Wells', which was a cable that instead of having iron in it had strands of iron running longitudinally?—We did not test any of Hall and Wells' as well as I remember. I remember his showing me a specimen, and also trying to decide with them upon the best form made by their machine. They brought me one which was so clumsy, I told them it would not answer at all. I told them it was not the slightest use testing it so, that they did not make any more. I wanted them to make it smaller in diameter and with less hemp in proportion to the wire, which they objected to do.

4199. You did not test this one of Mr. Siemens?—No I did not test this; it was designed while I was out with the Indian cable.

4200. Do you consider any inconvenience arises from the spiral form of the outer covering of the cable?—No, I do not. An objection has been urged that the spiral form causes the iron to untwist during the paying out; that is an error. I paid particular attention to that point, and I found that the cable in paying out sometimes took a turn one way and sometimes the contrary way. I attribute this wholly to the coiling. The cable when uncoiling takes out any turns or twists it may have got during the coiling. A cable which has been very often coiled and uncoiled, as the Atlantic was, would be full of twists, which in paying out would give it the appearance of untwisting the wire. It is dangerous to coil or uncoil a cable often, because each twist it gets is an incipient kink.

4201. Another argument which is used against the spiral covering is, that it tends to increase the extension that would take place if a straight pull came upon the cable?—I am aware of that objection, but all the cables of that form that I have tested extended very little indeed before they broke. I think, as well as I remember, under one per cent. I have stretched the core 25 per cent. I stretched the Gibraltar core more than 25 per cent. without any apparent injury to it electrically.

4202. If the outer covering stretches a certain extent and carries the core along with it, the argument, I think, is, that the outer covering goes back, but the core does not go back, or rather the copper does not go back?—That is in stretching the core. The gutta-percha has a tendency to go back, the copper has not.

4203. If the outer covering and the core are both stretched, for that reason it is desirable, is it not, to have an outer covering which shall allow of as little extension as possible?—Certainly,

4204. You said that the core would stretch 25 per cent., while the outer covering stretched only one per cent.?—I have stretched the naked core itself to that extent to ascertain how far the gutta-percha is capable of stretching. There is no doubt that the copper wire stretches also, but the gutta-percha has a tendency to contract, and the copper wire has not.

4205. Have you found what amount of extension the copper wire will bear with safety, so as to avoid its being forced through the outer covering?—No, I cannot say that I have. I have still got the specimen of core that was stretched 25 per cent., and the copper wire is not forced through. Soon after it was done, as a matter of experiment, I cut the gutta-percha longitudinally, and when I had cut very nearly to the copper then the copper came out to a considerable extent, but I found that the using of what is called Chatterton's compound over the copper and under the first coat of gutta-percha sticks as it were the copper to the gutta-percha, and thereby any tendency that it has to contract afterwards is distributed over the whole surface.

4206. Have you observed that in the Gibraltar core the compound does not penetrate thoroughly into the strands of the copper wire, and therefore that a sort of tube permeable to the water is made?—Yes, I have observed that and tried experiments upon it; it is a fact that it will allow water to permeate through it.

4207. Would you prefer the tube which is so formed by the strands of copper wire being entirely closed up with the compound?—I do not know whether I should prefer it or not, but my objection to having the strand perfectly saturated with this compound is, that I am afraid there would not be metallic contact throughout the conductor; it is more an electrical objection than anything else, but I may not be quite correct.

4208. (*Professor Wheatstone.*) In what way would the compound destroy the metallic contact in your opinion?—I think if the strand is perfectly saturated with this compound the compound may separate the different copper wires, and instead of their being in metallic contact, each wire may be separately insulated.

4209. That would be of no consequence so long as the continuity of each wire was maintained?—I do not feel myself capable of giving a decided opinion upon it. If there is no apprehension upon that sub-

ject, I am in favour of having the strand laid up with compound. It is, however, probable, that induction would be increased, the insulated surface being much larger.*

4210. (*Chairman.*) What is the speed of working in the longer lengths of the Red Sea telegraph, perhaps you can give the working in each section?—I can in general terms as near as I know; they are not very accurately known, but from what I have seen myself in the sections between Kurrachee and Aden, the first section which is about 480 nautical miles long, the next about the same, and the third 730; the speed in the two first sections you may say is 12 words a minute, and the speed on the long section is reduced to five or six words as the maximum.

4211. On the 480 miles the speed is 12 words a minute?—Yes, I believe a good manipulator might get more.

4212. That is the practical speed you can get?—Yes.

4213. And on the 730 miles it is reduced to five words?—From five to six, about half.

4214. You have no length of a thousand miles?—No, 730 knots is the longest length.

4215. What are the causes of the failure of the Red Sea telegraph in the sections in which it has failed?—I am scarcely prepared to say that positively, as all the information we have upon the subject is contained in short telegrams; the faults occurred after I left.

4216. What was the reason of the early fault which occurred just after the cable was laid?—Not having been on board ship I can hardly say; one of the faults was in consequence of the ring jamming during the operation of paying out.

4217. After it had been laid there were faults repeatedly occurring before you went out there which were repaired, were not there?—No, the only fault that I know of was repaired before I went out; that arose from an anchor after the line was laid and handed over to the company, it was broken by an anchor at Cosier.

4218. Another fault occurred after the period of which you are speaking?—Six months after the laying of the line.

4219. What distance from the end of the line?—I think the principal fault in the Aden and Sunkin section was on the Dallack bank.

4220. Is that a coral reef?—It is coral sand and mud.

4221. Are there any currents there?—No, except surface currents.

4222. Is the temperature of the water high?—I do not know what the maximum temperature may be. I have not the memorandum here of what it was when they laid that line, so that I cannot state; we have it in the log.

4223. You have not yet learned the cause of the failure?—No, not exactly, but my opinion is from what I have heard of the nature of one or two faults that appeared in the Dallack Bank, there were 12 wires out of 18 of the outer wires broken, and I am afraid the faults are attributable in the first place to that line being laid during the hot weather when the gutta-percha was within a few degrees of melting, and also I think another cause of the failure was in laying with a great strain, passing from deep water into shallow water, as the faults appear on the edge of the bank principally.

4224. (*Professor Wheatstone.*) At present is there a total cessation of the current or merely a diminution?—I am not prepared to say whether it is a total cessation. When the first fault occurred there was a total cessation of the current, probably with a very delicate galvanometer you might find a current.

4225. Do insects attack the gutta-percha much in the Red Sea?—We have not found it out, if they have—not to my knowledge. In some of these faults it appears that at some weak points the gutta-percha has been burned from the battery power used in working.

4226. Do you use very powerful batteries?—We use Daniell's; about one cell for every six miles. I think that is about the maximum we use, and in many cases we use less.

4227. Do you keep them well charged?—They are kept well charged, and renewed periodically. They are renewed ten cells at a time.

4228. (*Chairman.*) Did you observe many cases of eccentricity in the Red Sea cable when it was paid out, or any?—No; we had not the means of detecting any eccentricities during the paying out.

4229. You found no faults in that respect?—We found no faults in laying the line.

4230. How did you keep the cable cool in its passage out?—It was all battened down in cylinders with air chambers. The temperature was kept as low as possible by means of wind sails, and keeping the sun out.

4231. What was the temperature of the hold?—The temperature of the hold was about the temperature of the air; there was hardly any difference—about 76 to 78 degrees this last expedition.

4232. Through the whole of the passage out?—I am not aware of that. I did not make any inquiry. All we did was to test the cable at Kurrachee, and it tested exactly the same as it did when it left England.

4233. Have you considered the question of the different species of insulators?—No, I have not.

4234. Not more than gutta-percha?—Not more than gutta-percha and india-rubber.

4235. Have you made experiments on any of the various compounds that have been proposed?—No, I have not; we look more to the electricians to do that.

4236. Have you considered the question of coating the outer covering of the cable with anything like marine glue, to preserve it?—No, I have not; especially because I do not see that there is much advantage in it, unless it is to prevent the wires from rust.

4237. You do not attach much importance to that?—No, unless it keeps the wires from rust, and then I do to a certain extent.

4238. Have you seen a piece of the Atlantic cable which has been lying in the sea for some time, in which all the wires are rusted away, and present points like so many needles?—No; were those needle points caused in picking up the cable, or was the cable eaten through with rust?

4239. Eaten through before it was picked up?—I have not seen them. The same thing has occurred in the Red Sea.

4240. Does not that lead you *to think* that it would be desirable to protect the wires from rust?—Certainly, if you can do it, protect them from rust by all means.

4241. I am speaking of the shore ends, or parts which are liable to injury, and where you require to pick them up?—Any parts which are liable to injury from anchors, or anything of that sort, I think it would be desirable to protect from rust. But is it certain that it would do it?

4242. Have you formed any opinion as to the best sort of paying out apparatus for deep sea cables?—My opinion is, the simpler the machine the better—a simple break drum, with an ordinary friction band and lever.

4243. How many times would you carry the cable round the drum?—Five or six times, according to circumstances, subject very much to the depth of water. I should say that six times is ample. I am now getting some paying out machinery made at Easton and Amos', for the Red Sea, which is simple,

* N.B.—Since the above evidence was given, one knot of Gibraltar core has been made with the strand laid in compound; and, as far as the experiments upon such a short length can be depended upon, it appears that neither the conduction or the induction are affected.—H. C. Forde.

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and, I think, about the best kind that can be made, so far as I know.

4244. What size drum do you use?—We are going to use in this instance a drum 5 feet in diameter; but the Indian line was laid with a 7 feet drum. This machinery that I speak of is only for a small steamer the company are sending out to pick up and repair, and lay short lengths, if necessary. A 7 feet drum is ample under all circumstances.

4245. With a cone?—Yes; I think I should use a cone and rings; it is very desirable.

4246. Have you any other suggestions to make?—I think hitherto we have covered the wires with too few coats of gutta-percha. The Gibraltar cable has been more under our control than any other cable. The contractors in other cases took the whole risk and responsibility, and have had too much in their power; but from what is known of testing under pressure now, and the careful overlooking the core gets, the more coatings of gutta-percha are put on the safer the cable is.

4247. Would you test the core under pressure after it has had, say two coats, and then again after it has had the remaining coats?—I should like to do so, but in testing in water, the water must be perfectly clean; the liability to impurities prevents the next coat from sticking.

4248. The next coat of Chatterton's compound?—Yes; the impurities are more likely to get in under pressure.

4249. Is not that rather urged by the contractors, because it would increase the trouble of covering, than from any other reason?—It may be. I have examined the core after it has come out, and it is very slimy and dirty; no doubt it might be washed.

4250. If they change the water every day or every two days, would there be much difficulty from these impurities?—As impurities are sure to get in, I think, with a little more trouble, the core might be passed through cleaner water, and then covered again; it would add to the expense; the oftener it is overlooked, the more expensive it becomes.

4251. The great expense is in the labour?—Yes; I should like to see it done, certainly.

4252. What number of coats would you propose for an ordinary cable?—I think six would be quite enough; it depends very much upon the thickness of the gutta-percha.

4253. What thickness of gutta-percha would you propose for each coat?—I would make them as nearly equal as it was possible to be done. The core of the Gibraltar cable is half an inch in diameter, as nearly as possible. The copper wire, I suppose, is the tenth of an inch. I should put the gutta-percha on in four or six equal coats. There is about two-tenths of an inch thickness of gutta-percha, so that each coat would be about one-third of a tenth, that is, the thirtieth of an inch.

4254. Would you have the same thickness of Chatterton's compound?—It would be in thinner coats than the gutta-percha. In testing this core, we found occasional air holes go through the two coats, and we had to cut through the two coats to get to the bottom of the air hole.

4255. In a great many cases, of course, you must have missed the air hole. If you only tested the outer coat you could only detect the air hole in the outer coat, except in the accidental and rare cases when you met with one air hole coinciding in two coats?—Yes; you may miss an air hole, and there may be another air hole underneath not detected.

4256. Is the extra expense sufficient to deter you from testing under pressure more than the outer coat?—No; I think it is worth while to incur an extra expense. If you have four coats, test when two coats are on, and test again when the other two are on. The expense of doing that would not exceed 1*l.* a knot, I think.

Captain Sir EDWARD BELCHER, C.B., examined.

Captain Sir E. Belcher, C.B.

4257. (*Chairman.*) You have had considerable experience, I believe, in polar navigation, both north and south?—On both sides of America, in Behring's Straits as well as the other side. More particularly in Behring's Straits and the banks of Newfoundland among the icebergs.

4258. Have you formed any opinion as to the practicability of laying a cable to America by way of the Faro Islands, Iceland, Greenland, and Labrador?—I have.

4259. Assuming that we start from England, you would go to the Faro Islands, I presume?—I should prefer going to the Shetlands first.

4260. From the north coast of Scotland?—From the north coast of Scotland, viâ the Orkney Islands.

4261. And from the Shetlands to the Faro Islands?—Yes.

4262. Have you considered or observed the tides and whirlpools which are said to exist near the Faro Islands?—I have not; nor have I witnessed any of those whirlpools on the direct passage to Greenland. I think they belong peculiarly to the islands and to the tides which belong to the islands. I consider that the tides in the vicinity of the land always differ materially from the tides off shore.

4263. Do you anticipate that there would be any liability to injury to a cable from friction on the rocks in consequence of those tides?—I do not. I consider the information that we have at present of those islands is very imperfect, and that it is almost absurd in any person until he does obtain the information that is requisite, as to where the banks lie, and whether those banks are mud or sand or rock, to assert that the thing cannot be done. I have had a great deal of experience all over the world in dealing with rocks near the land, but I never knew of any rock bottom in 100 fathoms off shore except in the Coral Islands in the Pacific. There, I think, at 960 fathoms we lost the rock, and the bottom was found to be sand. After it has been *determined*, how the rocks lie, and if there are available gullies between them, containing mud or sand, then it will be easy to place in such situations cables adapted to the cases. In all such cases (as it has been imagined apply to islands like Shetland and the Faro Islands,) I would venture to suggest that you have been dealing with others much more liable to objection in the Mediterranean, and specially as to terrestrial heat! I view the islands above water as merely apices of what would be disclosed if the sea receded; and if abrupt ravines discover themselves at the coast line, they may naturally be looked for as continuous below water. But being filled with mud and debris, they offer a good bed for a cable, judiciously laid, in any of these determined hollows. Therefore, I must be understood in my replies to mean, that I would not run the cable *direct* and *across ridges*, but carry it to *the communicating branch*, which I suppose would run in the meridian north and south from the positions in question.

4264. Is the mud as a rule deep at 100 fathoms?—I think whenever you come to mud at that depth it is very slimy mud. In all cases where I have seen rope or any material that has been sent down in very deep water the mud has adhered to it when it came to the surface with great tenacity, and it was very difficult to wash it off. A very peculiar sticky mud is found at great depths. I think if you were to determine the outline of the soundings at 100 fathoms, and bring the cable up into the meridian of any of those points to which you propose to carry the line, you could run it up on that meridian perfectly free from any chance of troubles from icebergs. The ice in all those regions from Europe until you go round Cape Farewell and get on the west side of Greenland is merely floe ice. Floe ice in ordinary times should never exceed 7 feet in thickness. The law is half an inch, or 0·41 increase, for every day

during the winter months. So that you may arrive pretty nearly to what the floe ice should be. When you find what are termed heavy floes of 21, 22, 23, or 24 feet, that results from the breaking up of the floe, and one portion pressing above another until seven or eight layers become attached to each other, and the first thaw cements them into one mass.

4265. The floe ice I believe rises very little above the surface of the sea?—One-twelfth. I find that the immersion is $\frac{11}{12}$ths. I am taking the specific gravity that has been given us by different authorities, that would exactly make it, as nearly as possible. I see in the former voyages by Phipps that the floe ice got as high as $\frac{9}{10}$ths of immersion, some of the densest ice that they fell in with, the water being previously purged of air.

4266. Have you visited the coast of Iceland?—I have not.

4267. Along the east coast of Greenland you think there is no danger from anything but floe ice?—I think not from the account of Lieutenant Graah, the Danish navigator, he having passed along the eastern coast close inshore in an oomiak where he found the bergs calving off to sea. The water is so deep that there is no chance of bergs troubling the ground, and consequently any cable if it were carried up in a ravine in the fiords on that side would be quite safe, because the berg would strike the ground on either side of the deep gulley without troubling the cable in deep water.

4268. The bergs come from some point further to the north, do they not?—The bergs come from both sides of Greenland; they break off from the shore. They result from a thaw turning the water over the ice that happens to be formed upon the face of the land until at last the weight becomes so great, added to the sea underneath acting as a wedge, that down it comes. In fact the concussion of a gun being fired would bring it down. The icebergs are fresh-water ice generally; and the ice that is formed nearest the land should be of much greater density than that which is on the surface, because there is a pressure of some thousands of tons upon it; and we know that ice is compressible, because in the case of beer bottled in champagne bottles it throws the cork out of the bottle itself—the ice compresses itself, and instead of bursting the bottle, as it does with a shoulder bottle, it only forces the cork out and closes the bottle. I had a very thick glass cylinder belonging to a magnetic apparatus, and I stopped one end and filled it with water at 60 degrees. I then froze it under a temperature of 52 degrees below zero, and I found it only elongated half an inch, so that there were $\frac{34}{35}$ths one may say.

4269. Have you had any experience in the interior of Greenland?—No, I have not. I have been up the fiords for some short distance. I went in between Disco and the main. I passed through that channel for some distance.

4270. Have you formed any opinion as to what point of the east coast of Greenland you would carry the cable to?—I should propose carrying it as far south as I could into the first fiord that opens.

4271. And then you would carry it overland?—Yes, I would carry it overland, because the land about there, according to the description of several hunters who have been there, is low, and there are numbers of reindeer about; they will not live on the high land, they would have to go to the grass and they burrow like sheep or like bears and seek their food on the ground. They are reported to remain under the snow all the winter, and the hunters know where to find them by the air-holes.

4272. Is the climate such that you could maintain a line all the winter do you think?—The great point would be that you would have to employ a pretty large force on shore to cut down to the earth, and as soon as they got sufficiently low to get beneath the ordinary frost of each successive winter I consider that the cable would be perfectly safe.

4273. You would lay a subterranean line?—At least so low in the earth as I could get so as to prevent a thaw bringing it actually into contact with the surface during the summer. There are no moving glaciers mentioned on the southern point; the glaciers to be avoided are about two degrees northerly from the Cape. My meaning here applies to a subject not properly explained. The earth is generally frozen to the depth of six feet and more. If a proper trench be opened in spring, and the water resulting from thaws flowed in it during summer, it would cut a good channel. Any cable placed there and properly covered with the detritus would be perfectly secure, and *no surface action could reach it.* It would remain for ever unharmed; but this should be of peculiar construction and proper structures, or cavities cut into the rock, prepared for its security, as well as for easy access at the termini.

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4274. Do they go out to the east and west?—Yes; as far as I can understand, from Graah's work.

4275. In passing through Greenland you would not anticipate much difficulty?—I think not, but that is only a mere matter of opinion. I have read the account of Graah very attentively, and I see that he had no difficulty in crossing over the land there, and therefore I consider that people with better means and a good supply of food would find no difficulty.

4276. From Greenland would you go to Julianshaab?—To one of those southern fiords.

4277. And then across to Labrador?—Yes. There is a point that seems to have been overlooked hitherto, it is reported that a bank runs between Labrador and the coast of Greenland; there is a ridge somewhere which is well known to whalers to exist; a spur from Labrador which reaches across towards Greenland and turns the ice from the Labrador coast off towards Cape Farewell. The currents out of Hudson's Strait, Davis's Strait, and Cumberland Sound must tend to drive the ice out to the eastward; it meets with the eddy of the gulf stream which deflects it to America and the Banks of Newfoundland about St. John's, but the space about the mouth of the Straits of Belleisle and around there is generally free from ice all the year round. I have been since informed that this bank extends north and south off the coast of Labrador, and the cross bank has never been satisfactorily proved. If this be the case, the tail of this meridional reef, where it affords 200 fathoms mud, would prove the best point from which to start the connection to Greenland, assuming the spaces within to be protected by the bergs which are said to ground there; I think, after mature consideration, that all the lines should be run *independently from the western to* the eastern stations.

4278. Along the coast of Labrador?—I do not know how far up, but I should think it would be almost up to the mouth of Hudson's Straits. I do not think there would be any ice from that region which would press on the coast of Labrador, in proof of which none of the whale ships that have been beset or destroyed by the ice have been deposited upon that shore; therefore I suspect that the whole of that coast is kept free by the many inlets and currents on the shore. The currents set up upon the western side of Davis Strait and down southerly upon the other and you are obliged to work up on the east side till you reach Cape York, and then cross over and come down the west side; consequently the current which is running down the western side tends to sweep out south-easterly across towards Cape Farewell. The water about the north-east angle of Labrador is so very deep that there is no chance of any iceberg grounding there.

4279. And the ridge of which you have spoken would prevent ice bergs coming out?—It would tend to intercept the current and divert it easterly.

4280. It would intercept any icebergs which were sunk below in a great depth of water?—If they arrive at this ridge which is supposed to run across, undoubtedly they would ground upon it and be driven off in the direction of the current.

4281. If the cable were laid south of this ridge, in

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your opinion, it would be protected from icebergs?—Yes; my idea of laying a cable is to seek a depth at which no icebergs would touch it, and just to keep on the deeper verge of that depth, working along it until you come to Newfoundland itself. It should be attached to some vessel moored off until you have secured your communication, and then run across to Greenland. The heavy ice never presses upon Cape Farewell, and never has been known to touch the southern part of that land at all. My view in taking this route is not distinctly connecting it with any particular position in Labrador, but with Newfoundland at the Strait of Belle Isle, *if found convenient.*

4282. The ice is always carried round it?—Yes.

4283. And does not touch the east side of it?—No, I believe not. With regard to Newfoundland, I have counted in the month of May as many as 80 icebergs at a time on the banks of Newfoundland; and during the period I was employed there the only berg I saw grounded off the shores of Newfoundland was in 16 fathoms. The ice is so very nicely poised that if it was passing along the bottom I do not think that there is much chance of its raking the bottom; it would quietly roll over as a roller would do, and impress the cable into the ground instead of damaging it; it would not rut the ground up. The moment it touches the bottom you can observe it turn it over directly as easily as possible from the very nature of its construction. The temperature of the sea beneath is always about 36 degrees to 38 degrees, consequently it must be thawing; and as the under part of the ice is always in a thawing state there would be no edges to cut.

4284. Except at the moment of its turning round?—No; it would turn over, only requiring very little assistance to change its natural position of rest. I allude specially to bergs rounded, not field ice. The difference between berg and floe ice is very remarkable, and I think some people have drawn their conclusions from the aspect presented by upturned floe. The berg dissolves smoothly, and when it revolves presents a glossy smooth globular-shaped surface. But the case is totally different with floe ice. There the warm air globules rising from below bore into and honeycomb the under surface; indeed these air holes sometimes nearly pass through the floe, making *smooth holes like an auger* or pholas, varying from 1 to 1½ inches in diameter. Masses of such ice upturned are found to be very difficult to walk over, and offer a troublesome obstacle to sledge travellers. I think the rutting traces of grounded ice *forced up by external pressure* has tended to form the opinion that bergs tear up the ground they pass over.

4285. Your opinion is that it would be very feasible to lay and maintain a cable along the coast between England and America by the route of Iceland, Labrador, and Greenland?—Yes; I can tell you a little more about the bottom of the sea on the west side of Greenland, because I dredged along this coast working up to Disco from the southward, and I found it all mud even in about 15 or 16 fathoms. There is a bank off Holsteinberg, which is composed of fine sand, where they catch great quantities of cod.

4286. Your idea would be, I assume, to carry it from there towards the Straits of Belleisle?—I would carry it for instance to the Shetlands. I believe the whole of these parts have been sounded by Captain Thomas; and I think the Faro Islands also. As soon as you have determined, by sounding, that there is no rock, the better way would be to run in that direction. On Iceland we have no knowledge at all of any soundings. There is a fishing bank off there where the French are said to fish.

4287. Off the south point of Iceland is not there a certain amount of volcanic bank, on which as late as 1830 a volcano was seen or smoke was seen issuing from the sea?—I do not know, but we had Graham's Island thrown up off the coast of Sicily, but it has gone down, and there is a *sand bank* instead of it. I think the same objection, if it be made, applies equally to all portions of the ocean depths, and more particularly to the Mediterranean. But how it is to be discovered, beyond absolute test perpendicularly above the spot, I am at a loss to comprehend.

4288. Would not that be rather an unsafe place to lay a cable, in case the island might come up again?—I do not know that; it would depend upon the temperature of the water around. If the temperature of the water around there is high, why the further off (in deep water) the cable is the better.

4289. From the south coast of Iceland to near Cape Farewell?—I would run it as nearly as possible in the parallel until it arrives near the land. I would then take it into some of the fiords across there. From Graah's account, there is no ice here during the early summer months. There is the floe ice, which is attached to the land, but it cannot be of any importance, because whenever a heavy breeze sets in, the effect of the sea working under the floe, detaches it and breaks it up. From none of those fiords being clearly laid down, there can be no route determined through the floe. About August it is free from ice.

4290. Still, the northern part of the east coast of Greenland is entirely locked up with ice, is not it?—It has not been satisfactorily examined. Here is Graah's Island, where he stopped; but then he had nothing but a skin boat, a thing that in the seaway twists and turns in every manner. You can hardly make use of it at all. I know that the oomiak is a very difficult thing to handle in a seaway. In some of the northern portions of Greenland Scoresby says that he saw grass land. In the month of June he had grass and very hot weather. On the other side of America, at Behring's Straits you have ice, and at Blossom shoals I observed ice aground in 20 fathoms; but that is the deepest water in which I ever saw ice aground on that side; that is in 120 feet water. I do not think that the experience of Graah in an open oomiak, a mere skin boat, can be assumed as a guide as to the ice to be dealt with on the east cost of Greenland. I am, however, inclined to think that it will be imperative to watch carefully for the period to s[ail] from Greenland, and connect with the vessel l[ying] off. Also that the question of connecting it *with Iceland* should be secured by some beac[on] shore, if it should happen that heavy westerly [gales] prevented running into port. These are m[atters] demanding the most scrupulous investigation, and of infinite interest to navigation should our ships be stationed in that neighbourhood.

4291. A question has been raised about carrying the telegraph round by Asia to America. Would there be any difficulty in carrying it by the Aleutian Islands from one continent to the other?—I should say yes, by the Aleutian Islands. It could be carried from Cape Prince of Wales to East Cape. It is only 30 miles across.

4292. The Aleutian Islands would not form a stepping stone?—No, I think not. I think you would find great difficulty. They were originally volcanic. Upon the Alaska Peninsular we saw two volcanoes in action. The preceding remarks apply with greater force here. The Aleutian Islands are volcanic, and offer ironbound coasts.

4293. Are many of these parts inhabited?—They are all inhabited.

4294. You think it would be better to go further north?—I think it would be better to go to Cape Prince of Wales. From Norton's Sound to Cape Prince of Wales there are no mountains of any importance that would interfere with it, and you might make a communication by the river all the way to Norton's Sound, and carry it through the lakes up to the Mackenzie. From the Mackenzie the river would run by Porcupine River all the way to the head of Norton Bay, and from Norton Bay across to Cape Prince of Wales. There is a very good harbour at Port Clarence, where the "Plover" wintered.

4295. Is not the Russian Government at the present time constructing a telegraph as far as the Amoor?—I do not know, but if they run it to the Amoor they can very easily run it on to Kamtschatka.

4296. (*Professor Wheatstone.*) Is the country north of Kamtschatka inhabited?—It is inhabited all the way throughout the whole of the north coast of Siberia.

4297. (*Chairman.*) Are there no currents between Labrador and Greenland which would be injurious to laying a cable?—There are no ocean currents, I think, that would trouble any cable in any part of that region that I know of.

4298. The current at the bottom of the sea is not very strong, I suppose, in any case?—It decreases till you get to the bottom. In several cases it runs with the same force down to 1,000 fathoms. Off Cape de Verd I tried by five different depths of line, 100, 200, 400, 600, and 1,000 fathoms. I put them all over together from different boats, and watched them for two hours. The ship was drifted at the rate of five miles an hour past Cape de Verd. The whole of the barêças, which occupied the space of this room, never separated. In 1,200 fathoms off the Gallapagos, on the Equator, in the Pacific, I had the line up and down for at least half an hour. The vessel drifted very rapidly past the island, but there was no deviation from the perpendicular.

4299. As if the whole section was moving together?—Yes.

4300. It would not be like the case of a river, where the sands are confined by banks; there is no friction comparatively in the sea?—No.

4301. Have you sounded in very great depths?—4,600 fathoms.

4302. What process of sounding did you adopt?—I had about 40,000 yards of solid whipcord made for the purpose at Preston, capable of supporting a hundredweight, and I used weights of 28 pounds.

4303. Did you recover your weight, or leave it at the bottom?—I lost the line. It was not worth while to recover it.

4304. You pull it taut until the line breaks?—Yes.

4305. And that gives you the depth?—Yes, when it ceases to take the slack.

4306. Do you think there is any uncertainty in these deep sea soundings?—Very great. There may be two or three different under currents which you know nothing at all about. Even with 4,000 fathoms you may draw the line with your finger, but you cannot tell that it really is at the bottom. The last 100 fathoms goes out about a yard a minute. There may be an under current sweeping it away, and you think it is descending. But the mode that I adopted to obtain those soundings enabled me to determine that the sounding up to 2,000 fathoms was in a vertical line. The line was on a metal reel and it passed over another three feet reel with a trochiameter in its centre, so that I could take the velocity of the line until it stopped. Of course I knew it was descending, but the moment it began to travel, only at the rate of about three feet in a minute, I began to pull up again. In some cases I had to pull up 20 or 30 fathoms without breaking; but all the lines that go that depth into the water become unlaid and useless, and therefore it is not worth while recovering them.

4307. What does that arise from?—From the pressure, I suppose.

4308. In paying out?—No; the line kinks and doubles back upon itself and becomes very long jawed, so much so that very well-laid whip cord (I suppose I recovered about 300 or 400 fathoms), was utterly useless, all unlaid, entirely twisted out. That of course rendered the hauling in quite out of the question as to descent. This untwisting *is far below the surface.*

4309. Is that from getting a sort of revolution in passing through the water, the friction of the water causing it to revolve and untwist?—No, I do not think it is. Almost all new fishing lines that are used in deep water are the same; even a new line of 60 or 70 fathoms used for cod fishing you have to dry and twist again; you have a great deal of difficulty in taking care of the line.

4310. Do you bring up the bottom from great depths?—Yes. *Captain Sir E. Belcher, C.B.* 19 May 1860.

4311. Is that done by a second operation?—I did it when I sounded, of course bringing the instrument up again, and that brought the water up and the temperature from that depth. It was a separate machine. I used a tapered line for that purpose beginning at about half an inch, and the upper end of the line was 3½ inches. We were obliged to have such a line, otherwise the strain of hauling it in would break the lower line; but with a wire, that would not be necessary, because the power of the wire would be quite equal to a rope of that size. The Americans detach a ball which slips off, and they recover their line. Sir James Ross spoke about the Spitzbergen currents. Those currents do not seem to run at a greater velocity than about four m e ilsa day, which is nothing. I have some extracts from Parry's voyage, from which it appears that at 400 fathoms which was the greatest depth in 82° 17½′, and a daily drift of 3·375 in 82° 45′ he had no bottom at 500 fathoms, and he estimates his drift at four miles a day, that drift preventing him from nearing the North Pole, which he was then endeavouring to reach. Lieutenant Foster reports that "during their "absence after the middle of July no ice had entered "the northern bay of Spitzbergen, and that not a piece "had been seen in the offing for some weeks past, even "after hard northerly and westerly gales."* Therefore I do not think there is much to be feared from the Spitzbergen ice; that evidently does not come down to the southward.

4312. You think there is very little to be feared from ice between England and Greenland?—During the whole passage from the Orkneys to Cape Farewell I did not meet with a single berg until we got within 300 miles of Cape Farewell, and that was only a little bit of ice, it was no berg.

4313. Between Greenland and Labrador the bank which you mentioned would protect the cable from bergs?—The bank that runs round Cape Farewell and runs up that coast would protect the cable completely, because the evidence given by the Danes on that coast is, that no icebergs ever ground on the west coast of Greenland. It is off shore that they can see it pass, but then it is probably in 1,500 or 2,000 fathoms.

4314. What was the greatest depth of sounding that you took between Labrador and Greenland?—I never obtained any deeper sounding than 60 fathoms.

4315. That was further to the north I suppose?—It was along the coast southerly and abreast of Holsteinberg.

4316. Have you been by Hamilton's Inlet at all?—No; I do not know anything of it. The greatest measured height of any iceberg that I ever saw was 150 feet as a pinnacle, and when it canted, or turned bottom upwards, it was not above 80 feet.

4317. Sir James Ross suggested going to Rockall instead of Iceland, and from there to Cape Farewell, in order to avoid the danger of volcanic action at the south coast of Iceland and the currents at the Faro Islands; I understand you to say that the currents do not extend beyond the mere shores at the Faro Islands?—These are points that we want determined; it is a mere matter of opinion; assertions are made simply that those currents exist; but it does not follow because you have those constant eddies and apparent whirlpools that there should be a current, because I have tried the currents in similar whirlpools and have found none.

4318. There was no current moving bodily, you mean?—No. In the Mediterranean, over the Skerki rocks, between Africa and Maritimo, there are tre-

* It has generally been remarked, that where even slight current prevails "Ice will go to windward in the teeth of a "heavy gale," and looking to the power exerted on isolated masses, with 11 parts pressed on by current against $\frac{1}{32}$ to the breeze, it is easily accounted for.—E.B.

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meudous currents or eddies that you really fancy you must be in some danger of a whirlpool, or something of the sort. I have sounded and found not the slightest current. The whole of the space between Tunis and Maritimo was closely surveyed by myself from shore to shore, and I could generally get bottom. I knew precisely when I got the lead to the bottom what was going on at the surface. Very frequently in the Bahamas and about there, out of the gulf stream, you have these extraordinary eddies, without any apparent cause at all; and the same at Bermuda, where there is no tide, you have these eddies. I always attributed them to fish below, perhaps whales, or something of that sort.

4319. Are those eddies which arise for a short time and then cease?—Yes; in the track of a whale, it may be a mile or a mile and a half from you you come suddenly into its track, and you find a sort of whirlpool, and then you will see the whale at a distance, and know positively what the whirlpool arises from. In the Pacific that is very frequent indeed. I want to know, if Sir James Ross maintains that the current exists through those islands, why it ceases immediately after leaving the islands, and why we are not driven out of our course. Between England and Cape Farewell we steer a course calculating upon some slight currents, because we know very well the general tendency of the stream, and that the head of the gulf stream sets in a precise direction. We never find ourselves very much out of our reckoning, and, therefore, I am not disposed to accede to the existence of that current until it is proved.

4320. Assuming a line of cable to be laid down, do you imagine that it could be all laid in several sections at the same time of the year, or would you select a different period of the year for laying each section?—I think it could be all laid at the same period of the year, but I think each portion should be laid separately.

4321. I mean whether the season of the year which was favourable for laying one section would be the season of the year that would be favourable for laying another section; for example, whether on the east coast of Greenland the same period of the year would be as favourable a one as it would be for laying it on the west coast of Greenland?—That is the only point that I feel any doubt about, namely, the period at which you can get it to or from the east coast of Greenland—that we are none of us aware of; neither Grïah nor any other person who has been there *has solved that* question. Therefore until *that* is determined I should not be disposed to give an opinion, but my conviction is that between August and October anything that can be done should be done in those months, but you may work upon all the other points from May until October. The ordinary period of receiving our instructions for northern work from the Admiralty is to quit London in March. I see that Parry in the month of April was at Spitzbergen, consequently if he could go up there and *work* in the month of April I should say from the first of April to the last day of October anything may be done upon that line. The first 10 days of March are always, you may say, 10 of the coldest days of the year up there. After that the summer comes suddenly upon you, and the first of May throughout America is well known to those who have been in Canada and about there; the snow clears off, and you get the May flowers from under the snow. Therefore that is the first of the summer season. All the different saxifrages and other plants on the southern coast of Greenland are in full flower at that date.

4322. Do you imagine that if stations were established in Greenland and Labrador the present colonists would be available for the purpose of keeping up those stations during the winter, or that we should have to send out colonists?—I should think on the Labrador coast you would have about as highly-civilized a set of natives as you will find in almost any other part of the world.

4323. And in Greenland?—In Greenland they are very fair.

4324. Of course, they are Danish?—Yes, a mixed race. They have schools, and they are very well clothed, and are perfectly under command; there is not the slightest attempt at opposition. They are thoroughly under the Danish government, and if the Danish government gave an order, no, matter what it was, however impossible apparently to us English, they would have it done; they are very strict with them.

4325. If any expedition were to be sent out, it should start very soon?—Without a moment's delay.

4326. I presume you would select a steamer, such as surveyed the line between Gibraltar and England?—Yes. The sounding part of the business, if the Admiralty pleased so to order it, could leave England in 48 hours. I have not the slightest doubt that there are many surveying officers that are available; for instance, Captain Robinson.

4327. And Commander Dayman?—I should think he would be a very good man to employ in the deep sea part, because the two vessels that go should have quite separate duties. One should be instructed to run in the line of direct sounding upon the edge of the deepest water, and the other vessel should endeavour to work by traverses along the shore, so as to determine how the bank lay; that is to say, to fringe it. I think in doing that, the officer who is selected should be a practical surveyor, so as to avail himself of all the advantages which such a voyage would confer. The Royal Society might send its mission to be landed upon the different islands, and to be picked up as the ships came back; because there are a great many magnetic observations which would be important to be taken at the same time. Upon the subject of eddies I would remark, that, to my mind, these apparent eddies or whirlpools result from banks of which we do not yet know; and those banks may be very important to this country as fishing banks near our own coast, instead of travelling over to Newfoundland. I have always found that those eddies arise in the Mediterranean either from gulleys or shoals; and, in fact, where you would imagine that they arise from fish, you come immediately to the conclusion that there is a bank upon which the fish live. There is no part of the world where that is so perfectly evident as off the Lagullas bank. Off the Lagullas bank you have those eddies, but you have no great drift; and in three or four different times of crossing I have dredged over a great part of the Lagullas bank, and have taken great quantities of fish; and in the midst of those *eddies* we had no tide whatever. Again, on the coast of Africa, off the Bijuga shoals, there are more of those eddies than in any part of the coast of Africa; but there are no strong currents, although there are regular tides. In fact where the dredge *can work safely* in 70 fathoms, it is clear that no great current can exist, otherwise it would instantly be destroyed.

4328. Have you considered the plan for laying a telegraph line further to the south, by the Cape de Verde islands, so as to reach America in that way?—Not by the Cape de Verdes. I had an idea about running a cable to that shoal, which exists to the northward of the Azores. That bank has about 35 fathoms of water on it.

4329. Is there no danger to be apprehended from volcanoes*?—I do not know anything about that. There are volcanoes upon the Azores, but on this bank, where there is about 30 fathoms, if it exists, I see no difficulty in mooring a floating vessel without a mast. I have anchored a beacon in 100 and 150

* The question of submarine volcanoes will apply to any part of the sea, even to the present line laid. Before the heat resulting reached the surface, it must be abstracted by the water through which it passes.

fathoms, on the survey along the coast of Africa, which was carried on at intervals during three years and a half, and I seldom met with any accident, such as breaking adrift. I am perfectly satisfied, if a vessel were moored there, she might be made the medium of working between the two shores.

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4330. (*Chairman.*) You are the telegraphic engineer and electrician employed by Messrs. Silver, I believe?—I am manufacturing superintendent at Messrs. Silver's.

4331. You have had considerable experience in india-rubber, have you not?—Yes. Since I have been in Messrs. Silver's employ, I commenced with it from the first, besides having had information upon the subject years ago. I think I am about the only man that ever put down india-rubber wires under Mr. Latimer Clarke. The greatest length is about two miles in London. I put that down at the Strand office eight years ago, and it has remained in excellent preservation.

4332. Is that underground wire?—No; it is carried from the battery-room upstairs and about the offices. After it had been laying about for four years nearly, we put it up eight years ago, just when Mr. Latimer Clarke came into the service of the Telegraph Company.

4333. Were those wires covered with Messrs. Silver's masticated india-rubber?—Yes, with masticated india-rubber, because masticated rubber is the only rubber that insulates. It has been tried by experience by a great many that the pure gum will not insulate.

4334. Not pure bottle india-rubber?—Not the mass. You may take one piece, but you cannot make it mass upon a length of wire. That is a process that we have inquired very much into.

4335. How do you apply the india-rubber to the wire?—We apply it spirally in all cases.

4336. When it is cut in two places, and joined together again, immediately *after*, it adheres as closely as if it had not been cut, I believe?—Just so.

4337. I have seen wire covered in considerable lengths longitudinally, in which there seems to be no flaw at all?—There are many difficulties in that process that there are not in a spiral covering.

4338. What would the difficulties be in that process that there are not in a spiral covering?—In the first place, we should not be able to get the bottle-rubber to a sufficient length to do it, except at an enormous expense; there would be no means of procuring enough gum, in case we wished to cover the wire either spirally or longitudinally. The gum would not unite perfectly, and you could not get it to mass together.

4339. Do you find by putting it on spirally that you are able to secure the gum adhering thoroughly? Is it masticated at first?—Yes.

4340. What is the process of mastication?—Mastication is merely taking the small quantities of bottle gum and bringing it into a large block, and then when it is brought into a large block, it is cut out into nice thin slices—we will say 6 feet long and 15 inches bread. We must have it in the mass before we can work it with facility or with economy. To cut it out of the gum the expense would be very considerable; in fact, it would amount to something impracticable.

4341. Have you exposed the masticated india-rubber to pressure in water?—Yes.

4342. Have not you found that it imbibes water rather largely?—In our experiments not so. I have experimented perhaps more than any other man upon the subject. We had a pump of our own latterly, and I have gone into the inquiry very minutely. I could furnish you with the result of the experiments taken as they occurred. I have found no difference in the india-rubber after 38 or 48 hours, but rather an improvement under pressure than otherwise.

4343. (*Professor Wheatstone.*) Have you weighed the specimens to see whether they have acquired any weight of water?—No; I merely tested the rubber electrically.

4344. (*Chairman.*) I am speaking with regard to the absorption of water by the india-rubber?—I have not tested it for that purpose.

4345. Do you find that the surface of the masticated india-rubber alters by being exposed to water?—It always returns to its native colour when it has been long under water. I have had india-rubber under water for a year, and it is white, but the insulation remains exactly the same. I have had wire buried in the earth that has been there for years. I took it up in November to look at it, and it was white, and, as regards its insulation, was not in any way interfered with. It always returns, under water, to its primitive colour, or very nearly so.

4346. You say that you have tested india-rubber for insulation by hydraulic pressure?—Yes.

4347. To what pressure have you exposed india-rubber for that purpose?—9,000 lbs. is the highest.

4348. In what machine?—A machine made by a man named Parkins. It is rather a *multum in parvo*. It acts upon springs. I believe it is in every way correct, but the valve presses up a spring instead of a weight.

4349. (*Professor Wheatstone.*) What length of wire do you subject to pressure?—20 feet; that is the length of our pump.

4350. (*Chairman.*) Have you found any alteration in the insulation under pressure?—No, we have gone through the experiments with the determination to find the fault if it existed, and not to lose sight of it. Some late experiments of the Government have again caused us to inquire into it, whether they were right or wrong. And we are obliged to come to the conclusion that the Government experiments are wrong.

4351. To what do you attribute the failure in the Government experiments?—To mechanical injury; we do not use hard ends in Messrs. Silver's process; directly I got the pressure up to 400 lbs. the strength at the neck became lost, and the insulation was destroyed. I have gone through a variety of experiments to get the best end, and now I have arrived at a kind of packing that is perfect.

4352. How do you pack the ends?—We use ebonite ends.

4353. Is that vulcanite?—It *is* what we term ebonite; it is similar to vulcanite. Those pieces that I packed that had gone at the ends, I have taken them out after submitting them to pressure for 12 hours of 9,000 lbs., and the insulation was lost, and because I suspected where the insulation was gone I took them out and cut them off at the neck where they could have suffered from mechanical injury. The wire I put in with better ends tested perfectly for many hours. I hardly think it improves under pressure. It is so excellent I may tell Professor Wheatstone that in five minutes with a Peltier 400 elements there was no alteration in its electrical condition. It lost nothing, and the gold leaf always showed a similar condition.

4354. What thickness was that?—A quarter of an inch of 16 wire.

4355. (*Professor Wheatstone.*) A quarter of an inch entire diameter in one covering?—Three or four coverings; we always put on three; our small wires are insulated equally to the large ones.

4356. Do you mean to say that the whole of the core was a quarter of an inch in diameter?—No, the rubber.

4357. Have you formed any opinion as to the causes of the apparent liquefaction of india-rubber near the

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copper wire?—We only have one reason for supposing how it arises; our efforts have principally been directed to being preventive. In manufacturing it, it was resolved to prevent this occurrence, and in the wire at Camden Town, which has been there for six months, as it would show by cutting it in any part you please, there is no liquefaction there at all.

4358. (*Chairman.*) What is done with the india-rubber for that purpose?—We put an elastic gum over it with a compound of mica.

4359. You put some division between the wire and the india-rubber?—Exactly, for submarine purposes.

4360. (*Professor Wheatstone.*) Do you think that you have completely obviated that tendency to liquefaction?—Yes.

4361. (*Chairman.*) Do you think that that process is necessary for submarine purposes?—No; under water rubber never decomposes against the wire. I will give you an instance of it, and a very peculiar one. We laid a cable along Mr. Scott Russell's yard and across to the Great Eastern. Some barges or something of that kind went along the wharf and broke the cable off close about high water about midway between high tide. I went down to repair it with a man. I took the wall end and picked it up, and the man who was with me picked up the side in the water, and I found mine was liquefied. I said, "Mine is "liquefied," and the man said, "Mine is not." That which was under the water was perfectly good, while that which was along the wall had liquefied. This was before any preparation had been used. It was a very remarkable instance, and it could not have failed striking any person. What we believe the liquefaction arose from was this: when the sheets of the gum are cut out of the block they are piled up in single layers, and to prevent their sticking some manufacturers use a weak solution of potash over the surface. If something of that sort is not done when you cut the sheets they run together in one mass; and to avoid that a solution of potash is used. Other manufacturers use a thin lather of soap, and it appeared to me in the first instance that the liquefaction arose from the soap, because I always found that the wire was considerably oxidized whenever the liquefaction occurred. You will always find, when you see liquefaction, a large surface of oxide upon the wire. This oxide I presumed occurred from the fatty matter in the soap being in some degree left free and acting upon the copper. These are the facts in relation to the india-rubber. If we take a mass of bitumen and mix it with tallow it does not affect the rubber, but if we separate them, of course the fat goes to the rubber and dissolves it. We washed all the sheets with the soap till I became aware of this; I did not know that the soap was any particular injury, and it was never washed off. In our own cutting now we use no soap in all the stuff we use, and every sheet is washed independently of what we use, no matter what it may be, and I think under our treatment in future there will be no liquefaction against the copper. But under water there is not a semblance of it. I could show you a wire that has been under water for a year and the rubber is as perfect as the day it was put on.

4362. (*Chairman.*) Can you give any reason for this fact?—Just the one I have now stated in the sheets of rubber.

4363. I mean that the water prevents the liquefaction of the rubber?—No, that is a very singular thing; you may say that it is the heat, but we have had wire in water, we have a water cure for melting the sulphur which we use, and I have put the rubber into that.

4364. At what heat?—290°, the heat was on for six weeks except at night.

4365. And no liquefaction took place?—Not the slightest. I was very much surprised at that. Even melted bitumen at 408° or 420° does not affect the rubber at all. I am speaking of Fahrenheit; there is scarcely any heat affects it; under water nothing seems to affect it. I have had wire in water at 120° and 130° for six weeks under test; in those bundles of wire we never found any difference; it does not seem to affect them at all either under water, hot or cold, in cold water I never saw any tendency in the copper wire to put itself out of the centre, and I think myself if the heat was raised to any possible amount that there would be no tendency for the copper wire to get out of the centre. All our wire is cured, as I dare say you are aware, in water at boiling point, and if there was a tendency in the rubber to yield to the pressure of the wire it would then occur, but I think you may always find upon examination that the wire is sure to be in the centre of the rubber, that continues under all conditions, if you kink and twist it about anyhow, and then cut the kink through you will still find the wire exactly in the centre.

4366. Have you seen Hooper's preparations of india-rubber?—I saw some small pieces at Mr. Latimer Clark's.

4367. Were those recent specimens?—I saw them some months ago; they were vulcanised.

4368. Have you formed any opinion upon those preparations?—It is considered impracticable to make joints, if it could be done at a rate anything like cheap it would be a very valuable thing no doubt.

4369. To vulcanise the outer parts of the india-rubber?—Yes, whether it would insulate as well as the masticated gum I do not know. I think nothing will insulate superior or equal to it, because I think it must be understood by the scientific world, that before Messrs. Silver's wire came under examination no man had a right to think that there could be a wire insulated.

4370. By india-rubber?—By any means, it is the highest insulation ever obtained; there never was a wire insulated till Messrs. Silvers' came into use, not to the same extent. I have been employed on gutta-percha by the Telegraph Company for eight years. I have had pretty good experience in gutta-percha, and I know that india-rubber treated in the old way bears to this day a character immensely superior to gutta-percha. In the tunnels at Liverpool, where I was engaged for some time by the Telegraph Company, the Waterloo tunnel and the Wapping tunnel there are wires laid down. In the Wapping tunnel there are several india-rubber wires, and along the other tunnel there are gutta-percha wires; the gutta-percha wires were put up five years ago, and the india-rubber wires seven years ago, and the india-rubber is really good to this day, but the gutta-percha all the way it goes along the wall in many parts has gone bare, even in the darkest place, and on that account india-rubber is more durable. We have also the testimony of Mr. Walker, who has some india-rubber now just in the same condition that he received it; he has had it in a tunnel for one year. I may observe it is not of much consequence what size the india-rubber is used, if we cover it up to seven gauge, which is very small, the insulation is very nearly equal to what it is when it is covered up to two gauge, and there is no doubt that I could make just the same number of layers as with gutta-percha by taking thinner coats. The insulating property of india-rubber is admitted on all hands, and its less induction is a well-known fact. Those are facts which I saw at once on entering on the manufacture. It was not generally known, and no one would receive it as correct; many gentlemen that I spoke to about the less induction of india-rubber had no idea of such a thing, and they could not think it possible. There was a notion existing amongst electricians a year ago that the greater the insulation the greater would be the induction. I do not mean to say that the highest order of men held the same opinion; but those who were practically engaged in telegraphy at that day thought that the greater the insulation the greater must be the induction, whereas in india-rubber there is less induction.

4371. (*Professor Wheatstone.*) What is the greatest length of time that you have subjected india-rubber

to hydraulic pressure?—My longest time is forty-eight hours.

4372. What is the greatest length of wire that you have ever used as a submarine telegraph covered with india-rubber?—The longest length now is three miles, laid down in the Red Sea. I think it is the shore ends landing on the coasts of India at Kurrachee.

4373. Have you had any report of the working of that portion of the wire?—Messrs. Silver have; it is laid down in perfect order.

4374. (*Chairman.*) Have you seen any experiments made upon Wray's compound?—It happens that we manufactured it first, consequently I was obliged to have something to do with it.

4375. What do you think of Wray's compound?—It is a very beautiful thing.

4376. Is it easy to apply?—We did not go into the manufacture so much as others have since done. I should say there are difficulties; it would be plastic in the way they put it on the wire.

4377. Plastic like gutta-percha?—Yes, and will not cool so rapidly as gutta-percha.

4378. Do you think there would be a difficulty in putting on several coats of it rapidly?—Yes, I should think so; we only mixed the compound and made it as a specimen; it tested very well; it is a compound, as I dare say you are aware, of rubber, silica, and shellac.

4379. Is gutta-percha mixed with it?—No.

4380. Are not there some specimens of Wray's compound that are mixed with gutta-percha, and some that are not?—What we made was merely rubber, silica, and shellac; it seems to be found that its insulation is just that much less than india-rubber.

4381. You mean that the less india-rubber there is and the greater the quantity of other material, so is the insulation diminished as compared with using pure india-rubber?—Just so; it appears to me to be so from the tests I have made. The insulation with rubber is superior, and the induction appears to me to be less from the small specimens I have had, and I think the greater induction and the less insulation of Wray's compound is due to the small quantity of india-rubber. Another thing in Wray's compound is this, if any heat should occur there is no doubt the wire would have a tendency to push itself to the surface. If you take about a yard of Wray's compound and submit it to hot water it will shrink three or four inches; if it were submitted to heat in the hold of a ship, it is possible in my opinion, unless some process were adopted, that it would have a tendency to pull itself away at some part.

4382. More than gutta-percha would have?—It is softer than gutta-percha in warm water. What would make Wray's compound shrink would make the wire come out of gutta-percha entirely.

4383. (*Professor Wheatstone.*) How would india-rubber behave under the same circumstances?—No effect would be produced.

4384. What is the greatest degree of heat to which you have subjected india-rubber without affecting its insulation?—408° or 420° that was in hot bitumen.

4385. And you found the insulation of india-rubber perfect at that high temperature?—It seemed to make no difference.

4386. (*Chairman.*) Did not it diminish in insulation under that temperature?—I did not test it under heat. I merely put it in and took it out again to see the amount of injury the rubber received; then I let it cool. Of course india-rubber would, like all other gums under great heat, be affected in its insulation, but not up to 90°, not appreciably; when it gets to 130° then it becomes appreciable; it will lose nothing in a minute under 69° of temperature; at 123° by a Pelletier it will lose 20° in a minute; it would be about as good at that high temperature as gutta-percha is at an ordinary temperature. There is a great benefit which I dare say you have seen in india-rubber, that in such a heat as 100° in a ship's hold the wire will never have any tendency to work itself out of the centre. It may be a little more difficult at first to work india-rubber, though we do not feel it, but after the wire is made there would be less difficulty with it.

4387. Have you any other statement to make?—I think I may point to the india-rubber wires used at the Strand offices and in the tunnels as proving the great durability of india-rubber; we put it up eight years ago, and we have several times supplied the same tubes with gutta-percha, and the gutta-percha is always decayed, where the gutta percha comes down to the battery-room into the corner which is warm from the gas employed; it has not taken a great deal of effect upon the india-rubber, but the gutta-percha of the same size has crumbled away like chips.

4388. From dryness?—Yes, from dryness. Under ground or under water I think that india-rubber would be found to last for a life-time. All that we have buried and taken up after being down six or eight months presents precisely the same appearance as when it was put down. When it is covered with a thin tape or a film of shellac it resists the sun's rays almost entirely; of course it will not stand the sun any more than gutta-percha. The difference in that respect between india-rubber and gutta-percha is that one liquifies and the other dries up, if it should come to that. I have seen gutta-percha in the cellars of the Telegraph Company before being used at all crumbling in pieces like a broken tobacco pipe.

4389. Because it was kept too dry?—It was the method of the manufacture, I suspect; but gutta-percha has a tendency at all times to lose its adhesive properties, and india-rubber seems to collect it. I think that the liquefaction will be courted in some cases, instead of discountenanced. For example, wire that has not tested well, if I get the india-rubber to liquify, and I then test it, I find the insulation as good as can be desired: When I saw that at first I thought it would be a thing sought after, and I think so yet, a certain amount of it. I have seen old pieces of india-rubber that have liquified test superior to pieces of gutta-percha of a much larger size. This was a test trial brought on by some of the gutta-percha people to show their own material. It was not courted by us at all, and the piece of india-rubber which liquified tested perfectly.

4390. (*Professor Wheatstone.*) If india-rubber becomes liquified, do you think the wire would preserve its centre?—There would be the means of making it liquify to a certain extent, so that the centre would not be lost, but I could make a wire any time that would never lose the centre. I would just make the diameter of the wire one gauge greater, and it would be liquified one gauge more round it.

4391. We have seen some specimens, in which the liquefaction has extended so far as to appear upon the external surface?—I would propose to prevent its going beyond a certain extent.

4392. (*Chairman.*) Whatever distance it went, the wire would sink through that distance, would it not?—It would, but only the size of a sheet of paper. If you wrap a sheet of paper round the wire, that would be the size. I do not say it will ever be so; I do not point it out as a real benefit, but it appears to me, at the same time, that its insulating properties are so excellent, that it will some day be employed.

4393. (*Professor Wheatstone.*) What is the greatest length of telegraphic line laid down with india-rubber covered wire, and where?—The Isle of Wight possesses the longest quantity. I think there are 20 miles lying about there of india-rubber covered wire.

4394. That is only recently?—Yes. Mr. Walker is laying down wire of our manufacture for the Magnetic Company. He appears to possess great faith in it and Sir Charles Bright. They say such insulation they never saw for such a price.

4395. Where is that being laid?—On the South Eastern Railway; the Magnetic Company are laying down wires there.

Mr. J. Fuller. 19 May 1860.

4396. *Will* they be carried under ground the whole way from London to Dover?—No; they are overground wires, and our wires are for the tunnel portion underground in scantling.

DEAR SIR,

WITHOUT referring to the special questions which I have answered, it has occurred to me that other matters relating to icebergs, temperature of the sea at surface as well as various depths, may prove interesting. Further it is not improbable that the survey about to be undertaken may furnish us with more reliable data, than any we now possess as to the actual source from which the icebergs, supposed to come from Spitzbergen and other northern localities, *actually* result. I wish one of these bergs could be followed up to its dissolution with the accompanying data of temperatures, above and below water, as well as depths of grounding. I must confess I feel somewhat puzzled at the formations I have both *seen* and *read* of, after watching the effects of intense cold during two winters (ranging as low as 94° below the freezing point) without any thicker ice resulting, from congelation only, than 7½ feet.

And with regard to the action of bergs on the bottom and their floatation, notwithstanding experiment on small masses, we certainly witness instances where we find them *aground, totally at variance with calculation* as regards specific gravity. Two occurred on our last expedition. The first was off the port of Lievely (Disko), where the *Pioneer* lost her mizen mast by falling foul of one a-ground in 16 *fathoms*. If the calculated height above water was taken into consideration, that berg should have been immersed 800 feet, and yet it was a-ground in 96, and further, moved away in 24 hours!

The next case was at Uppernavik at our anchorage. The *Resolute* and *Pioneer* trusting to a berg a-ground in 10 fathoms, were driven off with it in a squall, clearly proving how easily it could be moved, the mass itself being enormous as compared with those vessels, and the power of the wind on them. Those bergs, indeed all I saw in Davis Straits, I believe derive their origin from the fiords of Greenland. We hear constantly of "the Spitzbergen ice," probably from its drifting from a northern latitude; but if we reason on the distance between it and the south point of Greenland, and the rate of drift, as determined by Sir Edward Parry, we find a course of about S.W. ½ W. and distance about 1,700 miles, which at the rate of four miles a day; *against prevailing westerly gales*, would take 425 days to accomplish! When, then, we may ask, would it reach the banks of Newfoundland? But from whence again is this current derived? We are told that it is "the deflected "stream from the Gulf of Florida." We will look presently to this stream as the *motor* and its temperature as affecting ice, as the gulf stream from the coast of Florida is imagined to produce the Spitzbergen current. I have taken some trouble to seek for data in confirmation of my view that, taking into consideration its strength, course, temperature, and ascertained drift of materials in it, we have no ground for attributing to it such a miraculous change as to admit of its losing its temperature so suddenly as to favour the transmission of the Spitzbergen ice.

Commencing at the Bermudas in lat. 32·20 N. I collect from the American survey in search of shoals, false Bermudas, &c., the following:—

Parallel of Bermuda 32·22° N., long. 58°,		t. sea	73°
	ditto „ 71°,	„	75°
On the meridian - 35·00° N., „ 65°,		„	75°
Again - - - 38·00° N., „ 70°,		„	77° to 79°.

This heated body of water flows to the E.N.E. until it passes the 50th degree of longitude in latitude 40°, and then preserves an east course.

The journal of the Grinnell Expedition affords us some further interesting data, and enables us to follow up the changes of temperature from the point where Commander Lee leaves off,—past Newfoundland up to the parallel of cape Farewell, as under:—

Latitude.	Longitude.			
40·0° -	67·30° W.	t. sea	58°	
42·0° -	61·30°	„	42°	
45·0° -	59·0°	„	38°	
Off Cape Race -	-	-	39°	
48·0° -	52·0°	„	36·3°	amongst ice-
53·0° -	51·0°	„	38·0°	bergs.
58·0° -	52·0°	„	41·0°	
60·0° -	53·0°	„	38·0°	

Here the temperatures rise on losing soundings and nearing Greenland.

The deep sea depths available are:—

1st.—5 June, 46·12° N.—Long. 55·7° W. 50 fathoms (rock) t. 37°; surface 42°

18 June, 63·4° N.—Long. 54·44° W.

surface	-	39°
10 fathoms		37·7°
20 „		37·0°
50 „		37·0°
100 „		37·3°
270 „		38·0° no bottom.

These observations go to prove that so early in the season as June, the medium in which the ice floated was an *active solvent*. This should prove a conclusive refutation of the sea in which ice floats being at, or near, 28°.

As we seem to be deficient in Arctic temperatures, I have extracted the following from the Antarctic voyage of Sir James Ross. I find on 3rd March 1842 equivalent to 3rd Sept., in the opposite region, being then in 67·28° S. and 180° the following temperatures recorded:—

At 600 fathoms		-	38°
450	„	-	37·5°
300	„	-	35·5°
150	„	-	34·2°
Surface	-	-	33°

And as sea water freezes at 28·5° we have consequently in that severe region about 4·5° of thawing temperature. This added to the action of the sun's rays above water would act powerfully in changing the equilibrium of the masses, and as I find Sir James remarks, after observing on the first,—"the accumulation of the heaviest masses of ice I ever "remember to have seen of a deep blue colour, and much "worn and rounded by the sea." I presume that the temperature of 33° to 34·2° down to 150 fathoms had operated, just as I have contemplated would be the case to the north, "in *rounding* and *smoothing* the bergs," so that they would only press the bottom instead of "*rutting*." He also observes sounding in 410 fathoms that, "the "leads sunk fully 2 feet into a soft green mud, of which a "considerable quantity still adhered to them."

But I think we may rationally compare either side of the Arctic seas with those of the Antarctic, and if this be permitted we shall find that the temperature of the sea, after we pass the 60th degree, has always a tendency not only to melt ice, but to extract from it its heavier component parts, and thus render it more buoyant and less likely to injure a submerged cable than ice recently broken off from the cold region where it was formed.

In illustration of this I will give several experiments I made on wines, beer, limejuice, and vinegar. They were all frozen *solid* under a temperature of—52°. They were exposed in gauze bags to thaw (at probably + 40° or more). All the *essences* dropped away *first*, leaving a *spongy snowy mass* behind; such I suspect is frequently the case with salt water frozen at 28·5°. Indeed I found, travelling with the sledges, that masses of upturned floe, exposed to the sun's rays, formed *icicles beneath*, and the ice became clear instead of a gloomy olive, and was fit to drink. It created, nevertheless, as *snow* did, *increased thirst*.

It is, I may say, almost an acknowledged idea, for *facts* we have not, that the ice "works its way" down the eastern side of Greenland, rounds Cape Farewell, creeps up the western coast, and turning about Cape York, travels down the west side of Davis Strait to seaward.

I am very sceptical as to the truth of this theory, as far at least as Newfoundland or Labrador ice is concerned. First, the presumed identity is questionable, and for the following reasons:—Spitzbergen and Polar ice fields are of salt water ice—not bergs—and drift wood is frequently noticed in it; none is traced in the ice we meet with in Davis Straits.

Next, in its passage through the water warmed by the gulf stream (as may be traced by the logs of the vessels sent to the Arctic seas in May and June, and not below the 40th deg. of lat.), this ice should be *rotten, disintegrating*, even up to Disko in June, (p. 4. 39° surface), and *dissoloing during its whole course* between May and September. Is this the fact?*

Next, if we *admit* this counter current to be caused by currents in action *up to* and *over* the great bank of Newfoundland, we should have two powers in operation to divert the ice from entering Davis Strait, or at least to cause a *divergence*, and send one portion down *direct* to *Newfoundland*, and thus intercept the navigation between the Orkneys and Greenland!

This, we know, is not borne out by any authority; but,

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* If this ice had to travel from May until September, as suggested, it must have been constantly subjected to temperatures *at least* 4 degrees above freezing; therefore, all that ice would be *rotten*.

leaving Arctic speculators to their cherished theory, let us be more practical in the well-known *facts* relating to the ice which visits Newfoundland.

First, that ice in its most *perfect unthawed* state, and in most cases *fresh water berg ice* (and from which H.M. ships have WATERED), comes *suddenly* along the eastern shores of Newfoundland in the month of *May*, long before it is asserted to transit the meridian of Cape Farewell in June! on its drift over at least 1,500 miles.

Now, we are assailed with the bugbear of the depths to which these bergs reach. I have proved them to be aground in 20 fathoms. I have witnessed them *forced off from terra firma to sea* under Mt. St. Elias, on the western coast of America, into *three fathoms* water. Every officer who has served on the Newfoundland station knows full well that at the period of "grand block," between 1st May and 1st June, icebergs *enter* the harbour of St. John's and *ground in three fathoms*. I remember one very large berg aground in Freshwater Bay outside the port, from which we obtained fresh water, and at which the boat's guns were fired to shake it. Its outer edge was in six fathoms, its summit about 40 feet above the sea level. *Mr. J. Fuller.* 19 May 1860.

As a cruiser on that bank during the American war, I can safely assert that after the month of July I never witnessed drift ice on *the southern limit* of the bank; indeed, the sudden transition of temperature between 40° and 60° N. (viz. from 38° to 58° = 20°) would at once point out sudden dissolution.

It is this "sudden dissolution" we should have properly authenticated, by the passages of some of our steamers on that station, watching a berg from its drift in the parallel of St. John's to its disintegration or dissolution at some determined southern parallel. These observations are all important to navigation, independent of the telegraphic question.

Believe me, yours faithfully,

EDWARD BELCHER,
Capt., R.N.

Captain D. Galton, R.E., &c., &c.

Adjourned.

Friday, 10th August 1860.

PRESENT:

Captain DOUGLAS GALTON.
Professor WHEATSTONE.
Mr. LATIMER CLARK.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

ROBERT STIRLING NEWALL, Esq., examined. *R. S. Newall, Esq.* 10 Aug. 1860.

4397. (*Chairman.*) I believe you have been connected with electric telegraphs for a very considerable time?—Yes, since 1848, when my attention was first called to the subject.

4398. You have laid down a very large number of electric telegraphs?—We have made or laid upwards of 10,000 miles.

4399. The last that you laid was the Red Sea line, was it not?—The line from Aden to Kurrachee, the Indian section.

4400. You wish, I believe, to offer some explanation with regard to some evidence which has been given to the Committee?—There are some parts of the evidence which I should like to put correctly before the Committee, with regard to the Bona cable between Sardinia and Algeria, upon which, I believe a great deal has been said. We undertook to lay that line in 1857; Mr. Brett gave us a specification, and also the amount that the company had devoted to pay for it, namely 50,000*l*. We were therefore limited somewhat as to the proportions of the cable. The insulated wire, however, is the same size as in the previous cable made by Messrs. Glass, Elliott, and Co. We used a copper strand of four wires instead of a single wire conductor. The distance across is 125 miles, and we proportioned the gutta-percha and copper to suit that distance. If we had had more money we should have made probably a thicker covering of gutta-percha; but as we had no funds supplied us we were obliged to run it as close as possible.

4401. Have you been generally limited very much in making cables by financial considerations?—We have not generally; we have recommended what, from experience, we have found to be the proportions required. What we call the Black Sea cable between Varna and the Crimea has been the foundation of a great many of our experiments, and the basis of our calculations.

4402. The length of that line was about 300 miles was it not?—About 330 miles. We therefore made the Bona cable to suit the shorter distance of 125 miles and laid it down. In the first attempt at laying it we made a mistake in calculating the weights on the break wheel, by taking kilogrammes instead of pounds; so that we had only one-half the weight on the break wheel that we ought to have had for the depth of water. We could not at first account for it; but when we got half way across we found out the mistake, and put on the proper weight. Consequently in the first half of the line we lost a very large quantity of the cable, but in the second part we lost a very small amount. If the mistake had not occurred we should have had plenty of cable on board the ship to carry the line the whole way.

4403. Was not the cable in a vessel that was towed by a steamer?—No, that was not our cable; our cable was laid from a large steamer the "Elba."

4404. (*Mr. L. Clark.*) Was the cable perfect when it was first laid down?—It tested perfect; it was not however tested as a cable under water.

4405. After it was first submerged and the four wires worked?—Yes; we had numbers of messages sent through each of the four wires.

4406. Then the faults that have since happened have come on spontaneously?—The faults have arisen in one or two of the wires, but they have been chiefly due to the land lines, and the cable has got the blame of it. Mr. Brett assured us that all the land lines for connecting the cables with the offices would be ready for us, but when we arrived at Bona there was not a land line to be seen, and we had to erect a temporary line ourselves, and the same at Spartivento, instead of four land wires there was only one; so that we were placed in very great difficulty indeed in making our tests, because the climate of Spartivento is so bad that any person remaining on shore is liable to catch fever immediately.

4407. Is it still possible to work all the four wires?—No; the cable has been lately broken within the last two months.

4408. (*Chairman.*) Is it a break, or an injury by lightning?—I understand that it is a break.

4409. (*Mr. L. Clark.*) At present the cable is not in use, I believe?—At present the cable is not in use. We are now arranging a vessel for the company to send out to repair the cable at this moment, a French vessel.

4410. (*Chairman.*) Do any faults develope themselves by degrees in any of the wires?—We have great difficulty in finding out the exact state of the cable, owing to its being in the possession of the French Government, and they have refused once or twice to give us the use of the cable again. They took possession of it immediately, and we had difficulties placed in the way of our testing it as we would have liked to have done; but the fact is, instead of three of the wires out of the four being bad, as stated by Mr. Brett, the French Government

F. S. Newall, Esq.
10 Aug. 1860.

have actually paid for two wires from the commencement. That was all that they required in their contract with Mr. Brett; and, from the evidence furnished before the arbitrators in Paris by the Company and the French Government, in the case of Newall *v.* the Mediterranean Telegraph Company, it appears that there were two wires in perfect working order up to February 1860, when the cable suddenly gave way. In July 1859 experiments were made on the two wires then in use, to ascertain whether there was any interference, by induction, between the wires when they were used simultaneously. Mr. France operated at one end for the Company, M. Margerie at the Bona end for the French Government. The result was, that there was no interference. At that time M. Margerie reported that two wires worked perfectly, and that a third wire would work if the land lines were in order. The fourth wire is doubtful; but for 10 days in July 1859, during which trials were carried on, all four wires worked perfectly well with from 20 to 30 Daniell's cells, and the ordinary Swiss Morse instruments. So that Mr. Brett's statements are contrary to the evidence.

4411. Are the other two wires used for commercial purposes?—Yes; the French Government only contracted for two wires, but Mr. Brett laid four.

4412. How did he get remunerated for the other two?—He wished to have the other two for commercial messages.

4413. Upon which he is paid?—Upon which he is paid.

4414. That cable was a heavy cable was it not?—It weighed about 3½ tons per mile; it was covered with iron. It was not so heavy as the cables which Mr. Brett had attempted to lay, and in all of which he had failed. One weighed 4½ tons, and the other 7½ tons per mile.

4415. Does your experience lead you to think that a heavy cable is safer to lay down?—As we have laid successfully the lightest cables that have been attempted to be laid, I think that the weight is not necessary or desirable unless near the shore.

4416. (*Mr. L. Clark.*) Do you consider that there is more risk in laying a heavy cable than a light one in deep water?—That depends altogether upon the situation. If the means for controlling its going out of the vessel are adequate, I do not see any danger in laying a heavy cable. We have laid the heaviest cables that have been laid, 13½ tons a mile.

4417. (*Chairman.*) If you were laying a cable to America, and if you had a proper ship, would you have much hesitation in laying an iron cable?—That would depend upon the weight. I would not, I think, hesitate in laying it; but I would not lay an iron wire covered cable there. I think it would be an unnecessary expense.

4418. When once the cable reaches the bottom, do you think it is safe in deep water?—I think it is comparatively safe. If Mr. Brett had had the information as regards laying cables, and the machinery suited for the purpose that I have invented, I think he could have laid that 7½ tons cable across from Cagliari to Bona in perfect safety.

4419. That would be in about 1,500 fathoms?—Yes; Mr. Brett has been misinformed in stating that there are upon that line a sort of irregular sudden depths, what we call Mr. Brett's precipices. He thought he had discovered several in laying down the cable, or he supposed that there were such from the sudden flights of the cable uncontrolled from the ship. We found in sounding the line across that it was nearly a dead level, with a fine muddy bottom, and that there were no precipices in existence there or anywhere else on the bottom. The French Admiralty have made correct soundings of it under the direction of Mr. Delamarche.

4420. From your own experience in the Mediterranean and the Indian seas, there is no sudden depth entitling it to the name of a precipice, or anything approaching to it?—Just so.

4421. In the Red Sea, is there not something of the sort, owing to the coral?—Yes, near the shore, but only near the shore in very shallow water. There I have seen a precipice. You may walk in in two feet of water, and then suddenly from the edge of the coral it goes down to a great depth.

4422. Where the water gets deep the silt has accumulated, has it not?—Yes, it is deposited gradually. It is therefore nearly a dead level, I believe, all over the world. It is generally stated that the bottom of the sea somewhat approaches the nature of the adjoining land, but that does not hold. In places where we have high hills, for instance, there are high mountains in Wales and Wicklow, but the bottom of the sea in the Irish Channel is nearly a dead level of 45 fathoms; and the same is the case in the German Ocean; it is nearly a dead level, and silt or mud is deposited gradually and equally, unless there is a strong current. There is a strong current between Port Patrick and Donaghadee, where we have laid two heavy cables. They cross the channel where there is a sort of ditch scooped out by the action of the current to a depth of 150 fathoms, where the adjoining soundings are about 100 fathoms; but still that is not a sudden precipice, it is a slope. The deepest water that we have successfully laid cables in has been across the Persian Gulf, at a depth of 2,000 fathoms.

4423. That was an iron covered cable, was it not?—Yes, that was an iron covered cable, exactly the same as we have laid from Suez to Aden and Kurrachee, weighing 21 cwt. per knot, and we paid it out at the rate of 8½ knots per hour.

4424. Is that part of the cable in perfect working order?—I believe so.

4425. On some parts of that line there are faults now, are there not, between Aden and Suakin?—In the section between Suakin and Aden, after having been worked successfully for nine months, a fault became apparent. We ascertained that they had used very heavy battery power just previously to the line being stopped. I was informed that they had used 200 cells instead of 40. A part of the cable that I took up in the Red Sea evidently showed that the gutta-percha had been damaged by no cause that I had ever seen before, and I think it was the battery power, from the burnt appearance of the gutta-percha, or lightning; at all events I had never seen any fault in the gutta-percha like it before.

4426. Have you any specimens of that gutta-percha?—No; they are in the possession of the Red Sea Company.

4427. Are they in England?—They are. Another fault that we cut out in the same cable, and which Mr. Lionel Gisborne in his evidence states to have been squeezed, does not bear the slightest appearance of squeezing. Before Mr. Gisborne saw it and before I saw it the gutta-percha had been acted upon by Mr. Gordon's knife, to what extent I cannot say, in order to ascertain whether the copper had been laid bare; but the gutta-percha had been cut out, and Mr. Gisborne has taken that specimen as being in the same state in which it was picked up, and has given evidence in accordance with his opinion in that way.

4428. Does it bear the appearance of having been burnt?—No, it does not.

4429. (*Mr. L. Clark.*) The great difficulty in telegraphy at the present time is the appearance of those spontaneous faults in submarine cables; have you formed any general opinion as to the cause of those faults, or is there any general cause, or do you know any remedy by which they may be obviated?—I have not seen the other specimens which have developed in the same section of the Red Sea cable, and I cannot give an opinion whether the cause I have mentioned may operate in the whole of the faults existing if there are more. I think the cause in this case has been the battery power, owing to the ignorance of the clerk employed who did it. They have some clerks who had never seen telegraphic instruments until they had been sent out to the Red Sea,

and, as I said before, they used 200 cells instead of 40.

4430. (*Chairman.*) Did they work with that power habitually?—No, only for a short time when they found the cable slightly out of order.

4431. Does that diminish your confidence in gutta-percha?—I think from what I have seen lately of faults in gutta-percha, that my confidence is entirely gone. I would never undertake to lay down a cable again laid in gutta-percha; the faults have been so great and so frequent that I have not the least confidence in it now. In fact, the Gutta-percha Company themselves seem to be of the same opinion, for Mr. Chatterton, their manager, has lately taken out a patent for subjecting gutta-percha to pressure in a tank containing a fluid substance, a combination of tar and so on, which will fill up the holes made in the process of covering the wire.

4432. (*Mr. L. Clark.*) Does he wish to fill up the holes or the pores of the gutta-percha?—I think it is to fill up the holes. We have had an immense number of holes cut out. They increased very rapidly indeed, from an average of about four faults in a drum, which contains a length of four miles up to 40 or 50.

4433. (*Chairman.*) Do you mean the Committee to understand that the more recent manufacture of gutta-percha has become worse?—Yes. With the increased demand the manufacture has become worse. In the first gutta-percha that we had, that is, up to 1854, the sizes of the covered wire then used were not so large as they are now. We used generally No. 2, and, I believe, with a larger coating, they have since used an inferior quality of gutta-percha, and therefore, we have an increased number of faults.

4434. Would the remedy for that be found in putting on thinner coats and more of them?—I think so; and they must use a better quality of gutta-percha.

4435. (*Mr. L. Clark.*) What was the general nature of those faults? Was it an air bubble or an actual electrical fault?—Chiefly air bubbles. I have here a piece of the Holyhead and Howth cable (*producing the same*) which we laid down in 1852. I think it has been stated by one of the gentlemen (Mr. Allen, 1613,) who has given evidence before the Committee that that cable failed the day after it was laid, but that is not correct. We telegraphed for four days after it was laid, and it then failed.

4436. (*Chairman.*) Is that a gutta-percha cable?—Yes; we laid it down in 1852 on our own account between Holyhead and Howth.

4437. (*Mr. L. Clark.*) Why did it fail?—We had no opportunity of taking it up until two years after it was laid, and then we attempted to take it up. It was covered with iron wire without any hemp between, and the wires were laid merely upon the gutta-percha. I took this piece up near Howth. It was evidently drawn out by a ship's anchor; that is, the iron wire was almost gone. The specimen before the Committee shows an extension of the copper and an extension of the gutta-percha. I will venture to say you will not get so good a specimen of gutta-percha now-a-days. It was very good gutta-percha compared with what we have now. About six inches of the copper was drawn out of the gutta-percha by the strain, and eaten away by corrosion.

4438. (*Chairman.*) Was it covered with thick iron wire?—No; it weighed about 14 cwt. per mile. In this specimen which I hold in my hand the gutta-percha has been drawn out for a length of four feet and reduced from the thickness of $\frac{1}{4}$ of an inch to less than $\frac{1}{4}$th.

4439. It was in fact far too weak to resist a boat's anchor?—Yes; and also to resist the very heavy current which runs round Holyhead at the rate of $7\frac{1}{2}$ or 8 knots an hour. I should think that the vibration caused by that current striking against the cable made it vibrate so much that it was cut through against a rock, just the same as the Channel Islands cable has been.

4440. Have you any information as to the failure of the Cagliari and Malta cable, and the Malta and Corfu cable?—We have had no opportunity of ascertaining the cause of the failure of the Cagliari and Malta line. It is said to be the result of submarine volcanoes. We believe it to be by boats' anchors in shallow water off Sicily. As to the Corfu and Malta line we have been informed that it was destroyed during a thunder storm. The lightning conductor was found damaged, and I have been informed that the table is supposed to have been damaged at a distance of 20 miles from the shore at Corfu. H. S. Newall, Esq. 10 Aug. 1860.

4441. Were the same precautions taken against lightning as in other cases?—Yes.

4442. You laid down that line did you not?—Yes, we laid down that line.

4443. Notwithstanding that a lightning conductor was used the cable was damaged?—Yes.

4444. Are there perfectly secure means of guarding a cable against lightning?—I think nothing but putting in a lightning conductor between the land line, if there is a land line, and the sea cable.

4445. (*Mr. L. Clark.*) You laid down a portion of hemp cable, did you not, in the Alexandria line which failed?—In the Levant we laid down a cable covered with hemp alone, in order to test practically that form of cable.

4446. Did you gain any useful experience in that instance?—Yes; we found that it would not answer in that part of the Mediterranean Sea, because when we took it up a few months afterwards we found that the hemp was completely eaten through by marine insects, the teredo.

4447. (*Chairman.*) In what depth was that?—It was in deep water, some of it 1,100 fathoms.

4448. (*Mr. L. Clark.*) Even at that depth do you think that the teredo had eaten *it*?—No; it was in shallower water that the teredo had eaten through the hemp.

4449. Had you any difficulty in laying that simple hemp cable?—None whatever. We laid 300 miles across the stormy Black Sea, from Varna to the Crimea, with no covering whatever but gutta-percha $\frac{1}{4}$ inch thick. I think there is no difficulty if the apparatus for paying it out is suitable for the cable.

4450. Had you good weather in laying down the Varna cable?—Tolerably fair weather.

4451. And the Alexandrian hemp cable also?—Mr. Liddell laid down the Crimean cable and the Levant cable, and in laying down the Levant cable he had very rough weather.

4452. As the result of the whole of your experience you think that with proper skill and suitable machinery, either heavy or light cables might be laid in deep water?—I think so.

4453. (*Chairman.*) For very deep water, if the cable is likely to be perfectly safe at the bottom there would appear to be no objection to laying it without any outer covering at all?—From our experience in laying the Red Sea cable, and finding that the iron wire is now completely corroded off it in various places, I think that it is worse than useless putting it on in any water except in very shallow water, and near the shore to guard it against ships' anchors. I would still guard the cable with very heavy iron near the shore, but in deep water I think any outside covering is useless.

4454. You think that the insulating material itself would suffice?—I should prefer imbedding the wires upon which we depend for strength, and which I would make of steel, in vulcanite or vulcanized india-rubber, so as to keep them entirely from the influence of the salt water. We find that the decayed part of the cables which we have taken up has generally been in places where there has been a great accumulation of decaying vegetable matter, where sulphuretted hydrogen is generated, and the sulphuretted hydrogen attacks the iron and corrodes it very rapidly indeed.

4455. That you would be liable to would you not at any depths?—Yes, we should be liable to that at any depths. We found the same thing occur in this

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Holyhead cable, that the iron wires were completely corroded off it, and there the cable went across a great deal of decaying vegetable matter.

4456. The strength of the steel wires would give strength in the paying out, would it not?—Yes, it would depend upon that, and I think it would require it.

4457. Yet you paid out the Varna and Balaklava line without any?—Yes, but we ran very great risk in doing so, and if it had not been for the urgent necessity of quicker means of communication with the Crimea we would never have undertaken it. We have been informed by the late Lord Lyons, that in his opinion the telegraph cable lessened the duration of the war by two or three months by affording the means of rapid communication with the Crimea. I think it was the most successfully laid cable that ever has been attempted, considering the nature of the material which was adopted.

4458. Did it not remain in perfect working order for a considerable time?—They worked through it for ten months, and we were informed by the director of telegraphs at Paris that it had been cut designedly just before the conclusion of the war. I found on examining the cable at Balaklava, that the outside wires had been cut through by a sharp instrument. There was a short stoppage in consequence, but we repaired it immediately. It was no mere accident, but it had evidently been done on purpose.

4459. Had you any experience with regard to the Atlantic cable?—As regards the Atlantic cable we were asked to undertake the manufacture of one half of it, namely, 1,250 miles, and we did so in accordance with the sample and specification furnished to us. That sample was a right hand cable, and we deposited a specimen similar to that before the committee of the directors, and made the cable in accordance with it, and when we delivered it to the company our responsibility ended.

4460. (*Mr. L. Clark.*) Was the insulation of your end very perfect when you delivered it to the company?—Very perfect, but it was not tested under water. We suggested to Mr. Bright, the engineer of the company, that the gutta-percha should be tested under pressure according to our patent, as we had already done with other cables, before it was manufactured, but the company declined going to the small expense of doing so, otherwise I believe that the cable would have been still more perfect than it was when we turned it out.

4461. (*Chairman.*) You had nothing to do with laying the cable?—Nothing whatever. The company took a licence from me to use my patent apparatus for laying it, but I was never consulted in any way whatever, not even as to the fitting up of the ships with that apparatus.

4462. (*Mr. L. Clark.*) Have you reason to suppose that the cable received injury from the exposure to the sun, or from any other mechanical reason?—I had no knowledge of it myself, I only understood from other parties that a great length of cable was injured by exposure at Greenwich. Our cable was manufactured under a shed and completely protected from the sun.

4463. Do you remember which end that was?—Our cable was put on board the "Niagara" under the charge of Mr. Woodhouse, and laid down on the American side of the Atlantic.

4464. (*Chairman.*) You have seen, I suppose, the report of Mr. Varley's attempts to raise that cable, and how completely the iron wires seem to have been destroyed?—Yes, I saw it in the "Times" newspaper, and it was just what I expected. I recommended the directors, before the contract was settled, that they should adopt a certain form of cable. They wished to know what form I would recommend, and I said I would inform them if they would give us the contract for making the whole cable, but not without. I have here (*producing the same*) a specimen of gutta-percha covered wire, made under my partner, Mr. Gordon's patent. It contains an insulated wire surrounded by steel wires; and then covered with gutta-percha.

4465. The steel wires, I presume, have a sort of twist?—They are laid spirally as it is termed.

4466. Else you could not get a pliant cable?—You would get a cable which would be very rigid, and a cable with longitudinal wires is more difficult to make, so as to get an equal strain on all the wires, than with spiral laid wires.

4467. (*Mr. L. Clark.*) Are the steel wires in contact with the copper wires?—They are completely insulated from each other, so that we could work through both sets of wires.

4468. (*Chairman.*) You do not anticipate any danger from induction?—No, not in the way I should use the cable. I would use india-rubber for protecting the wire, and vulcanized india-rubber for protecting the steel wires. I would apply it according to Hooper's patent. That is laid on either spirally or longitudinally.

4469. And a joint is made by heat?—Yes, a joint is made by heat.

4470. After the vulcanized india-rubber has been put outside?—Yes, the joint of the insulating material is made after the vulcanized rubber is put on in the manufacture. If heat were applied directly to the pure india-rubber it would destroy it. If pure india-rubber is put into hot water or subjected to the action of steam, it becomes tarry, and in the course of time the lump of india-rubber will be completely changed into a sort of tar from the action of the heat, a particular sort of decay goes on, even if the corner of a piece of india-rubber is subjected to heat the whole mass will become destroyed; a chemical change seems to go through the whole mass, therefore heat can only be applied properly to the insulator through the medium of a jacket, and that jacket Mr. Hooper makes of vulcanized india-rubber.

4471. Does he pass the core through hot water or steam?—The india-rubber covering is not passed through steam, but the raw vulcanized material is put on the india-rubber, then that is subjected to steam heat for about four hours at a temperature of 330 degrees.

4472. I presume the outer covering of vulcanized rubber is put on under great pressure?—It is put on by means of rollers. The insulated wire is put in between two fillets and then passed between two rollers.

4473. Like Siemens's application?—Yes.

4474. Internally the india-rubber is put on spirally, is it not?—Yes, but it may be put on either spirally or longitudinally. If it is put on longitudinally, then it must be in some way subjected to heat to make a complete joint, because if you take Siemens', at least such specimens as I have seen, and try to tear it, it tears longitudinally at once. If Hooper's is tested in the same way it will not tear so, it tears irregularly.

4475. After you had put it on by Siemens' process you would have to cover it by a jacket of some sort, and then submit it to heat?—Yes, by Hooper's process you might put on steel wires and then vulcanized india-rubber, and subject the whole mass to heat, and that would complete the manufacture. I think a cable made in that way would be far more perfect for deep water than any cable I have yet seen. In fact we are now arranging to lay down such a cable between Candia and Alexandria.

4476. In that part the depth of the water is about 1,100 fathoms, is it not?—It is 1,800 fathoms. We have failed three times in laying that cable, owing to faults in the gutta-percha, and not from any mechanical difficulty. We have had no mechanical difficulty in any cable that we have laid yet. The first cable that was broken in the process of laying down was the Atlantic cable. We took up Mr. Brett's cables which had been laid between Cagliari and Bona, after they had been down some years, and we found huge masses of cable all tied up together in a sort of Gordian knot, so that it was impossible to extricate the mass, and that must have been one of his precipices.

4477. (*Mr. L. Clark.*) Do you remember the depth at which you succeeded in taking up that cable?—About 1,000 fathoms; that cable was 7½ tons per mile. We got the shore end of it and we just hauled it on board.

4478. What is the greatest depth at which they have succeeded in grapnelling the cable?—I think Mr. Liddell has done so in 400 fathoms. When I laid the Cagliari and Bona line short about 11 miles from Spartivento and we dropped the end in 80 fathoms, I took the bearings very accurately by landmarks, and went out again some months afterwards and hooked up the end of the cable at once without the slightest difficulty. We have had very great difficulty in picking up the cables in the Red Sea owing to the coral bottom in shallow water in 35 fathoms, the grapnels get caught by the coral, and we were constantly losing them, and that has been one great difficulty in repairing this cable now.

4479. (*Chairman.*) Does the coral seem to injure the cable now?—No, we have never found any injury in the gutta-percha, even where the outer wire is corroded, there does not seem any mechanical injury to the wire. On the coral banks all is as sound as when laid. In the mud there has been strong local action which I cannot account for.

4480. (*Mr. L. Clark.*) Nor from insects?—Nor from insects.

4481. (*Chairman.*) What is the form of break that you prefer?—The break that we have used since 1852 is what we have used in the Red Sea.

4482. Is that a patent break?—The actual break wheel itself is not a patent, it is simply a break wheel; but probably from our experience we have arranged it in the best method now, so that we have no difficulty in laying down cables by means of it; we have the most complete control over the cable. In laying down the Indian line across a depth of 2,000 fathoms, we had not the slightest difficulty in managing it. Having ascertained the depth, we put on the necessary friction by means of weights, and simply let the cable run, and we did it with a very small loss of cable indeed, about 3 per cent., whereas in the Atlantic they lost about 25 per cent. of cable. In fact, our break wheel is so accurately constructed that we can almost tell the depth of water over which we are passing from the strain upon the break wheel, from observing the angle which the cable forms in going over the stern, and we have repeatedly told the man-of-war vessel accompanying us that we were altering our depth, just by judging from the angle formed by the cable.

4483. Have you paid much attention to the speed of working the cables after they are laid?—No, I have not. My duty was more the manufacturing of the cable and the paying of it out.

4484. With regard to the size of the copper wire, and the insulator, you must have paid some attention to that?—Yes, of course. All our cables have been calculated for the length and number of words per minute required, according to the theory first enunciated by Professor W. Thomson. The size of the copper, and the quantity of gutta-percha varies with the length.

4485. And your data have been well formed upon the experiments you have made?—Chiefly founded on the Balaklava cable.

4486. The speed of working that was rather slow, was it not?—No. I saw a message sent. The clerk was not aware that I was taking his speed, and I saw 17 words a minute sent through that cable from Varna to Balaklava.

4487. (*Mr. L. Clark.*) On what instrument?—On the Morse instrument. Our information has of course gone on, from every cable we have laid. Each has given us additional experience.

4488. (*Chairman.*) Do you subject the cables to a very close electrical test before you send them out?—Very severe indeed, and we have very careful assistants to look after the manufacture of the cables. In one or two cases we have had wilful injury done to the cables by the insertion of nails, by men doing it on purpose. That of course we found out.

4489. (*Mr. L. Clark.*) What should you say, from your experience, is the most suitable kind of cable to lay down in the Red Sea at the present time?—I would not again use gutta-percha. If our experiments upon Hooper's cable, that we have now in progress, are as satisfactory as they appear to me likely to be, I would use Hooper's patent.

4490. Without any external coating?—Without any external coating except near the shore; and I would depend for strength upon the wire imbedded in vulcanized india-rubber.

4491. To what depth would you carry your iron out?—I think there is no motion in the water beyond 20 fathoms, unless where there is a very strong current.

4492. (*Chairman.*) Should you not be afraid of anything running foul of it beyond 20 fathoms?—If there is any chance of ships' anchors running foul of it, I would carry it out deeper. I would avoid shallow water as much as possible. Ships do not like to anchor, unless for some extraordinary reason, in more than 30 or 40 fathoms. The cable between Singapore and Batavia has been damaged several times by ships' anchors.

4493. In what depth is it?—It is in very shallow water that we laid that cable, which is exactly the same as the Red Sea cable. Mr. Gisborne, in his evidence, states that it is slightly larger than the Red Sea cable, and that he prepared the specification. Now, Mr. Lionel Gisborne was not the engineer to that line. We made an offer direct to the Dutch government, and in our contract and specification it is stated to be exactly the same as the Red Sea cable. In fact it was a part of the Red Sea cable that we took for the purpose. There is a considerable part of Mr. Gisborne's evidence which appears to me to be given in a very careless and reckless manner. Statements are made for which there is no foundation, and therefore they are very incorrect. Any one reading it would suppose that he had carried out a great many experiments, but I observe that they are most of them our doing. At answer 38, he states that the failure of the Dardanelles and Alexandria line "must "be entirely attributed to the want of proper mate- "rials having been used in the manufacture of the "cable, and not to any fault inherent in the core "itself, as a core, or in the manner of laying it, as "laid down by Mr. Newall." We found, after taking up part of that cable that the faults were faults in the gutta-percha. Mr. Gisborne was not present at the laying of that cable, therefore what he states is only from hearsay.* He took no part in the Mediterranean expedition. What he says in his answer 65, regarding the manufacture of the Candia and Alexandria cable, the want of care, use of improper materials, and the neglect properly to test them, is entirely without foundation. Those cables were manufactured with all the care which could be used; the materials were the best we could

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* SIR, Gateshead, 16th Oct. 1860.

The following is an extract from Mr. L. Gisborne's letter to Mr. H. C. Forde, dated Suez, 20th April 1859:—

"We tested the cable yesterday and found it very *good*, both for "conductivity and insulation. The connections between the holds and "the testing room having been run along the deck during the greater "part of the voyage, had been injured by the heat, but the cable itself "is cool, and the gutta-percha apparently quite unimpaired."

And extract from Mr. Lewis Gordon's letter to Mr. H. C. Forde, dated Suez, 25th April 1859.

"The weather continues beautiful. The cable on board our ship tests "exactly as it did in the manufactory—uniformly perfect. We hope it "will be the same when laid, and that that in the 'Imperatrix' will "be in like good condition."

Compare these with Mr. Gisborne's reply, number 15.

"I may mention, as a question of heat, that when he first tested that "cable on board the 'Imperador,' about 900 miles of cable, at Suez, the "insulation was so bad that we could not speak through it on board "ship."

Any one reading Mr. Gisborne's evidence would suppose that the whole 900 miles were affected by the heat in the way he states.

I beg that his explanation may be added to the evidence.

I am,
Your obedient servant,
R. S. NEWALL.

Captain Douglas Galton, R.E.,
Board of Trade.

R. S. Newall, Esq.

10 Aug. 1860.

procure, and the whole was subjected to the most searching tests before they left our works, and they appeared to be in every respect perfect. It was only when the cable was subjected to the pressure of 1,800 fathoms of water, or 5,400 lbs. per square inch, that the faults in the gutta-percha, which were not visible under our test of 1,000 lbs., became apparent. It was the experience we gained then that induced me to write to the Treasury on the 16th November last, recommending them not to lay the Gibraltar cable. It was also in consequence of this that we did not tender for that cable. He was not allowed to interfere with the laying of the Red Sea cable.

4494. Was he the engineer to any of those lines; the Candia and Alexandria lines?—No. Mr. Gisborne had nothing to do with the manufacture or the laying of those cables. We bought the concession from him, and undertook to carry out the lines on our own account. We never consulted him; and he is not in a position to give an opinion about them.

4495. He was only connected with them in having originally obtained the concession?—That was all. We were our own engineers, and we had a strong interest in making them as we thought best. Owing to faults in the gutta-percha, we have lost 800 miles of cable between Candia and Alexandria.

4496. You are now going to lay that line, are you not?—We have not decided upon going on with it, owing to the interference of the English Government with our concession. They have compelled the Turkish Government to cancel our monopoly granted in 1859, and until that point is settled we are not decided what to do. We are anxious to do it, and to make arrangements with the Government, but we have a strong opposition to contend with.

4497. Is the Ragusa line intended to go to Alexandria or only to Candia?—The Ragusa line goes from Ragusa to Corfu, Cerigo, and Alexandria. It is intended to avoid Candia entirely; for what reason I do not know, as it is a longer distance from Cerigo than it is from Candia.

4498. Has anything been arranged as to the manufacture of that line?—I believe nothing whatever has been done.

4499. In whose hands is it?—In the hands of the Austrian Government. The Austrian Government have undertaken to lay the line, and the estimate for it is 500,000*l.*, but there is nothing specified, so far as we know, as to the sort of cable that is to be used. We had a contract in 1858 with Messrs. Brett and J. A. M. Pinniger to lay down that line touching at Candia for 365,000*l.*, and they went to Vienna, and induced the Minister of Finance to grant to them a provisional contract for doing the same thing for 500,000*l.* They would therefore have pocketed 135,000*l.*, but as they could not obtain a firman from the Turkish Government for laying, the contract was put an end to. In our last attempt at laying the Alexandria and Candia cable, we had got over the deepest water, and were within 70 miles of Alexandria when the insulation began to fail, and before we could decide whether to continue laying down, we had laid down 30 miles of cable beyond the fault. We at length decided not to lay down the faulty cable, and we had recovered 20 miles out of the 30. We had gone back from 1,200 fathoms into 1,600, and the cable came up as perfect as it was on board the ship, when it broke at the bottom.

4500. Was it a hemp-covered or wire-covered cable?—It was a wire-covered cable, partly the Red Sea cable.

4501. You were not able to arrive at the fault?—We had not arrived at it when the cable broke.

4502. Have you attempted to recover it?—We have not, except from the Candia end. We recovered some 70 miles again into a depth of about 1,000 fathoms, and then it broke again.

4503. (*Mr. L. Clark.*) That was also an iron-covered cable?—Yes.

4504. (*Chairman.*) I suppose it would be hopeless to pick up any cable at a depth beyond 1,000 fathoms?—I think so. No cable, I believe, has been picked up beyond 1,000 fathoms. With regard to hemp-covered cables, we found in the Levant cable that with the great weight of hemp outside, the small strength of the copper wire conductor was not sufficient to raise the cable in about 400 fathoms, the insulated wire always broke.

4505. In fact, hemp you consider to be an objectionable form of covering?—I think so, decidedly.

4506. Do you think that hemp combined with wire, as in the case of the Falmouth and Gibraltar line, is a good system?—I do not see that there is any advantage in it. I consider it a very bad form.

4507. Are you aware whether hemp undergoes any injury from deep water?—We found in the Levant that it was completely eaten through in pieces of about an inch in length.

4508. But in the deep water the pressure did not seem to produce any effect?—I think the water, in course of time, will rot it.

4509. (*Mr. L. Clark.*) Did it suffer from contraction of the hemp?—Not the least. The hemp was laid on by one of my patent wire-rope machines, so that the twist was not taken out of the yarns, and we then got a stronger cable than if it had been laid on in the common mode of rope making; and we found, by direct experiments made at Birkenhead, that there was no contraction when the rope was made in that way. We stretched a piece across the Birkenhead docks on purpose to test, very accurately, whether there was any contraction; the length was about 200 yards. It was done under the superintendence of Mr. Jenkin, and we found that there was no contraction, and that it would not do the slightest damage to the hemp cable. We therefore went on manufacturing sufficient to lay down between Chios and Candia to test the experiment on a large scale; we had no difficulty in laying down that cable, and in what we did take up, the gutta-percha did not appear to be in the slightest degree injured by contraction or elongation.

4510. (*Chairman.*) That line is in working order now, is it not?—Yes; but it is a wire covered cable. Our hemp covered cable failed from air-holes that we found in the gutta-percha. We therefore took up that cable, and we have laid down fresh wire covered cable, which has now been for the last 12 months in full working order.

4511. In what way did those faults from air-holes develop themselves?—In the process of putting on the gutta-percha during the time the wire was in the cylinder, air is included between the coats of the gutta-percha.

4512. When the cable was first laid down it was in good working order, was it not?—Yes; it was by degrees that the faults were developed. The currents seem to have increased, or rather to have created the holes in the gutta-percha, and to have entirely ruined it in that way. As regards the Gibraltar cable, I see no advantage in covering the steel or iron wires with yarn, as in this sample before me. I believe if experiments are *fairly* made there is no increase of strength by doing so.

4513. It is stated that there was an increase of strength obtained by that method?—I think that must have been owing to some imperfection in making the experiments. We have made thousands of experiments on wire, and hemp covered with wire, and we find that the strength is always less than of both the strength of the yarn and the strength of the wire separately. If, for instance, a wire bears six cwt. and a yarn bears half a cwt., and if the yarn is carefully laid round the wire at the angle which will give the greatest strength, the two combined will never bear six and a half cwt.

4514. (*Mr. L. Clark.*) Have you found the yarn and the steel difficult to make adhere together?—We have not made many experiments upon this cable. We made these specimens (*pointing to a cable composed of hemp and steel*) for the Government. They were

made during my absence by our manager. I see not the least advantage in this form of cable, and I would rather have put on the steel wire without that mixture of yarn.

4515. (*Chairman.*) Do you see any advantage in having the steel wires protected by the hemp from water against rust or decay?—No, I think not; I think it would not protect them in any appreciable degree; at least, not to cover the expense of putting on the yarn in this manner.

4516. (*Mr. L. Clark.*) You laid down the Irish cable without any serving on the wires, but, as I understand you, you do not think that a safe system?—I think not, because of the joints of the wires; there is a chance of their cutting into the gutta-percha and damaging it. It was the first cable that we made after the Dover and Calais line, before the cable that we laid to Holland; it was the second cable that was laid. The gutta-percha appears indestructible in water.

4517. Have you ever seen anything like decay of the gutta-percha in water, or any change?—I have not.

4518. (*Chairman.*) There is a statement by Mr. Gisborne as to the strength of hemp and steel combined at question 98, page 11, where he says, "Thus, if I take one of these steel wires, and break "it by tension, and then take a certain quantity of "hemp, say, a pound, and put it into a strand and "break it, and add the sum of these two together, "they make, say, a ton; I can then put that hemp "upon the wire in such a form that the hemp and "the wire together will bear more weight than the "sum of what they will separately." Do you agree with that?—Mr. Gisborne in the first part of that answer says, "I have made a very large number of "experiments upon steel, welded and unwelded, "from different manufacturers." I believe those experiments were made by us, but in reply to that statement of Mr. Gisborne's, I think his experiments must have been incorrectly made. I do not think it possible, from my experience of wire-rope making, and from the experiments which I have made. If it is possible, it certainly is, as Mr. Gisborne calls it, a mechanical paradox.

4519. (*Chairman.*) With large-sized wires put on in a spiral form, do you consider that the core is protected by that sort of arch which is obtained, seeing that when this cable is placed under strain, these different wires (*illustrating the same*) are prevented from coming in at all?—I think that the core is protected in that way, and in all our experiments upon the strength of submarine cables we have never found the core in the slightest degree injured, even up to the breaking point. It is only after the wire is broken that the core becomes in the slightest degree injured. We have broken submarine cables over and over again experimentally, and we have never yet found the core injured except by the breakage.

4520. (*Mr. L. Clark.*) Do you think the spiral form at all objectionable from its elasticity and expansion?—No, I think not, because the spiral form I consider perfect. Of course I would make it a long spiral; if you make it a short spiral, you may injure the core from the extensibility of the covering.

4521. (*Chairman.*) A long spiral prevents the very great extension in the cable when the weight is put on?—Yes; you may go from a spiral of a single wire up to a spiral where there are 20 wires, as it were, in the cable. In a cable of one spiral you have no strength whatever, and the core would break immediately, but when you come up to a long spiral, such as we have found out by experience to be the best, then I should say that the core is not injured at all by elongation or by compression.

4522. Assuming that you have a cable made like this (*pointing to specimen on the table*) Messrs. Wells and Hall's patent, with the wires laid in longitudinally, and kept in their place by a hempen covering; when this cable is coiled, the outer wire necessarily assumes the form of a spiral?—Necessarily so; and there is another point in regard to coiling cables. It has been stated by several of the witnesses that it is disadvantageous or wrong to do so; but what we do is to coil the cable so as to take a turn out for every coil that the cable makes round the cylinder. Then when this cable is paid out over the ship, that turn is put in again, and the cable goes out over the stern just as if it had come direct from the machine; there is no turn put in or taken out. I have never seen the slightest disadvantage arising from the spiral form of cable.

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4523. When you take up the cables again they are not injured, they are not opened out or closed?—Not in the slightest degree. It is impossible that they could open out; they are fast at the shore, and they are fast on board the ship; so that unless the ship itself should make a turn there could be no untwisting, and there is no untwisting. I have examined cables over and over again, going over the stern, and there appears to be a slight twist, but that is only the turn put into it which has been taken out in the hold, in coiling it in the hold.

4524. (*Mr. L. Clark.*) You equalize the strain?—Yes; there must be an equalizing, because as you come near the coil next the cone, the coil is put into a length of, say, six yards, and when you come to the outside it is a length of 30 yards; therefore as there is a certain length of cable in suspension in the water, the turn which is taken out in these unequal lengths tends to equalize itself.

4525. In a cable with the wires laid longitudinally along it, would there be a great difficulty in getting a uniform strain upon the wires?—Yes; we have always found great difficulty with them. In the spiral wires that is equalized by the machine; there is an equal strain put upon each wire, and as it is drawn off by the machine these wires are laid with an equal tension round the core. This is proved by the mode in which the wires break; we generally find them all break together; whereas if they were not equally strained they would break unequally one after the other. We tested a cable the other day containing a core of Hooper's insulated wire for the purpose of seeing its action under strain. It was surrounded by yarn, and there were 16 wires on the outside in the same form as the Red Sea cable; we put it under tension, and the elongation of the cable was very slight indeed. Up to more than half of the breaking strain it was not above $\frac{1}{4}$ inch in 10 feet that, is, under 0·2 per cent, or about one fourth part of the steel and hemp cable described in Mr. Gisborne's reply, number 93. We tested the core after being subjected to that strain, and we found *it* not in the slightest degree injured or altered.

4526. You consider Hooper's cable, do you not, to be very perfect in its insulation?—It is superior to anything we have ever seen as yet; it is about ten times better than the gutta-percha specimens that we have.

4527. You anticipate no evil effects from the sulphur in the outer covering penetrating into the interior?—No, so far as we can judge from the oldest specimens there appears to be nothing disadvantageous in it.

4528. (*Mr. L. Clark.*) Have you any experience of joints in vulcanized india-rubber?—The piece we are testing at Birkenhead contains two joints in a length of a quarter of a mile.

4529. Are they mechanically perfect?—Yes, they appear to be as good as any other part of the cable.

4530. (*Chairman.*) Is there any difficulty in making the joint on board ship, for instance, or when you are away from port?—No, I think there is no difficulty in making the joint.

4531. (*Mr. L. Clark.*) There is a specimen on the table of vulcanized india-rubber jointed, could that be done in a cable on board ship?—Yes, I think it could be done easily.

4532. (*Chairman.*) Have you no fear of the action of the sea or other water upon them?—I have specimens that have been made a great many years ago,

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and the water appears rather to protect the vulcanized india-rubber in the same way as it does gutta-percha. In fact the valves of steam-engines, which are subject not only to the action of the sea water, but to the mechanical action of beating, and which are made of vulcanized india-rubber, appear to be almost indestructible. In Mr. Hancock's book on the manufacture of india-rubber, he gives an instance in which valves have been in use for four years, and have worked upwards of 100,000 miles on board ship at the rate of 75 to 100 strokes per minute.

4533. And they were perfect at the end of the time?—Yes, they were perfect at the end of the time, and Mr. Hooper's water beds have been in use for upwards of ten years, are still perfect. Therefore water appears to be a preservative for vulcanized as it appears to be for gutta-percha. We have tried the absorbing power of this vulcanized wire, and we find that it takes up almost no water, the quantity we can scarcely measure. These experiments have been made under a pressure of 800 to 1,000 lbs. to the square inch, and it is not acted upon at a temperature of 300 degrees. We found in laying down the land lines in the Red Sea, which we had to do in trenches, that almost before we could get them laid the gutta-percha was melted; therefore we had to lay the land lines at night, and to water the earth to keep it cool.

4534. For hot climates the gutta-percha is in your opinion eminently unsuited?—Totally unsuited.

4535. Can you give the committee any statement as to the present condition of the Red Sea and India cable, taking it as a whole?—I have had no information lately. We repaired the cable, and the last intelligence I had was from our superintendent, Mr. Risley, by a telegram, dated the 8th of July; he was then about to make the last joint between Aden and Suakin. That joint was made, and he went with our vessel to Aden. The cable worked for four days, and a fault again appeared in it, and the nature of that fault they have not yet ascertained. It is between Aden and Suakin, on the part that we had been repairing.

4536. What have been the causes of the faults generally?—The only two that I have seen are those that I referred to before, one of which I believe to have been done by very great battery power or lightning; and with regard to the other I am not able to give any opinion as to what the cause was, but certainly it was not by squeezing, as stated by Mr. Gisborne.

4537. The whole line had been in working order?—It had been in perfect working order for nine months, from Suez to Aden. This had failed just before we completed the Indian section; but as after this stoppage messages reached England in six days from Calcutta, I presume the stoppage arose from some wrong connection of the instruments in this stations. The Indian line was certified as perfect by the company's superintendent, and taken over by him on behalf of the company. We were responsible for its continuing perfect for a month after it was laid, and each section we had handed over as having stood a month's test.

4538. Do you expect that it will be in working order again?—I see no reason why it should not be so, or why it should not continue so. Their greatest want is a good steamboat and a good submarine superintendent.

4539. You think that there is nothing seriously the matter with it?—No, I think not.

4540. (*Mr. L. Clark.*) Are you still repairing the faults in it?—We undertook it on the part of the company, but I am not certain whether our vessel has left or is still employed by them.

4541. (*Chairman.*) You have had considerable experience in contracts for laying telegraphic cables. Do you consider that it is a good plan for individuals to contract for the cable to be laid at the risk of the contractor?—I think it is not.

4542. Why do you think so?—I think that the risk is too great to be put on the shoulders of any individual or firm. I think that all those lines that we have laid down ought to have been undertaken by the Government; and the only reason why the matter has been left so much to contractors is that there is no one who is possessed of the information necessary to enable him to specify what is required to be done. The business of submarine cable manufacture has only risen up within the last ten years. I may say we were the first to take it up, and therefore we were obliged to gain our own experience and to pay for it, which we have done pretty heavily by losses; but there was no one else who has taken up the matter until very lately, and we have not found any great advantage in what they have done. Several cables have been designed by so-called telegraphic engineers, as they are placed in juxtaposition with contractors, and, so far as I know, those cables have not been found any better than those left entirely to us, if so good.

4543. But would you place no risk upon the contractor at all in case of failure?—I would of course place some risk upon him, as an inducement for him to do his best in carrying out the work. I am referring now, I may say, to cables which have been laid in the Irish Channel. For instance, we laid down two lines from Portpatrick to Donaghadee, and those lines were left to us entirely; in fact, for one of them we had no contract until after the cable had been laid down, and the company, as a matter of form, wished it to be done; but we undertook the specification of them, and the manufacture was left entirely to us, and we carried them out, and they have not cost a penny from the time that they were laid in 1853 or 1854 till now. They are heavy cables, but there is a strong current in the channel, about seven knots an hour sometimes.

4544. There the company stated, did they not, what the cable should be required to do?—No, the company left that to us entirely. We knew that the distance was only some 26 miles across, therefore we gave them the large wire necessary for that short length, and a larger gutta-percha than was necessary. And comparing our lines with others, I may state that the insulated wire that we used in the Bona and Cagliari line is exactly the same for 125 miles as has been used by Messrs Glass and Elliott between this country and Denmark, a distance of 460 miles; therefore, I think the owners of the Danish cable must find it rather difficult to telegraph through it. We have always based our calculations on giving rather more than was required for the work.

4545. At all events the system that you would suggest, would be that the company should state to the contractor what work they required to be done, and leave the whole thing in the contractor's hands?—I would leave the whole thing in the contractor's hands; that is, in the hands of those ultimately responsible for the working of the line.

4546. But you would not put a future liability upon him, except as a certain amount of penalty?—I think so. I think we shall never again undertake the risks which we have done in laying cables.

4547. Is there any other point on which you could give information to the committee?—With regard to testing insulated wire under pressure, I see that the core of the Gibraltar line is to be subjected to a test of 1,000 lbs. I think that is a mistake. I think that the cable should be subjected, if possible, to the strain which it is to bear. You would require for the Gibraltar cable for a depth of 2,500 fathoms, a pressure equal to 7,500 lbs. to the square inch, and therefore to test it only under 1,000 lbs. is not what it ought to be. I would test every covering of gutta-percha as it is put on. The Gutta Percha Company have always avoided subjecting the gutta-percha to a proper amount of pressure. They subject it to pressure in a canal at a depth of 10 feet, which I consider no pressure at all. We have found length upon length of insulated wire sent to us under that pressure fit for nothing.

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4548. (*Mr. L. Clark.*) Have you tested gutta-percha under great pressure?—We have not gone over 1,000 lbs. to the square inch; we have not machinery for it, but I think it might be done. But I see that they are reducing the test of the Rangoon and Singapore cable to 600 lbs. We test under a pressure of 1,000 lbs., and even that has not proved enough in the case of the Candia and Alexandria cable, because, though it stood that pressure and appeared to be perfect, yet when we came to lay it down under a pressure of 1,800 fathoms it proved faulty, and it has done so in three cases.

4549. Would you test india-rubber?—I would test india-rubber similarly.

4550. For what length of time?—If the tests are very perfect at first I think no great time is required; a few minutes, say ten minutes, would be sufficient.

4551. (*Chairman.*) The method which you suggest for making the cable is, first, covering the copper with india-rubber and then putting steel wires on, then covering the wire with vulcanized, and then putting it through heat; would not that render it necessary for the whole to be made by one person?—I think so, and I think that would be an immense advantage. The whole responsibility then comes upon one person.

4552. And the contractor for the outer covering never can practically take the risk for the gutta-percha?—I think not.

4553. (*Mr. L. Clark.*) Would it be your intention to insulate perfectly both the interior conducting wire and the exterior steel wires?—Yes.

4554-56. It would be necessary to do that, would it not, otherwise you would have the amount of induction due to the size of the steel wires?—Yes, I would insulate all the wires, and I would make the cable in one length, and have no joints.

Benjamin Sharpe, Esq., examined.

B. Sharpe, Esq.

4557. (*Chairman.*) I believe you wish to explain to the committee some very recent improvements which you have adopted in electric telegraphs; we have seen this plan which Mr. Saward sent us?—What I sent to Mr. Saward was done in a great hurry, from seeing an advertisement in the "Times;" it was simply illustrating my plan, but the principle of longitudinal protection was secured to me in the year 1857.

4558. What is the reason why you prefer the system of longitudinal protection for the outer covering of the wire?—Simply because it resists the strain immediately, and prevents the slightest elongation of the core. I have made drawings of my plan (*producing the same*).

4559. Have you found that there has been any very great injury to cores which have been protected by spiral wires?—Spiral wires stretch like a cork-screw. If you merely require strength for lateral protection, they are or are not necessary, but if you require to strengthen your core to prevent it elongating, there is no question that the longitudinal system can alone do that. I found that the spiral system was so adopted by the wire makers, and that they had got so wedded to the system of spiral telegraphs, that I thought it was almost hopeless to contend with it; but I read in the "Times" the other day, in a very long article, that "there is not a single engineer of "eminence who does not condemn the principle of "laying on the outside wires spirally, instead of lon-"gitudinally, in a line with the strain they have to "resist." I then thought that it was time to move more actively in the matter.

4560. Have you a machine for laying on these wires?—Yes, and this wire has been manufactured.

4561. To any extent?—No, not to any extent, but this (*producing the same*) is a drawing of the machine, which is of the very simplest description.

4562. Do you find no difficulty in giving all the wires an equal strain?—None whatever.

4563. Is the machine in existence?—I have not been manufacturing it upon any large scale, but it would be done if I commenced manufacturing in an extensive way.

4564. (*Mr. L. Clark.*) Your principle is that of putting on straight iron wires parallel to the core, and covering it round with some external covering; is that covering hemp?—My patent includes both hemp and wire, and as is generally the case, embraces a number of matters connected with the telegraph. I have drawn up a short explanation of the different modes of protecting the internal core, which I will read. The only objection that has been urged to the longitudinal system, that I am aware of, has been the want of sufficient flexibility.

4565. (*Chairman.*) How do you obtain flexibility?—By means of india-rubber. If it is on a flexible core, of course the core will accommodate itself to the wires. They will press in.

4566. But will not that injure the core?—No; if you take any elastic body like a piece of india-rubber, and alter its form, by giving it the smallest bending, the whole accommodates itself (*describing the same*).

4567. Where is the india-rubber applied?—The india-rubber is the insulating medium. I may say that very recently I have been turning my attention more particularly to india-rubber, and I think we have in progress a cable which will offer more advantages in a mercantile point of view than perhaps any cable that has ever been devised.

4568. Are the wires in the drawings made of copper?—Yes, and they are made in the hexagon form partly to save the cost of india-rubber.

4569. For what length of cable is it intended?—For the Transatlantic cable.

4570. What size would it be?—That (*pointing to a specimen*) is the exact size of the Atlantic cable, only I have placed the wires on my own principle, in what I consider a superior form, adopting india-rubber instead of gutta-percha, and placing strands of wire in a longitudinal instead of a spiral position.

4571. Have you proportioned the thickness of the copper conductor to the length better than was done in the Atlantic cable?—I have very much increased the size of the copper conductor, which I think was far too small before. The india-rubber is not part of my patent, but I am adopting india-rubber because it enables me to carry out the longitudinal system in a more successful manner; but, since I have been making up this form of india-rubber cable, Messrs. Silver have authorized me to state that it can be manufactured by their process. By this particular system of mine a sufficient amount of insulation is first given by india-rubber to the central wire. On the surface of that india-rubber six wires are then placed, which, of course, form a hexagon, and being all equidistant from the centre, they form a series of equilateral triangles with it. The whole are then wound round with india-rubber, and a junction with the first india-rubber effected under Messrs. Silver's patent, you may then add additional wires and repeat the operation as often as you like, so that you may have 7, 19, 37 or any number of conductors all insulated by an equal substance of india-rubber.

4572. (*Mr. L. Clark.*) Is what you have been describing one of the essential features of your patent?—Not of this patent, but it is a plan which has occurred to me, and it is being manufactured under my name. Whether I shall confine it to myself or not I have not decided. If the committee will allow me, I will give them a description of the various drawings before them. No. 1 represents a cable with the copper wires insulated with india-rubber by Messrs. Silver's patent process. This cable is protected by 16 No. 13 wires, secured at intervals by a finer wire, and is then served over with spun yarn and tarred; it will be found to be a light and efficient cable, and from the wires being placed on an elastic

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body they will accommodate themselves readily to the cable whenever it assumes a curved form.

4573. I imagine your improvement to be placing the wires longitudinally?—Yes, that has been secured to me since 1857.

4574. (*Chairman.*) That is the main part of your patent, is it not?—No, there are other peculiarities, there is also the making of it, and there is the paying-out apparatus.

4575. (*Mr. L. Clark.*) I see there is one general principle running throughout, that of a longitudinal wire protected by an outer covering?—Yes, but it is not of single wires alone, I make them in strands. In my specification I say, "This invention has for "its object improvements in electric telegraph cables, "and in apparatus used for paying-out such cables. "In the manufacture of electric telegraph cables, in "place of coiling the exterior protecting wires or "strands round the interior telegraphic core, as is "usually now done, the protecting wires or strands "are arranged longitudinally of the core, and are "secured around it by a fine wire." No. 2 is a cable capable of resisting a very great strain, as it is provided with no less than 44 No. 13 wires as a means of protection. The insulation of the copper wires is provided for on Messrs. Silver's patent, as in No. 1. There is also a coating of india-rubber between the two layers of wires, which allows the cable to be easily bent, and at the same time furnishes an additional means of insulation. No. 3 is similar in dimensions to No. 2, but instead of a second insulation of india-rubber, a coating of hemp only is placed between the protecting wires, to give them flexibility and reduce expense. No. 4 has the conducting wires insulated with india-rubber, in the same manner as before, but the protecting wires are coated with india-rubber, which both protects them, and imparts to the cable a very great degree of flexibility. No. 5 is constructed in the same manner as No. 4, but has much additional strength and protection given to it by the insertion of additional wires in the interstices formed by the contiguous circles of coated wire. No. 6 describes a cable of the same dimensions as the Atlantic cable, but with india-rubber instead of gutta-percha as an insulator, and the protecting wires arranged longitudinally instead of spirally of the core. No. 7 shows that the diameter of No. 6 may be greatly reduced in consequence of the greater insulating power which india-rubber possesses over gutta-percha. No. 6 has the same diameter as the Atlantic cable, but when india-rubber is used, so great a thickness of insulating medium becomes unnecessary; three instead of eight stranded wires are also given, which weight for weight are much stronger.

4576. (*Chairman.*) You have taken the thickness of the india-rubber from Messrs. Silver, and not from any electrical experiments of your own?—Just so. I do not claim that as my patent, only it is carrying out an object which would be of great commercial advantage. No. 8 I leave for a moment; No. 9 shows the protecting wires placed in an elastic coating of india-rubber, in order to give flexibility. No. 10 is the same, but in this case the core with its protecting wires is surrounded by a flexible air-tight tube, secured at short intervals with wire, by which means a series of air chambers are formed, which, by lessening the specific gravity of the cable when first paid out, materially lessens the risk of fracture from any sudden strain, and as the cable gradually sinks the increasing pressure of the water causes a gradual compression of these air chambers, until at last the tube adheres with much force to the core, to which it imparts an additional means of insulation.

4577. Do you think there would be no difficulty in paying it out over the break?—There would in that form, but if it is on india-rubber there would be no difficulty. I should prefer three stranded wires tested up to 2 cwt. or 3 cwt.; these will stand between 2 cwt. and 3 cwt. each. No. 11 is the same as the last, but in this case the protecting wires are shown as reduced in number to the extent which the reduction of the specific gravity of the cable has permitted. No. 12 shows a light cable without any protecting wires, which are dispensed with, owing to the great reduction of the specific gravity by the use of the tubes. These tubes might again be served over with spun yarn, which I think is a very great advantage to every cable, and they might be tarred, which is, I think, a very great protection, and it enables the cable to last much longer.

4578. (*Mr. L. Clark.*) Is that a feature in your patent?—It is included in the patent.

4579. There is no novelty in using the wires in the way you describe?—I have seen it attempted by means of circles of separately insulated wires, but it is a novelty doing it in the way I am now doing. No. 13 is a cable with a very large conductor, and trebly insulated, with its specific gravity greatly reduced; No. 14 shows the amount of strength, which, if needful, can be given to a cable, and that each protecting wire being surrounded with india-rubber the greatest amount of flexibility can at the same time be secured; No. 15 describes five conductors separately insulated, with the strengthening wires so arranged as to secure flexibility; No. 16 shows a mode of economising india-rubber and space with three conductors possessing great strength and flexibility. If you revert to No. 8, you will see that it is a seven conductor double fortified. This cable will be found to possess, both in a mercantile view and as regards insulation, immense advantages over any cable yet invented. It supplies at once seven distinct means of simultaneous communication for the public, whilst the central wire, being surrounded by an additional coating of india-rubber, has at the same time a double security given to it, and yet this cable is only constructed of the same diameter as the Atlantic telegraph cable, which has but one wire of communication.

4580. (*Chairman.*) That is simply a statement that india-rubber possesses greater insulating power than gutta-percha?—Yes; but the advantage I have is the double insulation and reduction of diameter. No. 17 is constructed on the same principle as No. 8, but with an additional coating of india-rubber, by which 12 additional conducting wires are insulated, and at the same time a treble protection afforded to the central wire, and a double protection to the six wires adjoining it—a cable with 19 distinct means of communication is thus obtained.

4581. Do you mean all those wires to be used at the same time in conveying messages?—Yes, I intend them to be used simultaneously. The principle which is here adopted is that of surrounding each separate wire with the same thickness of insulating medium, and whatever that thickness may be, all alike enjoy it. To prove this, circles can be drawn around each wire with the same radii, the distances between the wires forming a series of equilateral triangles, and composing a hexagon. The india-rubber is laid on in distinct layers, under Messrs. Silver's patent. Each layer forms a distinct means of insulation. This form of cable is of necessity most economical, both as regards space and the amount of india-rubber used, as, owing to the mode of junction, the india-rubber which surrounds each wire is at the same time the insulator of the six adjoining wires. If 19 circles are joined together it will show the great diameter the cable would be, and the increased quantity of india-rubber that would be required, and yet each of these circles would only be simply insulated instead of doubly, trebly, or quadruply, according to the number of the coatings. No. 18 is the same as No. 17 with an additional layer of 18 conductors making in all 37. By adopting this plan of separate insulation you reduce the cost of each wire from 20*l.* per mile to 8*l.* 12*s.* 11¼*d.*, which is a very important feature in it. The manufacturers say that there will be no difficulty about the manufacturing of these cables. I may observe that an uncle of mine, Mr. J. R. Sharp, of Dochill, near Alfreton, was the first inventor of

electric telegraphs; in 1813 he exhibited his invention before the Admiralty (so it appears in the Patent Office) with three miles of wire.

4582. The main feature of your improvement is rather an electrical question?—Yes; but this plan of paying out the cable is another important feature, inasmuch as you may greatly reduce the weight of the cable.

4583. What is the principle of paying it out?—The simple principle is this. (*The witness explained a drawing of the paying-out apparatus.*) The cable passes through a block here, and there is a rope attached to the block and rove through another block and a compensating weight, and any sudden strain coming causes this weight to rise.

4584. (*Mr. L. Clark.*) It is not a new invention; it has been patented and described in a great variety of forms, if it has not been used?—But not patented or used before 1857. It is a first invention, and a simple contrivance by a sailor, and I think it answers the purpose. If you can pay your cable out by a means of that kind, you do *not* require so great a strength of cable, and you may reduce your iron wires materially. If you guard the cable against any sudden strain, as the stern of the vessel rises, of course this compensation gives, and you may weight it to any extent you choose. It is far better than the contrivances which they had on board the "Agamemnon," which were very injurious and not well adapted for the purpose.

B. Sharpe, Esq.
10 Aug. 1860.

Adjourned.

Friday, 24th August 1860.

PRESENT:

Captain DOUGLAS GALTON.
Professor WHEATSTONE.
Mr. BIDDER.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

FREDERICK CHARLES WEBB, Esq., examined.

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4585. (*Chairman.*) You are, I believe, a civil engineer?—I am; I was formerly with Mr. James Walker, of Great George Street.

4586. Will you state to the Committee what your experience has been in submarine telegraph engineering?—In 1850 I assisted at the laying of the Dover and Calais first gutta-percha experimental wire. In 1852 I was appointed assistant engineer to the International Telegraph Company, and superintended the testing and manufacture of their cables at Sunderland, about 500 miles in length. I constructed a dock for this purpose, and tested the cables under water—those were, I think, the first cables tested under water. I then assisted at the laying of the first three Hague cables, and I had the entire superintendence of laying the fourth cable and the shore ends; I laid the cables across the Irish channel from Holyhead to Howth, and several minor cables across the Firth of Forth, the Tay, the Solent, and the Humber. For about four years I had the sole charge of the maintenance and repair of all these cables, and I designed the machinery, the system of buoys, &c., and organized the whole plan by which the repairs were successfully carried out by me. In 1857 I was employed on the Atlantic cable in the "Agamemnon," I afterwards fitted up the "Leipsig" to recover some of the cable. I assisted Mr. Newall to complete the Bona cable off Cape Spartivento, which had run short, and I assisted at the laying of the Cagliari and Malta and Corfu cables in the same year. In 1858 I assisted at the lifting and repairing of a portion of the Bona cable; I also assisted to recover Mr. Brett's cables, which were lost in 1855 and 1856. In 1859 I repaired the Dover and Calais cable. I repaired the Cagliari and Malta cable for the Mediterranean Extension Telegraph Company in the same year. I tested and made a report on the Atlantic telegraph cable for the Atlantic Telegraph Company. I laid the Isle of Man cable for Messrs. Glass and Elliott, and afterwards proceeded to Kurrachee, to test the last sections of the Red Sea cable for the Red Sea Telegraph Company.

4587. Will you give the Committee some particulars of the means which you adopted for the repair of the Hague cables?—The first time that a breakdown took place was in 1853; the cable was cut in two on the Dutch coast. I went over in a tug, grappled up the cable, under-ran it, and made the final splice. I think that was the first time that a cable was ever repaired. The cables were sometimes broken by ships' anchors at 10 miles and 20 miles from the land, and in these cases I continued the system of under-running the cables by means of an under-running sheave hung at the bow of the tug, passing the cable over it, and under-running to the broken end; but eventually the cables got broken at a distance of 50 or 60 miles from the land, and to have under-run that distance would have been very difficult—so I adopted the plan of winding the cable into the ship by means of steam machinery, and as it was very probable that bad weather would come on, and that we should have to cut and leave the cable or else, perhaps, break it, I organized a system of buoys so as to enable me to leave the work altogether, if necessary, and come back to it, after an absence of three or four weeks. The cables were thus wound into the ship until the broken end was arrived at, when buoys were laid down, and the other end dredged up, the cable on board was then spliced on and relaid. These arrangements were perfectly successful, and the cables have been repaired in that way, at distances of 50 or 60 miles from land, and the system is still continued. Indeed one of the two plans thus first carried out by me has been since adopted in every case where a cable has been repaired.

4588. What was the system of buoys?—Applying two nun buoys for buoying the end of the cable, and mooring flag buoys on each side, so as to be able to find the place again easily. Each nun buoy is moored by a chain to a mushroom anchor. This anchor is connected by a chain to the telegraph cable. One buoy is thus moored and attached about 300 yards from the end of the cable, and the other at the end itself. The buoys thus ride by the anchors, and no strain comes on the telegraph cable, which lays slack and quiescent on the ground. By thus having two buoys, quite separate and independent of each other, the security of the end is increased, as one buoy may be sunk by a ship, or break adrift, without causing the total loss of the end. The flag buoys are not well adapted for the actual buoyage of the end, because they are not so easily launched or unmoored, and lifted in rough weather.

4589. What was the reason of those cables breaking down so frequently?—They were much too light in their construction for the anchorage across the North Sea. The fishing vessels anchor sometimes 50 or 60 miles from the coast, and they thus frequently broke the cables in their endeavour to lift the cables to the surface to clear them from their anchors. The cables being laid perfectly tight on the ground before

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they can be lifted to the surface, in 20 or 30 fathoms water, are necessarily subjected to a great strain.

4590. Do you think that they at all purposely destroy the cable?—I have seen two or three cases where the cable has been cut, but it is possible that this has been done as a ready way of clearing the cables from their anchors or possibly from their nets: In general, however, the cable has been broken, and I think this has been done, where it is far at sea, in the attempt to lift the cable to the surface, because it is principally the fishing vessels that anchor far at sea, and they would scarcely be heavy enough to break the cables by riding to them, if they did not attempt to lift them from the ground. Where they are broken near shore, they are no doubt frequently broken by the colliers. The water being shoal, a light vessel like a fishing-boat could lift the cables, and free their anchors without breaking the cables; but if a heavy vessel gets foul of a cable, her weight in a tideway or in a breeze would be sufficient to break the cable the moment she tripped her anchor, when the strain would of course come on the telegraph cable. I am speaking here of the cables when the outer covering is in good condition. In some places, however, the outer wires are much eaten by rust, and at such places a very slight strain would break them. I have seen places, on the rough ground, where the outer covering was completely eaten through for a few inches, whilst a few yards off from the same spot the cable was quite sound and strong. If even a fishing vessel drifted, with her anchor down, foul of the cable near such a spot, she would certainly break it even without attempting to lift it from the ground.

4591. Do you prefer the single or compound cable system?—The single, but the cables must be made much heavier than the Hague cables, for such water; then if they are laid separately, several miles apart, and marked so that you can go and repair one, without interfering with the others, I believe that to be the best system. Because in a compound cable, with four or more wires, there is greater risk in laying to begin with; for if you have one wire which goes bad in paying out by a fault of insulation, you must either cut your cable altogether, and thus cut three good wires through, or you must leave the bad wire in the cable; whereas in the single cables you lay one wire at a time, and anything which happens to that does not affect the others. The same thing applies to repairing; a ship breaks one of these cables, and the rest are available for the transmission of signals, whereas with a compound cable the breakage would cause total interruption to the communication. Of late they have imagined that these cables can be made by adding a little more iron to the outer covering, sufficiently stout to resist ships' anchors altogether; but it is seen that in the Dover and Calais, and Ostend cables, and the Zandvort cable, that this does not effect the desired object; all these cables have been in their turn broken, and every time that they are broken the traffic is interrupted altogether. I therefore consider that in all cases single cables are better, but of course you must make them heavy enough.

4592. You say that you repaired the cable between Cagliari and Malta in February 1859. Will you give the Committee some particulars of that operation?—I went out to Malta for the Mediterranean Extension Company, and met Mr. Newall's ship the "Elba" at Malta. I found that the company's superintendent had already gone to work, and, imagining the fault to be near Malta had separated the cable about a half a mile from the shore. I went out in a tug and spliced this up, and then made my electrical tests. I made out the fault to be 188 nautical miles from Malta, and I immediately wrote a note to the directors, so as to show them that at that time that was the distance which I made the fault to be by my electrical test. I then went out in the Elba, and grappled up the cable off the coast of Sicily, cut it, and buoyed the end leading to Malta; I then began picking up towards the fault. I gradually got in 20 miles of cable, and then the cable suddenly parted at the bottom, there evidently being some heavy weight on it. We were then in 160 fathoms water. I laid down buoys and commenced grappling for the cable. I grappled up the cable after two or three days, about a mile and a half Cagliari side of the obstruction, but there being a slight breeze on there was a good strain on it, and I was obliged to cut away the part which led towards the obstruction, in order to save the end which went towards Cagliari; there was a danger of breaking the cable on the Cagliari side of us, and I was obliged to cut away the part which led to the obstruction. I got the end on board and tested towards Cagliari, and found that portion perfect. I spliced on some thick cable, and paid it out from the fore part of the ship, because we were in very deep water, and I was afraid of going through the operation of changing the cable from the fore part of the ship to the stern, which is a very delicate operation, and I wanted to get into shoal water again, to lay to for daylight, for it was then dark. I paid out three or four miles of cable and laid to until the morning. About four o'clock in the morning the cable parted at the bottom again—it was jammed in some manner at the bottom as if in some crevice—there was a little sea on, and the lift of the ship appeared to cause it to part close to the bottom again. I had then to go to Malta and get some fresh ropes and grapnels, and come back again. I then grappled the thick cable I had spliced on up again, and spliced the cable I had on board on to it, and paid out towards the buoys on the end leading to Malta, which I had left a fortnight before. I then made the final splice, having been three weeks about the whole work. The fault was about 175 miles from Malta. I was thus about 13 miles out in my tests, but as I could only test from one end, of course I could not get as accurate a result as if the tests had been taken both from Cagliari and Malta. I believe that that is about the greatest distance that a fault has ever been prophesied at, and verified by the cable being repaired.

4593. Did not the cable fail again?—Yes, it failed about six weeks afterwards suddenly.

4594. What further steps were taken, and what was their result?—The "Lady Seale" was sent out by the Mediterranean Extension Company, and their superintendent at Malta, Mr. Andrews, was in charge of the attempts to repair the cable for about five months, but they eventually failed, and lost the end in 300 fathoms.

4595. An opinion has been expressed before this Committee that the second failure was owing to the first repairs having been imperfectly executed. Do you think that this second fault existed at that part of the cable where you repaired it, and if so do you think that it could have been occasioned by an imperfect splice?—Certainly not; I think that the state of the case seems to show that it was not at that spot, because the fact of their picking up in 300 fathoms shows that they had got past the point where I was at work on the cable, besides which I have never known a splice fail in any way before. I have made, I suppose, some hundreds of splices at sea, and I have never known a splice which was not as firm and strong as any other part of the cable. The fact of the cable failing suddenly shows that it was through some mechanical force being applied to it. To form, therefore, the opinion that the second fault occurred through an imperfect splice, it must first be *supposed* that the fault occurred at the splice; secondly, that the splice was weaker than the rest of the cable; and, thirdly, that the force applied was less than could break the cable in its usual state. All these are mere *suppositions*, and are unsupported by any evidence whatever, and, consequently, whoever expressed the opinion must, I think, be rather reckless in forming opinions.

4596. What was the nature of the bottom at the part where you picked up the cable?—It appeared to me from the grapnels to be mud.

4597. And yet you think that there were deep cre-

vices existing?—I was alluding to where I grappled the cable up the first time on the Cagliari side of the obstruction. The place where the cable got jammed and broken at the bottom was two or three miles from where I grappled up the cable the first time. I do not, of course, know for certain whether rocks with crevices exist. I only know that the cable got jammed suddenly, and broke short off at the bottom, and that the ground was rough. This was also about the spot where the original obstruction existed.

4598. Have you any evidence by soundings of the existence of crevices?—No, I have not. We frequently got casts of the lead during the work, but always got about 160 fathoms; but in grappling at that spot where I mention that I imagine there may have been crevices, we continually lost grapnels. I have no doubt that that spot was rough, but the part to which I alluded first as being muddy was further out and nearer to Cagliari, and there the grapnels never appeared to come across any foul ground.

4599. Did you form any opinion as to whether there was a probability of volcanic action taking place at the bottom of the sea at that part?—Nothing would lead me to suppose that there was such a thing.

4600. It was off the Adventure Bank, was it not?—It was on the very extreme edge of the Adventure Bank—you could hardly call it on the bank itself. I can show you the position on the chart.

4601. (*The witness pointed out the same*). The further failure must, I presume, have taken place nearer to Cagliari, in the deep water?—Yes; I imagine so. In fact it seems to me very probable that it was not near the place of the first fault at all, because the company's superintendent, when testing for the first fault, made out the fault to be close to Malta, and it turned out to be 175 miles from Malta. He went to work close to Malta, and the fault was 175 miles off, so that the second time it is just as possible that his tests for the second fault might be as much out. If the fault was at the same spot and at the very splices where I repaired it, it seems strange that after five months' work it was found impossible to repair it.

4602. Has it not been reported that the water near the spot where the faults occurred has greatly increased in depth?—I have seen that reported from Mr. Andrews in one of the blue books, but I think that the value of such statements depends a good deal upon the authority on which they are made; and I do not think that Mr. Andrews was sufficiently acquainted with surveying instruments to determine his position so accurately at sea as to say whether he was a mile one side of the place or the other. The ground is very steep there.

4603. You say that you assisted at the recovery of the two cables laid and lost by the Mediterranean Company off Cape Spartivento in 1855 and 1856, and also at the repairs to the Bona cable in 1858. Are there any matters in connexion with those works which you think would be of interest to the Committee?—The repairs to the Bona cable in 1858 present some points of interest. We commenced picking up the cable from Cape Spartivento, and picked up 30¼ miles; this brought us into a depth, by the chart, of about 700 fathoms. We hung on to the cable at this spot for three days and nights in rather a fresh breeze.

4604. Was Mr. Brett at that operation?—No; this was some repairs to the Bona cable, made by Messrs. Newall and Company, for the Mediterranean Telegraph Company. We wound in the cable until we were in about 700 fathoms, and we then paid out the cable to the shore again. I think that that is the only case where such deep water has been reached and the cable successfully relaid. Then, again, on the recovery of Mr. Brett's cables, we picked up the whole of the cable of 1855. This was a heavy cable weighing about eight tons to the mile. It was laid in two pieces, each extending from the shore for about 30 miles into a depth of about 600 or 700 fathoms, where there had been a run during the paying out, and the cable had been cut. One of these pieces terminated in a huge entanglement of about a mile of cable. The cable of 1856 was also laid in two pieces, but the end of one of those sections was 17 miles from the land at the nearest end, and the only means of ascertaining where it was, was by the splice in the first piece. This cable weighed about five tons to the mile. When we came to the splice in the first of these two pieces, we laid down buoys, and went on picking up, and then came back to the place, and grappled up the end of the second piece in 100 fathoms of water, and we recovered a considerable portion of both those pieces. I do not know whether I have made myself clear on that matter. When the cable was laid in 1856, they laid out a portion, and then had a run and had to cut. They then began to under-run from the shore, but long before they got to the end they cut the cable and let go the sea piece, and spliced on the part leading to the shore to what they had on board, and then began paying out afresh. The spot where they did this was 17 miles from the shore. Therefore, in picking up the first piece, when we came to that splice, which we looked out very sharply for, we laid down buoys, and returned to this, and grappled up the isolated piece which they had let go in 1856.

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4605. And recovered it?—We did not recover it all. In both those pieces of 1856 the kinks kept coming in oftener and oftener, and each time, as we got into deep water, one wire would go, and then when the next kink came in, two wires would go, and then three, and at last the cable went altogether at a kink inboard.

4606. Did you find generally many kinks in the cables which you picked up?—In that laid by Mr. Brett a great many.

4607. And in the Mediterranean cables?—Not in the Bona cable which was laid by Mr. Newall himself—it was laid very taut indeed at the end, and when we picked it up there were hardly any kinks in it, except just at the spot where we made our splice in 1857, when we completed the cable about 15 miles out; but in the deep water there were no kinks at all, and the cable was relaid without having to cut out any kinks but those near the splice.

4608. When the cables were laid slackly were there a large number of kinks?—Generally when the cable is very slack kinks appear when the cable is picked up, but sometimes where it is laid quite tight, kinks appear on picking up. These occur during the process of picking up. (By "picking up" I mean winding into the ship, not grappling up the cable). Sometimes in the North Sea, in picking up a cable, where the cable is buried in the ground in little ridges, as it is torn out of those ridges, although it is lying perfectly tight on the ground, it flies into a kink; every time that we feel it break out of the ground, the next minute up comes a kink, although we know that it cannot have existed in the cable when it was laid down, because it was shoal water, and of course the cable was laid quite tight. I think there are three ways in which kinks may occur. Firstly, before leaving the ship, through inefficient means for uncoiling; these need never take place, and could not take place without detection. Secondly, when the cable is laid with too great allowance of slack, the cable, I have little doubt, deposits itself on the ground in a series of half hitches; in picking up, these are liable to be drawn into kinks. This is still more likely to occur when the picking-up is commenced from the end that is first paid out, because the part first drawn at in each hitch would be the under part. Thirdly, the kinks occur in the way I have mentioned, when a bight suddenly breaks out of the ground. No kinks are likely to exist, therefore, in a cable laid with ordinary care until it is lifted.

4609. Have you ever had occasion to abandon altogether the repair of a cable on which you have been engaged?—No; I have been frequently driven

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away by bad weather, but I have always left the ends buoyed or buoys near the end, and returned to the work, and completed it.

4610. Have you experienced much difficulty in ascertaining by your electrical tests the position of the faults?—I have generally been within five or six per cent. in the distance.

4611. What system do you adopt?—At first I measured the resistance by the deflection given on a horizontal galvanometer with one pair of plates, having previously tabulated the deflections given by each additional 10 miles of cable during the manufacture of the Hague cable. Afterwards I used resistance coils and a differential galvanometer, and of late I have seen and used a very beautiful modification of Professor Wheatstone's parallelogram or differential resistance measure introduced by Mr. Siemens. The resistance tests are not the only ones, however, for these must be checked by the return current.

4612. Have you had any resistance coils made for the cable before it was laid; made during the manufacture, so as to be adapted to the cable?—No; I have always had to find my standard from portions of the cable which I have had to test, because they have been different cables and different sections, and copper of varying conductivity; and as I have frequently to test different cables with which I have had nothing to do during manufacture, it would be difficult and expensive to possess resistance coils made expressly for these cables.

4613. In constructing a cable, do you think it desirable that there should be such coils constructed at the same time?—It merely simplifies the figures, nothing more; there is no other advantage in it.

4614. It facilitates practically the finding of faults, I presume?—It saves you about five minutes' calculation, that is all, I think. It saves your multiplying by some figure, 2·1 or 2·5, or whatever may be the proportion which a unit of your resistance coils bears to a mile (or any other unit of length) of the cable you are about to test. This must be found by careful experiment on some known length of the cable. Of course during manufacture you have the best possible means of obtaining this information. It is impossible, however, to rely on the distance of the fault by the mere test for resistance, when testing only from one end, and when the cable is severed. I always check such tests by the return current. For supposing, for instance, the cable is broken only a few yards from the shore, and that from the end being drawn into the gutta-percha, it offers an immense resistance, say 200 miles. Then the resistance would show 200 miles; but if we apply the return current test, we see that there cannot be anything like 200 miles of wire subject to induction in circuit, and you would know that the resistance was not caused by mere length of copper wire subjected to the necessary conditions for inductive action. I have known several cases where the broken end of the copper wire has been perfectly insulated by the gutta-percha being drawn over it. In such a case the deflections on a galvanometer, caused by the charging and discharging of the cable, as compared with the result of the same operation on known lengths of cable of the same pattern, gives a tolerably accurate measure of the distance, and I have prophesied the distance of several such faults in that way.

4615. Is the system of buoys to which you have alluded an essential precaution in laying and repairing cables?—Yes, I think it is; and I think that in several cases of cables which have been lost, they would have been saved if it had been adopted. I take some little credit to myself for bringing this system in, for I was the first to use it to any great extent on the Hague cables, and I afterwards bought buoys, ropes, chains, and anchors, and all the gear for buoying, for Mr. Newall. When we laid the Malta and Cagliari cable there were no buoys at all on board; Mr. Newall now always uses them. The other day there was a case off Muscat; a fault occurred on board, and the old story of laying down buoys and picking up and using them had to be gone through; in fact, in the way that I have used them on several occasions. I also arranged the buoys for the Atlantic expedition, and indeed buoys made exactly similar to my design are now generally used on all telegraphic expeditions.

4616. If I understand you, you attach a buoy to the cable, and then you lay buoys round it to mark the position of the splice?—Yes; but the actual way of applying it to the cable is not exactly attaching it to the cable itself. I attach a chain to the cable of say 10 or 12 fathoms, and this again I attach to a mush-room anchor, or a common kedge anchor; from that a chain leads up to the buoy. Then, again, in deep water, I employ rope (Manilla generally), and a chain near the bottom and near the buoy. There are several details in the way of bridles and apparatus for getting the buoys quickly in, in foul weather. Because if you merely pitch them overboard and lay them down, you cannot always easily get them up again by your boat in rough weather, and they are of no use. There are also several other expedients which facilitate the buoyage of the cable. There are also flag buoys.

4617. What are the flag buoys?—The flag buoys are a modification of the buoys used in the navy for surveying. I took the idea from them, but made some improvements in them. A mast goes through the buoy, and this carries a flagstaff on it. At the bottom of this mast there is a pig of ballast, which balances it and keeps it always upright. I believe it was first introduced into the navy by Captain Belcher. I may mention that I had two years' experience in nautical surveying under Admiral Bullock, during which time I had a good deal to do with fixing position at sea, sounding, buoys and boats, and steering across tide ways, &c., which I have found very useful in cable work.

4618. The object being to attract your attention to the position of the buoy?—Yes; the flagstaff is about 18 feet high.

4619. What is the greatest depth in which you have grappled up a cable and lifted it to the surface?—160 fathoms; that was in the case of the Malta and Cagliari cable. Three times I grappled it up in that depth.

4620. Did you find much difficulty in grappling it in that depth?—I found difficulty; it took time to do it, and great care, going very slowly; and great care was requisite also in handling the ship, so as to be sure to dredge exactly at right angles to the cable, and also in feeling the instant the grapnels took the cable, so as to ease away the grapnel rope, and get the ship eased up to the cable so as to avoid all unnecessary strain. The difficulty is increased when the cable is laid tight, because then it will not bear lifting to the surface in deep water, on the bight, without danger of breaking. The difficulty depends also greatly on the nature of the cable. With a strong and heavy cable you can not only feel the cable more easily, but you can handle it, in heaving it up, with less fear of breaking it, particularly when there is any sea, wind, or tide. This cable being so very light and tight added to the difficulty.

4621. If you did not dredge at right angles to the cable what would happen?—You would be very likely to slip along it and slip off the end, if it was for an end that you were dredging. That is about the greatest depth, I think, at which a cable has been dredged up. I do not know what they have done in the Red Sea, but I know of no other case where a cable has been grappled and lifted in anything near that depth.

4622. What is the greatest depth at which you think a cable could be destroyed wilfully by grapnels?—I do not see any great difficulty in destroying a cable at 300 or 400 fathoms depth. The depth of water does not add to the difficulty very much in the way of destroying it. It adds greatly to the difficulty of lifting it to the surface, particularly when the cable is laid tight on the ground, but it does not add

much difficulty to the fact of catching hold of it with a grapnel. A few inches of sand over a cable or a rough bottom adds a great deal more to the difficulty of grappling it, than 100 or 200 fathoms of water. Where there is sand over the cable, the grapnels will not reach it, and where the ground is very rough you lose your grapnels, but in the deep water all you require is to use proper ropes and a chain at the bottom, to keep the grapnel well down on the ground, and handle the ship and rope carefully, and I hardly see any limit to the depth at which you could catch hold of the cable, provided it lay on the surface, and the ground were not very rough. If you have only a rope down, the grapnel is liable to jump off the ground; when you have a piece of chain near the bottom it keeps the grapnel well down on the ground, and is thus more likely to hook the cable.

4623. Then you would not consider a cable safe in perhaps 500 or 600 fathoms against grappling for purposes of destruction?—I should think it perfectly possible to destroy it at that depth. I should not like to say what is the limit, but I certainly think that if a cable was lying on the surface of the ground, I could get hold of it in 400 fathoms.

4624. (*Mr. Bidder.*) If you knew where it was or near where it was?—If I knew the direction of it if the ground was smooth. If you do not know the position of it, it is only a question of the time of your dredges. It is a question of time and patience.

4625. (*Chairman.*) If you know that a cable passed between two places, you think that you would be certain to meet with it at that depth?—I will not say certain, because sometimes when you are grappling, you go over the cable two or three times without catching it, but I think with perseverance, unless it was buried, you would be sure to get it. If you go the least bit too fast, the grapnels are liable to jump off the ground; if there are stones or any roughness, the grapnel is liable to jump over the cable, but with perseverance you are pretty sure to get hold of it, unless it is buried. Where people have failed in grappling up in deep water, the difficulty has been that they have wanted to get the cable up, and when it has been a very light cable, they may possibly have broken it and dredged right through the cable without knowing it at all. That, as far as wilful damage goes, would answer the purpose. You do not want to know whether you have broken the cable or not, I suppose, as long as you do break it; except perhaps in order to know when to leave off.

4626. What is your opinion of the general cause of the failures in submarine telegraph undertakings?—I do not think that they have had a fair chance. I think that if you look at the root of the evil, you will find that they have been very loosely managed. Professional experience has been entirely ignored in the matter. If you look at five of the later companies, the large submarine companies, you will find that three of them, the Mediterranean Company, the Mediterranean Extension Company, and the Channel Islands Company, had no engineers at all. Then again the Atlantic Company certainly had an engineer, but he had had no practical experience in submarine telegraph engineering. Mr. Bright had had no experience in submarine cables when he was appointed engineer. No doubt he may have been very clever in those matters in which he had had experience, but still, he had had no experience in submarine cables. The same observation applies, I think, to Mr. Gisborne. He was appointed engineer to the Red Sea Company, when he had had, as far as I know (and I have followed up the thing for the last 10 years), no experience in the matter whatever. Therefore I think that none of those undertakings have had as fair a chance as they might have had, considering the fact that many engineers have for several years made submarine telegraphy their profession, and gained considerable experience and knowledge on the subject. It seems to me as if the directors of these later companies were determined to give their undertakings the very minimum chance of success. I think the interest of the shareholders (which is evidently the possession of a permanent telegraph) has not been sufficiently represented in the practical administration of their work. The concessionaire of a company is scarcely interested in the permanent welfare of the company; his interest is to see a company formed and apparently fairly started and then get paid for his concession. His heavy interest in the rapid starting of a company would thus be apt to lead him to ignore any difficulties in the permanent maintenance of the work, even if he was sufficiently experienced in the matter to perceive them. His interest in the permanence of the work is very slight, whereas he has a heavy pecuniary interest in the actual existence of a company, be it ever so temporary; whilst by taking on himself the office of engineer he may gain great experience, and perhaps professional credit by the first momentary success of the work. The contractor in the same manner is naturally very much interested in the starting of a company in order that he may obtain a large and profitable contract; but as he has only to maintain the work for a week or a month, he has no pecuniary interest in the permanence of the work. Both these are therefore interested in promulgating the theory that there is no difficulty in the maintenance of their schemes and works, and that the whole difficulty lies in the first act of laying the cable. Yet up to the present time these matters have been nearly entirely entrusted to, and managed by, the concessionaire of the company (quite regardless of his experience in the matter), or else the contractors, or perhaps by both. I think this has led to these temporary works. I consider that in all the cases of the late submarine companies, the interest of the shareholders has been carelessly sacrificed for the immediate benefit of the concessionaire or contractor, who have been the sole parties benefited by the works, of which they have had practically the whole management. The success of a telegraph company depends on the permanence of the work, and not on the mere feat of paying out so many hundreds of miles of slender cable which shall transmit so many words a minute at the end of a month or week. The main care, therefore, should be that the engineer of the company should see his way clear to repair and maintain the work in permanent working order, where repair is possible, and to see that the proportions and length of spans of the cable are perfect where the water is so deep as not to admit of repair before the company commit themselves to a hasty contract for the paying out of immense lengths of scantily proportioned cables. Engineers experienced in the maintenance of these kind of works are evidently the most competent to deal with this question. There are many details in the matter besides the general proportions of the cables, the length of spans, selection of route, number of wires necessary, &c., which experience alone can point to, and which, when properly taken hold of, add greatly to the durability and security of the work. But when a submarine telegraph is merely made a question of paying out so many hundreds of miles of slender cable, in enormous spans, which shall last for a week or a month, without any precautions for maintenance, without anybody seeing, or caring to see, their way clear to maintain them, nothing but the most temporary work can result. The question of maintenance is in fact dropped altogether. The whole policy is founded on the principle that when once the cable is down, it will last for ever, never mind what is the locality, nor what is the depth of water or size of the cable. There is a quotation I have here (from page 92 of the first Blue-book on telegraphs) which seems to be a type of the principle that is acted on in these matters. Mr. Fowle, in writing to the President of the India Board, says,—"The estimate "of making the Euphrates line is 222*l.* per mile. "That of maintaining it is perhaps wisely omitted. "The estimate of the Red Sea line is about 120*l.* a "mile, and experience has proved that a good cable, "once laid successfully, requires no maintenance."

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Now I think that if the maintenance of the Red Sea lines, as at present constructed, were mentioned, it would be found to be an enormous item, even if the line, as at present constructed, can be maintained at all. The cost of maintaining a land line, through the wildest country, could not approach to the expenses which must be incurred to keep the present Red Sea cable in repair. The primary cost in the above quotations appear to be also wrongly stated. To suppose any long land line (which, with one wire, would cost in England about 30*l.* per mile) could ever reach 222*l.* per mile is preposterous, whilst it is well known that the Red Sea cable has cost nearly 250*l.* per mile instead of 120*l.* Had the maintenance of the work, instead of being thus ignored, been at first carefully considered by telegraph engineers experienced in the maintenance of these works, no doubt a work would have been constructed which might have been maintained at a reasonable yearly expense. Submarine telegraph directors seem, however, to have a chronic distaste for practical experience in these matters, and (in spite of each successive failure) proceed on the principle that their only duty is to enter into a brief and hasty contract for the laying of so many hundreds of miles of cable which shall last a few weeks. That when this is accomplished all their difficulties are over, and that as this first and only risk is taken by the contractor, there is no necessity for an engineer's experience in these matters. This system is unfair to shareholders and discouraging to those telegraph engineers who have been for years steadily learning their profession in the details of these works, and who now find that submarine telegraph companies, rather than avail themselves of the professional experience thus accumulated, prefer to gain their experience directly for themselves, though at the cost of the whole capital of their undertaking. The most serious consequence of this reckless policy is, however, the threatened extinction of all telegraphic extension whatever.

4627. (*Mr. Bidder.*) Do you think that that would be corrected if a clause were added to any contract for laying a cable, that the maintenance should be extended, say, for six or twelve months?—I think that it would, to some extent, but I do not think you would get the contractors at present to undertake it.

4628. If the contractors with all the experience which they have, are afraid to undertake the laying of a cable, making any terms whatever, does not it strike you that that shows that submarine cables are of very uncertain operation at the present moment?—No; I do not think that that follows. As long as the contractors can get companies to believe that the whole risk is in the laying they will not care to undertake the maintenance. Yet I have no doubt that if the maintenance is gravely considered by those competent to see the necessities of the case, the security and facilities for maintenance of the work could be guaranteed. I do not say in a case like that of the Atlantic cable, but in many, if the constant and main danger were looked in the face, that cables do require repair and require maintenance, it would be perfectly possible to take such steps that a permanent telegraph might be constructed on many of the lines. I do not say on all. I do not say on the Atlantic, there it is a question of neck or nothing. But even in the case of the Atlantic, the probability of success was reduced to a minimum by the reckless management; but there are many lines, taking the Red Sea for instance, where there is no excuse for that neck or nothing policy. The Red Sea line need not and ought not to have been treated as a deep water telegraph.

4629. Does not it occur to you that if we ever attempt the position, that we can lay a cable so perfect that there is a fair chance of its remaining for a long period, in the meantime, unless you put upon the contractor some liability beyond the mere fact of sending a certain number of words per minute, you do not get a sufficient inducement for him to be very careful in his manufacture, and to apply all these tests which shall discover defects?—I can hardly say to what extent that would benefit the actual manufacture of the cable. Unless the maintenance is for several years, and unless the whole project is put into the hands of the contractor, I think that it would not benefit it much. I believe that, as a general rule, they do take fair precautions, so far as the quality of the materials and the manufatures are concerned, where they are responsible for the laying of the cable. But it is to be remembered that the permanence of the work does not by any means depend on these points alone. The proportion of the several parts of the cable, the manner in which it is laid, and the route it takes, the length of spans, and numerous other points involved in the general plan of the undertaking, are matters of at least equal importance with the mere construction of the cable as regards the permanence of the work, and these are the points which should be carefully attended to by the engineers of the company.

4630. If the contractor was liable for maintenance for any fair period of time, do you not think that the selection of the cable, and the mode of construction, would be tolerably safe in his hands, provided you specified the conducting power, that is to say the capacity of the wires?—Certainly, if you give him the maintenance for a term of years, say 25 or 50 years, and bind him in heavy penalties, and leave the whole thing to him, then, I think, you are quite justified in scratching out engineers altogether, rather than have the maintenance totally neglected, as it is on most works. But when you only give the contractor the maintenance for a week or a month, and leave the whole thing entirely in his hands, I say that then it is liable to lead to these temporary works. I do not see, however, why the maintenance cannot be carried out by the company if they will deal fairly with the matter and employ a proper staff, and take all the necessary precautions from the commencement for the permanent maintenance of the work.

4631. That was not the object of my question—it went beyond that. In point of fact the liability for a week amounts to nothing, it is merely to get the thing down for a week, and to make it carry a certain amount of messages, and after the week there is no care of what becomes of it. I admit that the contractor ought to be paid properly for the maintenance, if it was for years I should prefer it, and you would insure a more perfect cable, and a more careful process of laying it than is now adopted, by the system of buoys, and everything else as to its being repaired?—How many years would you say?

4632. As long a period as possible—that is a mere question of detail?—I certainly think that there is some period of maintenance which would insure every thing being done which is possible to be done in the matter, but I do not think six months or twelve months sufficient.

4633. Would you recommend that?—If it is supposed that companies will not take any steps for maintenance themselves, I should think that about as good a bargain as a company could get if it was possible to get it. If the contract is for 25 or 50 years—mind, I lay great stress on the time. But at the same time I cannot see why a company cannot, if they take the proper means, maintain a telegraph as well themselves as by contract, particularly on a very large work, where the magnitude of the work is sufficient to keep a large staff and ship in constant work.

4634. The question is not one of bargain, but the question is above all things for a great national object, to get a cable upon which the country can rely. When we are spending £800,000 on a cable the question of a few thousands here or there is nothing as compared with the importance of keeping that cable in efficient use. You are probably aware that the Inter-National Company have made such an arrangement (not for a long period) with Messrs. Glass and Elliott?—I was not aware of that.

4635. (*Chairman.*) Will you enumerate what in your opinion were the immediate causes of each particular failure?—The Mediterranean cable, laid off

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Cape Spartivento in 1855 by Mr. Brett, I think, was about the first great failure. That, I think, was due simply to an insufficient mechanical knowledge on the part of those who superintended it.

4636. Do you mean on the part of those who superintended the laying, or who superintended the repairs?—I mean simply the laying. That first cable was lost during the laying; it was a very heavy cable. It was, perhaps, the first experiment in deep water, and it was a heavy cable placed in a sailing vessel in tow of a steamer, which is always bad, because you cannot handle a vessel in tow so well as you can a steamer. Then, again, the break was insufficient; there were not sufficient turns round the break, and the cable ran away, and they had to cut it. Then they started again from the shore, and the same thing occurred again. There was rather a curious thing there, too, that in picking up these portions which we did in 1858, the second piece, added to what we know was saved, and is now lying in the East India Docks, is not sufficient to reach across to the African coast, so that they actually must have started with less cable than the actual distance across, independently of any slack in paying out. Then, in the next year, 1856, very much the same sort of thing occurred again—a run. Then they started again, and ran short off Galita. Then they had no buoys on board, and they had to lose the end altogether; that was in 300 fathoms. If they had had buoys, there is no doubt that there would have been no difficulty in buoying the cable in 300 fathoms with hemp, and they could have left it for a couple of months or more, and come back and spliced the cable on, and finished it. With regard to the next failure, the Bona cable, the insulation on some of the wires has always been defective. There the cable was successfully laid, it ran short, but we completed it—the cable was successfully laid, but the design of the cable was out of all proportion to the distance: it was No. 4 or 3½ gutta-percha, something very small indeed, and it was such a covering of gutta-percha as an ordinary telegraph engineer would not have thought of laying on such a long and important line through deep water.

4637. Who designed that cable?—I believe the concessionaire of the Company, Mr. Brett; I have always heard so. Then again there was the Atlantic cable; that certainly was a novel case; but still many of the steps which were taken were those which people who had experience would not have taken. For instance, in the first year the cable failed through the break; the break was totally new.

4638. Were you with the Atlantic cable on its laying?—I was on board the "Agamemnon."

4639. The first year, or on her return?—The first year, only, 1857. I was engaged simply to assist at the coiling on board and paying out of the "Agamemnon's" portion. I had nothing to do with the design of the cable or with the insulation, which was intrusted to Mr. Whitehouse. The breakage which took place on board the "Niagara" was through the break. The break was a totally novel affair, designed by Mr. Bright and Mr. De Bergue, neither of whom had ever laid a mile of cable themselves, and although Mr. Bright had three engineers under him, Mr. Canning, Mr. Woodhouse, and myself, all experienced in submarine telegraphy, we were not consulted about the break at all. The break was designed and patented, and we were told that that was what it was to be. That break no doubt caused the first breakage. In endeavouring to overcome imaginary objections to the old system of break, the designers produced a machine full of real dangers and objections. Then great discussions took place about breaks, and a fresh break was designed by a large body of scientific gentlemen, by which, after several breakages, they succeeded in laying the cable the second year. That break even was much heavier and more complicated than necessary, and I do not think anybody would ever employ anything in the least like it again, for the same kind of cable. Nothing in the least like it has ever been used since, but the same kind of break that existed before the Atlantic has been, with some slight modifications, employed on all the cables that have been laid since. I consider the actual failure of it the second year was due, firstly, to its design, the quantity of copper and gutta-percha being out of all proportion to the length of the cable. Secondly, to the passage of the cable through tanks of hot tar, in which it was left standing during dinner hours, and to its being left uncovered in the sun, by which I believe the insulation to have been injured. Thirdly, to the fact of its not being tested under water, by which these defects would have been discovered. Lastly, the extreme battery power used, which would increase the faults. The shipping, and unshipping, and reshipping of it so often, no doubt, exposed it to additional chances of mechanical injury. I have also had some joints shown to me which are said to have been cut out of a portion of the Atlantic cable, any one of which would of itself be sufficient to cause failure. In fact, the number of chances of faults occurring in it without detection until paid out appear to me, and always did appear to me, so great, and the probability of its being perfect appeared so small, that it has always seemed to me a miracle that it was even possible to get a few messages through the cable at all.

4640. Do you know anything of the Malta and Corfu cable?—I assisted to lay it, but I have had nothing to do with it since.

4641. You are not aware of the reason why it has failed?—I am not aware of the actual reason. Then, again, there is the Malta and Cagliari cable. There is difficulty in saying what the actual cause of that failure is; but there is no doubt that if the cable had been much thicker, it would have been much easier to repair, and there would have been a chance of repairing it.

4642. (*Mr. Bidder.*) What was the weight of that cable?—About 17 cwt. to a mile. Then there is the cable from Alexandria to Candia. Those have all three failed in the laying, and two of them, the first and third, were cases of failure of insulation during the paying out; and the second was a case, I have heard, of the cable actually breaking at the stern of the vessel. There is the Red Sea cable; I could not say exactly what is the immediate cause of the faults there, but I believe the cable to be a great deal too small, both in the proportion of gutta-percha, the proportion of copper wire, and the proportion of iron wire. The spans are also too long. It has been said with truth by Mr. Gisborne, in speaking of the proportions of the Atlantic cable, that it was like entering a Shetland pony for the Derby, and I think that, to use a similar simile, it may fairly be said that the Red Sea cable was like entering a donkey for the Leger.

4643. (*Chairman.*) In what way do you consider that the want of size in the copper and the gutta-percha affects the durability of the cable?—It strains the power of insulation (if I may use the term) of the gutta-percha (taking into account all the slight imperfections which may exist in it) to such an extreme that I do not think it can stand it when there are such enormous lengths. You are obliged to use considerable battery power, and perhaps there is a minute fault in the cable, and it gets gradually worse, and they put on more battery power, and it at last goes altogether. We know that with a given battery power where you increase the resistance you increase the tension throughout the cable, and consequently the tendency for the current to force its way through any slight faults. Then, again, when you have these great lengths, you increase the battery power to overcome that resistance; so that you increase the tension and the tendency of the electricity to force its way through the gutta percha in two ways: first, by the resistance on a given battery power, and, secondly, by the increased battery power which you use to overcome the increased resistance. I am here speaking of the gutta-percha coating taking into account all the

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slight imperfections to which such a long length is liable in manufacture. Of course even this tension would not be sufficient to cause discharge through pure and perfect gutta-percha of the thickness of the Red Sea cable coating.

4644. Were you with any part of the Red Sea telegraph, with the laying or the subsequent operations of testing?—I was sent out by the Company to test the last sections, and to be on board the vessel during the paying out, to inspect the operation, but the contractor refused to let me be on board, and I only accompanied the expedition in the "Retribution." I afterwards returned over the line, and tested the line for insulation at the end of a month after each section was laid.

4645. Were you present at the repair of any faults?—No. I was at Aden when the arrangements for the repair of the line were made, but I had nothing to do with them. The company were not prepared by any precautions of their own for the repair and maintenance of the line, and the whole matter was therefore left in the hands of the contractor. I believe the result has been that the Imperidor was employed for five months, and expended 300 miles of new cable, and the line only lasted good, after repair, four days. The expense of these repairs cannot be less than 60,000*l.* I think this shows how necessary it is to take every step during construction to facilitate the repairs, and to be prepared with everything necessary for the work. For if the cable had been strong enough to be easily handled, laid in shoal water, and in short sections, such expense could not have been incurred, even if the faults had occurred. In fact, this expenditure of 300 miles of new cable on a section 650 miles long amounts to renewal of half a section; it cannot be termed repairing.

4646. In what condition was the line when you tested it?—The insulation on one section, between Kooria Mooria and Aden, I considered fair insulation, and on the other two sections it was good. The insulating qualities of a cable can, however, in my opinion, only be fairly judged of by testing the cable before, during, and immediately after submergence, and then after a lapse of time, and comparing the effect which submergence and working has on the insulation. This information the contractors prevented me from obtaining, and I consequently was only able to give a very qualified opinion on the perfection of the cable; in fact, only an opinion as to the actual state of the insulation at the time I tested it. The section between Kooria Mooria and Aden transmitted about seven words per minute.

4647. What distance was that?—I think it is about 700 miles, but nobody but the contractors know the exact length, as they would not allow the Company's officers to obtain this information.

4648. Is that the longest section?—It is.

4649. At what rate did the other sections transmit words?—Between twelve and fourteen.

4650. Those sections were about 500 or 450 miles, were not they?—About 500 miles. That was the rate at which they actually got the test messages through the cable. But I consider that for practical purposes we might fairly work at about ten words per minute.

4651. Did you test the other sections between Aden and Suez?—No, I had nothing to do with them.

4652. Do you consider it of importance to test a cable under water before it is laid or during manufacture?—Certainly, I do. I advocated it very strongly on the Hague cables, and constructed a dock at Sunderland to test them in. I advocated it for the Atlantic, but it was not adopted; and it is quite evident that that cable went to sea full of faults. Mr. Glass and Mr. Henley now test all their cables in water.

4653. Would you test the cables under water under a pressure equal to that to which they are to be subjected?—No, that is impossible. No doubt, if you could do it, it would be better than under the low pressure which is possible. But even with a low pressure, with a simple covering of water, if they are soaked for many weeks or months, with a continual battery power applied to them, I believe that it is the means of finding out many weak points in the gutta-percha, and certainly it would detect any bad injury. I have heard one or two manufacturers say that they have found several faults by these means, and I have done so, and we know that Mr. Newall, who does not advocate testing under water (for I believe he has never adopted it) has had faults in the cable after it has been shipped. I have always heard that on the Alexandrian line, where there was a hemp cable, the insulation failed while they were paying out. Again, on the third Alexandrian line it did the same. Again, between Muscat and Kooria Mooria a fault occurred of insulation; it was found out shortly after it went over the side into the water, and it could not then have had any great pressure on it. I do not say that a low pressure will find out every fault, but I believe that it will find out a great many. It will find out anything like a stick with a knife or a bad joint, or the effects of heat on the gutta-percha, or any mechanical injury to the insulation. Besides which I think it is safer to keep the cable in water than in air.

4654. If I understaud you rightly you would advocate such an amount of pressure as would tend to find out all faults?—Certainly, if you could apply it—but that is impossible. It would be impossible to construct a closed tank large enough to contain the whole cable, and capable of sustaining a pressure of 6,000 lbs. to the square inch. I am speaking of testing the cable whole. We know that by several processes which are adopted, the cable is tested in sections, under heavy pressure, though nothing like 6,000 lbs.; it is done so by Mr. Reid's process, and I believe that it is tested very accurately in sections; but I speak merely of testing it in the whole, after it has gone through the ordeal of manufacture; and I think that if the Atlantic cable had been tested in that way there would have been every probability of having a much better cable, at any rate a cable with much fewer faults in it. I think it is quite evident the Atlantic cable was full of faults.

4655. Then you do not think that all the precautions which are possible for the permanent maintenance of cables are taken at the present time?—No, certainly not; I think that they are generally entirely neglected, as long as the cable can just be laid down, that is considered sufficient. I am speaking of these later companies; not of those cables laid round England, because those companies have had experience and they do now take every precaution for the maintenance of the cables, but by the later companies there has been no attempt to take any precaution for the maintenance of their lines. Originally the Mediterranean Extension Company did not even know where their cable lay; when the cable broke they had to apply to the contractors for a plan of their line; they did not know where their property lay. Then they have no precaution in the way of ships or machinery, or in testing instruments or spare cable or staff or anything of the sort; they think that when the cable is once down it is to last for ever, and that there the question ends. At the same time the cables of these later companies are of the very lightest description and scanty insulation and conductor, thus inviting failure on works where every precaution should be redoubled. There is no attempt either to keep the cable in shoal water where possible, or to run it into shoal water at certain points, so as to facilitate the maintenance.

4656. (*Mr. Bidder.*) But do you think that whoever lays a cable, whether a Company or the Government, the Company or the Government ought to have a person on board to supervise the whole operation?—No doubt, and not only that; I think it is not only the question of being on board ship, but the whole scheme of the project from beginning to end; I think that the engineer who superintends the project for the line of telegraph must bear in his mind the means which he is going to take to keep it in repair, where

the line will admit of constructing a telegraph that can be repaired, and he must see his way clear to repair and maintain the cable without long interruptions to the traffic. No doubt the paying out of 600 or 700 miles of slender telegraph cable in one section so as to last even for a month is a feat of skill, but the mere performance of a series of such feats has no resemblance to the construction of a permanent line of telegraph. Such work is not engineering, it is simply backing up and paying for the performance of a series of *tours de force* without a reasonable chance of any permanent and practical result.

4657. Do you know why you were not allowed to be on board in the case of the Red Sea telegraph?—I can only trace Mr. Newall's personal objection to my being on board, to the fact of my having said very much what I am saying now in a letter in the money article of the "Times" a year ago. I pointed out the reckless policy that was being pursued in these matters, which could only result in benefit to contractors and concessionaires. I stated then very much what I am saying now, that the steps necessary for the maintenance of the cables were entirely neglected, and that professional advice and experience were ignored, and that as long as that system was carried out there was no hope for telegraphs, and now I believe the great crash has come. Through this system, submarine telegraphs are condemned by the public indiscriminately as dead failures.

4658. (*Chairman.*) Do you consider that if the experience which had been gained up to the commencement of these later works had been brought to bear on the subject, it would have tended to prevent or modify their failure?—Certainly I do. I think that in most of the cases if it had not prevented the failure it would have modified it to a great extent. I think that in the case of the cables lost off Spartivento anybody having a little experience in submarine cables, (and at that time there were several who had had experience,) would have had proper buoys, and all the rest of it—all the proper gear for that work. In the design of the Bona cable, if the experience of any ordinary telegraph engineer who had had any experience of insulation had been applied to to design that cable there is no doubt that it would have been designed with much more copper, and a much greater quantity of gutta-percha. I think, on the Mediterranean extension lines, more substantial cables would have been laid, which would have been easier of repair, and less liable to mechanical injury, and with more copper and gutta-percha. Then, again, in the Atlantic cable, I think that the same course would have been pursued with regard to the proportions of the cable, and the means of testing would also have been looked to more carefully. And besides that I think that the break which had been used before on most cables, and which has been used since in water as deep as the Atlantic with perfect success, and which seems to be, with some slight improvements, very well adapted for the purposes of paying out cables, would have been employed, and that there would have been no mystery about the break at all. In the same way on the Red Sea lines, I have no doubt that if proper persons had been fairly consulted the whole scheme would have been carried out in a totally different but more substantial manner, and in such a manner that it could be maintained.

4659. Have you formed any opinion as to the best mode of covering the cables for deep sea telegraphs, where they are out of the reach of grapnels?—I have not formed any decided opinion as to the very best, but I believe the importance which is laid on the question of the outer covering is greatly overrated. I do not think the failures that have occurred have been due so much to the actual nature of the outer covering as to the proportions of the different parts and to general mismanagement. I do not believe the pattern of the Algerian cable, which is now about to be laid, to be good; I believe that as the wires do not touch each other the core will be liable to strain, particularly when the hemp gets decayed. I can hardly think that the hemp can add any strength to the rope, and certainly I do not believe that the combined strength is greater than the sum of the two taken separately. I do not believe, in fact, that the modulus of tension is greater than with even ordinary cables. The only benefit that can result is a slight decrease in the specific gravity, and consequently of its terminal velocity in water, which will ease the strain a little during paying out, but, as I believe the cable would not stand a strain so nearly approximating to its breaking strain as a common cable would, without injury to the core, I do not think there is any advantage gained. I believe steel wires would be an improvement on the iron, but I think the point for the greatest attention for telegraphic cables generally is in some means of preserving the iron wires from rust.

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4660. Have you any opinion to offer specially bearing on the Red Sea and India telegraph?—Yes, I think that there there is an example of very erroneous engineering, and I think it is the result of the policy I have pointed out. I consider that with a coast line, leading all the way from Suez very nearly up to India, it is perfectly practicable to construct a permanent telegraph, a telegraph which can be maintained for a certain sum per annum, but it must be done on a totally different principle from the present one. It must first be acknowledged that cables do fail from one cause or the other, and that therefore you must take steps to maintain them. I think that the spans on that line are much too long—the cable is much too weak in mechanical strength, and constructed with too little copper and too little gutta-percha—there has been no attempt to keep the cable sufficiently in shoal water, and I consider one wire insufficient.

4661. Do you mean that the cable has been too light for the spans? I presume that with a large span you would have a larger cable?—I certainly would have a larger cable for a large span, but in any case even with smaller spans, I should have a larger cable than that one. Larger and stronger in every way.

4662. Do you limit the spans to any distance?—I am speaking in two ways of the spans—not merely of the span for the electrical working, but of the span, looking at it as a mere question of maintenance. I consider that for the maintenance of the line, the spans should not be in longer sections than 50 miles, where possible.

4663. Does not it in some degree depend upon the amount of copper and gutta-percha in your line? The question of the rapidity of working and the strain on the insulating capabilities of the gutta-percha depends upon the size of the gutta-percha and the copper wire, as compared with the length of the span. But in the question of maintenance, it is this, that there are certain chances of the cable getting out of order—you want then, at once to be able to test it as we do in our street wires, or along the express wires on a railway—you want to be able to go to one of these 50 mile testing points and say, "There is a fault, take up "that section or a portion of it, and put down a new "one." But when you have spans of 700 miles long, if one little fault occurs at any part of the length, the whole of that section is out of order, and it is an affair of some months and frightful expense to repair it, whereas if the cables were in short sections close to the shore, in 40 or 50 fathoms of water, you would at once distinctly find out where the fault was, and repair it quickly and with little expense.

4664. (*Mr. Bidder.*) Then would you bring the ends right on shore?—Yes; I think that there is no absolute objection to that, the only thing to fear is the sun, as far as I can see, and I think that difficulty might be got over. I do not think that there is any great fear of the Arabs; I think that they might be easily disposed of by small subsidies to the Shieks in some parts, and in others by concealing the ends of the cable. Then again I believe that you ought to have two or three cables at the very least for a line like that. I believe that it will be an utter impossibility to keep a single wire in repair—to keep

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every portion of the 3,000 miles perpetually in repair, even if the cable had been more substantial and every other precaution taken; and I think anybody conversant with the maintenance of telegraph cables could have given this information before the work was begun. When you have two or three wires a section in one place may be out of order, and a section in another place on the other line may be out of order; but you will always manage with two or three wires to get one whole wire right through. Then of course you must have one or two ships fitted up with buoys and machinery, with a stock of spare cable, and you must have the cable laid in such depths that you can get at it.

4665. (*Chairman.*) Do you think that the contracts as at present drawn up for sub-marine telegraphs are adapted to meet all the contingencies of the case?—No, certainly not. I have always thought, and I pointed it out a year ago, that nothing is looser than they are. They are partly drawn up by directors and solicitors, who know nothing at all about the practical necessities of the case. For instance it has generally been the case that there is a clause that the payment for the cable shall depend upon the cable transmitting so many words per minute, and there is no mention made of insulation at all. Now we know very well that the question of any rate per minute does not depend on insulation at all. I do not say that you can speak through a cable which has no insulation, but the insulation may be very imperfect, and you are able to speak just as rapidly as if it was quite perfect—because where the insulation is bad, say not at a single spot, but where there is a general defect in it throughout, the effect of induction on the rapidity of working is slightly decreased, for the static charge is quickly discharged through the faulty places, the cable will not hold its charge, and you can actually transmit faster than with a perfect cable—but that cable will not last long; it will very soon go in the same way that the Atlantic cable has gone, and the Bona cable, and others. I consider that that clause is very imperfect altogether; there ought to be a certain state of insulation specified.

4666. Would not a sentence stating that it should be in working order meet the case?—I think that that is very vague indeed; it does not meet the point in the least; it may transmit messages, but the meaning of the words "working order" is, I think, a question which will admit of much dispute.

4667. If the insulation was defective I presume that the engineer would not report that it was in good working order?—He might not, but the contractors would argue that it fulfilled the conditions of their contract, and I think would have a good case in a court of law. I may mention that of the three sections which I tested on the Red Sea line, I consider that the insulation on two was good and one fair. But there was one section in the Red Sea line in the previous year in which there was a slight fault, it did not get any worse, and I believe that section was eventually taken over; but suppose it had got worse and had still transmitted words, it would immediately have been a beautiful case for litigation between the company and the contractor; the contractor would have said, "It is in working order for it will transmit mes-" sages," the company would have said, "It is fail-" ing fast and will very soon fail altogether."

4668. What words would you introduce instead of "good working order"?—I would introduce that it should stand such and such a test of insulation. How I should draw it up at a moment's notice I can hardly say, but I certainly would put in a certain test.

4669. A specified test?—A specified test.

4670. And at the expiration of a specified time?—Yes; I think it would thus be possible to secure as good insulation as the pattern and length of the cable would admit. But it must be borne in mind that even this would simply provide for one quality in the cable, viz., careful insulation. There are, of course, numberless other points to be provided for in a contract which is to specify a perfect and permanent telegraph. Yet a clause stating the cable shall be in working order has been in most contracts almost the sole clause to provide for the quality and nature of the work. Nearly all the other clauses have related to the mode of payment.

4671. But I am assuming that even in any case, even if the contractor is to maintain it for a certain number of months or for a certain number of years, at the expiration of that time you would only be bound to relieve him from it, in case it was capable of standing a certain test of insulation?—Exactly so. Then again the whole arrangements are left entirely in the contractor's hands.

4672. To what case are you alluding now, the Red Sea telegraph?—I was looking at the Red Sea contract, but I am speaking of most of the large contracts of late. The whole matter has been left in the contractor's hands; there has been nobody on board ship in some cases on the part of the company at all. Here is a clause in the Red Sea Company's contract, which seems to me to contradict itself: "The Board " are to have the right of inspecting and testing the " cable and materials during manufacture and while " laying by their own members, and by their officers " and other persons appointed by them in such manner " and at such times as they may think fit." So far it is all well and good, but then it goes on to say, "so as " not to interfere with the progress of the work." Now that contradicts the whole thing, because the contractors can always make that excuse, and they did. When I was sent out by the Red Sea Company, the contractors made that excuse at once; that clause was quoted, that I was not to interfere with the progress of the work, and consequently that I was not to be on board. Then here is another clause in the Red Sea contract; the cable is to be laid for a certain sum, and a part of the payment is to be made at "90*l.* per nautical mile, " to be paid at the end of each calendar month on " the quantity of cable made, according to the terms " contained in Article 2;" then at the end of this clause it says: "The cable as made and paid for " under this agreement is to become the property of " the Company; any surplus cable, remaining after " the final completion of the contract, is to belong to " the parties of the second part," that is the contractors, so that the company actually pay for a certain amount of cable, which is shipped and sent to sea for the work, and any of that cable that is saved after the cable has been laid becomes the property of the contractor, although already paid for by the company nearly in full, and it is, therefore, the contractor's interest to save as much of that cable as he can, consequently he will lay the cable as tight as he can. Yet the officers of the company cannot interfere if they see the cable being laid too tight.

4673. From what you saw, do you think that any harm has accrued to the Red Sea cable from being laid tight?—I do not think, as far as regards the immediate injury to the insulation, that such has occurred, but I believe it always to be objectionable to have it laid so very tight, because in repairing it always increases the difficulty.

4674. (*Mr. Bidder.*) I think that Captain Galton's question was, whether you think that the contractor was operated upon by that clause, and actually did lay it inconveniently tight?—Not having been on board, I cannot say actually how tight it was laid. I have heard opinions expressed by those that were on board on the part of the company that it was laid very tight. At the same time the officers on board on the part of the company were not allowed to ascertain what strain was put on, or what length of cable was paid out. I know that in laying the cable from Kurrachee to Muscat the spokes of the break wheel were broken from the strain put on it, and that there were about 300 miles of surplus cable, which became the property of the contractor, saved between Aden and Kurrachee.

4675. (*Chairman.*) You do not know it from your personal knowledge?—I do not know how tight it was laid from my own personal knowledge. I do not

lay much stress on the fact of its being laid tight, except over rough ground, as far as injury goes. I do not think that such injury can accrue from the great strain on the cable; but I think that great inconvenience in repairing it would result from it; because it stands to reason that if the cable is already tight on the ground, lifting the bight to the surface and under-running it must put an enormous additional strain on, and be very likely to cause faults when it passes over the small sheaves of a boat under such heavy strain, even if it does not break the cable. In such a case a cable should never be under-run, but should be picked up in portions, repaired, and relaid. I pointed all this out in the paper I read at the Institution of Civil Engineers in February 1858.

4676. You do not think that any injury in laying it would result from tightness in laying it?—I believe that they would break the cable before they would strain the gutta-percha. At the same time if the ground were very rough and rocky, if the cable is laid without sufficient slack, it would be suspended in festoons from rock to rock, and I think might be liable to fail eventually at the points of suspension.

4677. (*Mr. Bidder.*) Have you any experience, and have you come to any conclusion, after a cable is laid in a quiet sea all perfect, as to its durability, looking at the simple operation of decay, the rusting of the iron, or any change in the nature of the gutta-percha?—In the gutta-percha I have never seen any decay at all. With regard to the iron wire my principal experience has been in the North Sea, English and Irish channels, and Mediterranean; and there is no doubt that there are places, where it lies on the surface and on rough ground, where it decays very rapidly both in the North Sea and in the Channels. I have seen places in the Dover and Calais cable where the wires have been eaten away so as to be almost hanging by a thread, and the same in the Hague cables. There have been places which have come up where literally there has been nothing but the gutta-percha holding the cable together, whilst the cable a few inches off has been quite sound. It seems as if violent galvanic action had taken place from the proximity of some more negative substance than the iron, to which the iron formed a positive element. The hemp serving in such places retains the impression of the iron wires, showing that there has been no mechanical violence or attrition. In the Irish channel, over the muscle beds off Holyhead, rapid oxidation takes place, and in some places (close off the head) violent attrition, the tide running there very hot. In the Solent very rapid oxidation takes place. On the Dutch coast, for miles the cable is as perfect as when it was first laid; the galvanizing has not gone from it, and there it is generally slightly buried in the sand, which is evidently the reason why it remains in such a perfect state. There is another cause which may tend to its being more perfect there, which is, that the water is less salt; the fresh water out of the Rhine coming down the coast, there the water is much less salt than on the English coast. Still on the English coast the cable is frequently found buried in ridges of sand, which have grown up over it. In such places it is also quite perfect, although a few yards off in a bad condition, where it has laid on the surface. Then there is another rather curious fact with the shore ends off Orfordness. They are the same sized wire and the same quality of wire, and they are galvanized also, and yet the big shore end, which consists of 60 of these wires together, whenever I have examined it, has always appeared just as perfect as when it was first laid, and immediately outside the end the small cables are considerably eaten away. I recollect Mr. Edwin Clark speaking of some experiments on the Britannia Bridge, which have always seemed to me to have something to do with this case I am speaking of. He said that there, when some of the plates were lying about separate, they rusted very fast, and it rather alarmed them; they thought, I suppose, that if the bridge was going to rust at that rate it was very little use putting it up. Then, I suppose, from seeing a great number of plates together, and noticing that they did not rust so fast, they tried an experiment; they rivetted a piece of plate on to the bridge, and they put another piece of the same size, insulated by glass also on the bridge, at very nearly the same spot. The piece that was insulated rusted quite fast, and the piece which was rivetted to the bridge hardly rusted at all. Therefore, I imagine that that has something to do with those big Hague cables—where the large mass of iron is it does not appear to rust so fast. I cannot exactly see through what cause, but it seems to me to be the case, because there is the tail of the big cable there, and the small ones branch off—in these all the galvanizing has gone, and in the other it remains perfectly good. It is all very rough ground, and there is a very strong tide, so that possibly the weight of the shore end may be the cause by preventing oscillation, but I do not think that would entirely account for it. In the Mediterranean, although the water there is undoubtedly very salt, those cables that I have had an opportunity of examining appeared to have been but little affected by rust. The 20 miles of the Cagliari and Malta cable, which I picked up and relaid in 1859, appeared throughout to be in good condition. It is true it had only been down about a year. The pieces of cable off Cape Spartivento which we recovered in 1858, and some of which had been down since 1855, appeared also but little affected by oxidation, and there were none of those symptoms of sudden and violent oxidation at particular spots which I have seen on the cables in the North sea and channels, and which I have alluded to. The bottom in all these cases was principally mud, but off Cape Spartivento, near the shore, in some places the cable was covered with young coral. The cable appeared in such places to have been suspended free from the ground, for the young clean coral completely enveloped it, and appeared to grow out from it equally in every part of the circumference, and in a radiating direction. In some places it was so completely covered, that not a particle of the cable was visible for 40 or 50 fathoms consecutively, and as it came out of the water it had the appearance of a huge but beautiful coral necklace. There were no greater symptoms of rust here.

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4678. (*Chairman.*) Did you find that the nature of the bottom had any influence on the rusting. Where the cable was laid through sea-weed, or organic matter in a state of decay, or anything of that sort, did you find whether it tended to accelerate decay?—The nature of the bottom has no doubt considerable influence on the rusting. Where it is rough and hard ground, I have generally found the greatest decay. I believe, in general, that cables are greatly affected by resting on substances which cause galvanic action. I think the oxidation is also accelerated in a tideway by the coatings of rust being washed off by the mechanical action of the moving water, even where actual attrition does not take place, and I attribute the generally good condition of the Mediterranean cables partly to the absence of tide. At the same time, I would observe that the most perfect cases of preservation occur when the cable is buried in the sand, as in some places in the North sea. But I know scarcely any cases where the cable to my knowledge passed through seaweed or other organic matter, with the exception of the coral I have mentioned.

4679. It has been stated, with reference to the Red Sea telegraph that in parts where it has been exposed to mud and a good deal of organic matter, the decay has been very rapid, and that in those parts where it has laid on coral, or no organic matter undergoing decay, the cable is as perfect as when it was first laid?—I have no great experience of the case of weeds; I can only say that wherever there is sand, and it is buried in the sand, it is very perfect, and no doubt there is very little organic matter in the pure sea

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sand. The most in the way of anything like vegetation, or anything of that sort, has been with one of the Hague cables; when we picked it up once, about ten miles were covered with very fine specimens of zoophytes, and a few weeds, but I do not recollect seeing any more symptoms of rust. I think that galvanising is a very good plan. I think that it adds several years to the life of the cable.

4680. Do you propose to cover the iron with any material—do you propose to cover it with any material like that before you, which Mr. Latimer Clark proposes?—I am not aware what heat it stands—whether that would be adapted to the Red Sea; but I think that in many places, where the cable is not very likely to be much disturbed by anchors, and where oxidation goes on rapidly, it would be of great benefit. Where the cable is often hooked by anchors, it would soon get ripped off, because a ship, in clearing her anchor, often drifts along the cable for a considerable distance with the cable hanging over the fluke of her anchor. It struck me whether it would be possible to electrotype the wires with copper. I certainly think that the protection of the iron wire is the most important point for consideration in the improvement of telegraph cables.

4681. Would not the expense be very great?—As far as I can judge at present it would, but of course where a process is worth doing, the question is whether it cannot be made much cheaper than we believe it to be at present.

The witness withdrew.

8, Chalcot Terrace, Regent's Park,
September 25, 1860.

SIR,

In reference to my answer to the question 4659, I beg to submit a few further remarks on the subject of deep sea cable coverings, if you consider them worth printing.

So many engineers and scientific men have since the Atlantic project turned their attention to this subject, that I should not like to insist on any pattern of my own as being the very best. I beg, however, to offer a few remarks on the general question, and to offer a pattern for experiment. At the same time, I believe that the failures that have occurred have been too hastily and generally set down by the public as attributable solely to the nature of the outer covering of spirally laid iron wires.

I do not think that the failures that have occurred can be traced so much to the actual nature of the outer covering, as to the proportions of the different parts, and to general mismanagement.

A good deal has been said about what are termed "light cables," and no doubt that by means of hemp, instead of iron, the mere modulus of tension of the cable in water would be increased, and this would be one advantage during the operation of laying a cable in very deep water, if it could be attained without any corresponding disadvantages. It must be borne in mind, however, that the mere capability of a cable to support a great length of itself in water is not the only essential in an outer covering. Some amount of absolute strength with little bulk is required, besides a good protection from abrasions or mechanical violence. In laying a wire covered cable in very deep water, no doubt the narrow margin between the breaking strain, and the strain required to be applied to lay the cable, is a source of one difficulty, as it requires the necessary strain to be carefully applied.

Still there have been scarcely any failures which can be traced solely to this cause. The failure in the first attempt to lay the Atlantic cable in 1857, was evidently due to the construction of the break, which was totally different to anything used either before or since. The second year, two or three breakages took place, but these were also with a very heavy break. Still the cable was eventually laid, and evidently failed in its insulation, through causes totally unconnected with its outer covering.

There has also been, I believe, one other case of breakage during paying out on the Candia and Alexandria line, and two in picking up on the same line. But it should be borne in mind that many hundreds of miles of wire covered cables have been laid.

I think there is also a good deal of misconception as to the advantages to be gained by merely decreasing the specific gravity of a cable. Many imagine that an increase in the modulus of tension must necessarily follow. It is easy, however, to see that the specific gravity may be decreased, whilst the modulus of tension is also decreased. For if we add to a given heavy cable a substance, which, without adding any strength to the system, is heavier than water, though of less specific gravity than the rest of the rope, we actually decrease the modulus of tension, although we decrease the specific gravity of the whole system.

To take an instance, suppose a wire-covered cable be served round with hemp, giving the spiral of the hemp a very short lay. The hemp will not take its strain with the iron, and consequently can add no absolute strength to the cable. Hemp is, however, heavier than water; the cable will not therefore support so great a length of itself as it did before the hemp was added, as it has no greater strength, and yet has a greater weight per yard run. The specific gravity of the whole rope is, however, less than it was before the addition of the hemp, for as the specific gravity of the hemp is less than that of the iron-covered cable, the mean specific gravity of the two taken together must therefore be less than the iron-covered cable taken alone. Here then the modulus of tension is actually decreased by a decrease of specific gravity.

To gain therefore any increase in the modulus of tension, we must either increase the absolute strength without increasing, or in a greater proportion than we increase, the absolute weight in water, as by using steel wires, or else we must decrease the absolute weight in water, without decreasing (or in a greater proportion than we decrease) the absolute strength, as by using hemp alone, or, supposing it were practicable, aluminium alone.

For the reasons I have pointed out, I believe that very little addition in the modulus of tension is attained by combining hemp and iron, in the pattern recommended for the Falmouth and Gibraltar cable, and now being laid from Algeria. It is said that in that cable a great increase in the modulus of tension has been attained. I cannot, however, believe that the hemp takes its breaking strain with the iron, and if it does not, it must positively, as I have pointed out, decrease the modulus of tension. My reasons for believing that the hemp does not in this pattern take its bearing with the iron are as follows:—First, a hemp strand will of itself evidently stretch more than an iron wire before it takes its breaking strain, and if the two were therefore even laid parallel they could not take their breaking strain together.

In this pattern, however, if we take each wire with its hemp serving, and consider it separately as an element of the rope, it will be seen that the hemp is laid spirally round a straight wire. Now, the soft and compressible nature of the hemp does not allow of the principles which prevent stretching in a series of solid iron wires laid spirally round a central core to obtain; for elongation can evidently take place by the strands compressing themselves in section closer to the iron wires, and consequently they will not, from this reason alone, take their strain with the iron. There are, therefore, two causes which lead me to believe that the hemp does not take its strain with the iron.

There are further objections in my opinion to this pattern. As the iron wires do not touch each other, but are separated by the soft hemp, the mechanical principles which render a solid iron covering so excellent as a lateral protection, and so difficult to stretch, do not obtain. The rope it appears to me would, therefore, easily stretch, and the core would therefore be exposed to greater risk than on an ordinary cable, besides which in passing over a pulley under a heavy strain, I think the wires are more liable to cut into the core than in an ordinary cable, where from their touching each other they form a solid arch, which cannot easily have one wire crushed in.

In a good solid wire covered cable, the cable may have a strain applied very nearly equal to its breaking strain without any damage to the core; whereas in the Algerian cable pattern I believe it would be dangerous as a general rule to approach anything near its breaking strain. The advantages of a slight increase in the modulus of tension (which it is stated is attained in the Algerian cable pattern, but which I can scarcely believe,) must thus be rendered useless if the limit of safe strain is reduced by the practical objections I have pointed out. The covering does not, either, offer a good protection from such dangers as knives, nails, or any such chances of injury, which should always be taken into account in designing an outer covering.

It must be borne in mind, however, that a decrease in specific gravity, if it be accomplished without a decrease in the modulus of tension, is a benefit as long as the actual process of paying out is taking place. For the terminal velocity of the cable in water will be decreased, and consequently, if we take two cables of the same absolute strength, but of different specific gravity, that which has the lowest specific gravity can be laid, with a given amount of slack, with least strain.

Thus if we were to take two similar iron covered cables, and lay on to one a substance which, without adding

strength to the rope, was of exactly the same specific gravity as water, then the following effect would take place:—The two cables would still have the same absolute strength and the same absolute weight in water, and consequently would support the same length of themselves in water. The modulus of tension would therefore be the same. But as the specific gravity of one would be reduced, and consequently its terminal velocity, it could be paid out with ann given amount of slack, however small, with less strain than the cable of greater specific gravity; although to lay both absolutely without slack the same strain would be required, viz., the strain necessary to support a piece of either cable in water of the depth of the sea.

Again, if the specific gravity be reduced by adding a substance which, without adding strength, is heavier than water, though lighter than the rest of the rope, we get the following effect:—The cable of lesser specific gravity will have the greatest absolute weight in water, and as the absolute strength of both remains the same, the cable with the lesser specific gravity will be able to support the least of itself in water. The cable of lesser specific gravity will, in fact, have, as I have before shown, the lesser modulus of tension.

But as the terminal velocity of the cable of lesser specific gravity is reduced, there will be *some* amount of slack with which, if the two cables be laid, that of the lesser specific gravity will be subjected to the least strain, although to lay both absolutely without slack, that with the smallest modulus of tension will require the greatest strain.

In a combination of hemp and iron, therefore (where there must be a reduction in specific gravity over the *iron* alone), even if there is no increase in the modulus of tension, or even a positive decrease, there will be some amount of slack with which the cable can be paid out with a less strain than without the hemp. This benefit may be obtained in the Algerian cable, but it is a question whether, if there is no increase in the modulus of tension, this advantage is not counterbalanced by the other practical disadvantages.

The benefits I have pointed out as resulting solely from decrease in specific gravity, only exist as long as the cable is being paid out. When the ship is stopped or the cable being wound back or picked up, the proportionate safety depends, firstly, on the modulus of tension; and, secondly, on the absolute strength of the cable.

When the ships and cable are quite motionless, and the cable hanging up and down, of course the proportionate safety depends solely on the modulus of tension, and not on the absolute strength; that is to say, a cable that would bear, say, 5,000 fathoms of its own length, would be just as safe as another cable, which, though of double the absolute strength, would also only bear 5,000 fathoms of its length.

When, however, we begin to operate on the cable in any way, as by winding in with a sea on, then the absolute strength comes into play. Strains are put on the cable, as by a sudden lift of the ship, or by a jerk or surge on the drum, or by the ship drifting and bringing a horizontal strain into action, which are not caused merely by the weight of the cable; and in such cases the safety of the cable depends on its absolute strength.

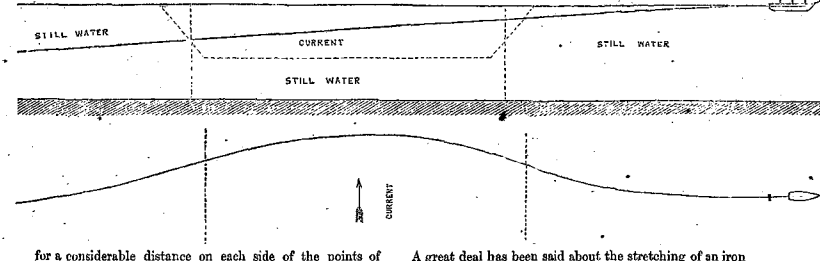
This will be more easily allowed if we consider the case first in shoal water, say 200 fathoms. Here the weight of the cable is still the sole element of strain when the ship and cable are motionless, and the safety of the cable depends on the *proportion* this strain bears to the breaking strain, quite independent of the absolute value of that breaking strain. In fact, the safety of the cable depends on its modulus of tension, the same as in deep water.

Thus, if a cable like the Atlantic would be subjected to a strain of 3 cwt., its breaking strain being 4 tons, and ten of these formed into a compound cable, would be subjected to a strain of 30 cwt. and stand a strain of 40 tons. Therefore, although the modulus of tension would be the same for both, the surplus strength or margin of strength would be ten times greater in one case than the other.

It would be absurd to suppose that, considering all the contingencies and varying strains to which a cable is subjected, particularly in foul weather, that this compound cable (possessing ten times the margin of strength of the single cable) could not be handled and laid with the least danger of the two. It is therefore evident that absolute strength is a practical advantage, and if so in the case we have supposed, it would be so in still deeper water, and it is a question at what depth this absolute strength shall be neglected as useless. In considering this, it must be borne in mind that the operation of laying long lengths of telegraph is not limited to the sole operation of steadily paying out.

It has not unfrequently occurred that faults of insulation have only been detected a few minutes after they have left the ship, and that winding back, or, as it is termed, "picking up," has been necessary, and in such case the absolute strength would come into serious requisition. This has occurred both with light and heavy cables. Absolute strength would appear therefore to be an advantage even in deep water.

In considering the specific gravity of a cable, it must be borne in mind that there are practical objections to a very low specific gravity; for, if very low, the cable cannot be laid with sufficient slack to meet the necessities of the irregularities of the ground where there is rough ground, and would thus be suspended from point to point. During the act of submergence such an enormous length of cable would also (with a very light cable) be suspended in the water in a straight line without any slack, that a portion of its centre might be acted on by a stratum of current, whilst a larger portion at each end of such length might be in still water or in water moving in a contrary direction, and thus a great strain would be brought on the cable at every point in that portion of the cable which remained in the current, and



for a considerable distance on each side of the points of contrary flexure. As the current continued to act and sweep the bight away, taking up whatever slack existed, the strain would be increased until the cable might be damaged or broken, before it could sink through into the still water below. This may be a remote contingency, but still it should not be neglected in considering the case from every point of view.

In such a case, with two cables of given strength and bulk, that which had the greatest specific gravity would here stand the best chance, as it would sink faster through the current, and thus not give time for the current to take up the amount of slack with which it would be paid out. With cables of the same specific gravity and bulk, that which had the greatest absolute strength would stand the best chance.

A great deal has been said about the stretching of an iron wire-covered cable, caused by the spiral lay, but this is greatly exaggerated, and it is evident that those who insist on this as a necessary theoretical consequence do not see the mechanical properties of the case.

For any elongation to take place each spiral wire must be straightened to some extent, that is, it must approach nearer to a straight line. Now this can only arise, taking a single wire, either by its retaining its regular number of convolutions, and by its being flattened in that position, or by the rope actually untwisting. The first of these effects can only take place by each wire approaching to the centre of the rope, *and thus occupying a smaller circle*; but as the wires are already in contact, they can only assume this new position by crushing each other laterally. They, in fact, form an arch, which, if hollow, would be in unstable equili-

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brium, and liable to be disturbed by any lateral pressure on one wire, but the core, without receiving any great pressure, acts as a kind of centreing, keeping the whole system in shape by preventing any one wire getting out of position, by being forced inwards; whilst the tension on each wire prevents it from getting out of position, by being forced outwards. Thus, as long as the wires are thus kept in their circular position, no elongation can take place, and no crush can come on the core. Any slight elongation that takes place is simply due to the wires being scarcely in solid contact in every part of the rope, and thus, until they take their bearing, and are in contact throughout, there will be a slight degree of elongation. This elongation is not, therefore, a necessary consequence of the theoretical conditions of the case, but is simply the result of the impossibility of absolute perfection in manufacture, and depends, therefore, on the degree of perfection with which the rope is manufactured, and I believe in a well-made cable will not exceed a half per cent. before the cable breaks.

It is true with a cable covered with strands, like the Atlantic, the strands do not form such a solid and stable arch as the solid wires, and, consequently, much more elongation will take place, and I think this is undoubtedly a disadvantage in a strand-covered cable.

As I have before said, the Algerian pattern cable must also, from the wires not being solid contact, be liable to a greater per-centage of elongation.

In a covering of solid wires, also, it is evident that the greater the number of wires the more unstable must be the system.

The second effect, viz., untwisting, can occur in two ways; first, in the case of one end of the cable being free to revolve, which is not, however, the case in paying out a cable. Secondly, by one part of the rope untwisting, thus twisting up another part tighter. Now, in paying out a cable in very deep water, the upper part of the cable which is in suspension from the ship to the ground, is under great tension, whilst that portion which is near and at the bottom is always slack, since the cable is always paid out with a certain per-centage of waste cable.

The weight of the suspended cable will attempt, therefore, to untwist the upper portion, and twist up the lower portions; and as the lower portions are not only slack, but have been already untwisted at the top, they offer no great opposition to this action, which can, therefore, take place in very deep water to some extent, although as each portion of the cable goes through exactly an equal amount of twisting and untwisting, it is in exactly the same state when laid as it was before it left the ship. This action undoubtedly took place to some extent in the Atlantic cable, where it is stated that pieces of paper attached to the cable made several revolutions between the stern-wheel and the water. In a solid wire covering there is, however, another force which tends to prevent this untwisting. The wires being laid on by the patent wire process, are only bent into the spiral position, and are thus free from all torsion in their fibre.

To untwist, therefore, it will be seen (if the experiment be tried), that torsion must take place in each wire, and as with a long spiral it requires an immense tension to produce untwisting at all, the joint resistance to torsion of all the wires together is sufficient to prevent it entirely. Indeed, I have watched very carefully to see if untwisting took place, both in paying out the Corfu and Malta lines through 1,900 fathoms, and in picking up the heavy cables in the Mediterranean, and I never could detect a trace of such action.

In the strand-covered cable, for untwisting to take place each strand must itself be twisted up tighter. This force replaces the resistance to torsion of the solid wires. It seems evident that the resistance of these strands to be twisted tighter is not so effective in preventing the untwisting of the cable as the resistance to torsion of the solid iron wires, since the action of untwisting takes place more in the strand than the solid wire rope.

The question of the coiling and uncoiling of a cable, and the effect upon such cable, appears to me from various statements to be little understood. Some imagine the rope has actually a series of permanent twists or turns put into it by the process of coiling and uncoiling.* This is not the case. When a rope is coiled, the rope is twisted once round on its axis, for each convolution of the coil, and is thus in an over-twisted or under-twisted state (according to the way it is coiled), as long as it lays thus coiled. But the act of uncoiling puts a reverse turn or twist into the rope for every convolution, and thus the turn put in, in coiling, is neutralized, and the rope is in the same state as before coiling.

It is true that, by bad coiling, the twist requisite for each convolution may be put into the cable all at one place in the convolution of the coil. In the act of uncoiling, although a turn in the reverse direction must of necessity occur for each turn put in in coiling, yet this again, from the manipulation, may be put in at an opposite part of the convolution to that where the turn was put in in coiling. The two turns will thus exist for a moment in the rope, though they are in *reverse* direction, but the moment any strain comes on the rope they will instantly neutralize each other, and the rope will be exactly as it was before coiling.

If the rope be coiled in a perfectly circular coil, and the leading part led down over the centre of the coil, this effect even cannot occur, for in coiling the turn will be equally distributed over every part of the convolution, and in uncoiling the same, and thus the only effect of coiling is to cause the rope whilst it is laying in the coils to be in a state of slight torsion, which is immediately annulled on its leaving the coil. The amount of torsion depends on the size of the coils in proportion to the size of the rope, and it is, of course, greater on the inner convolutions of the coil than on the outer. It is dangerous to make the inner diameter of a coil very small, for there is evidently a limit to the flexibility of every rope. These effects would apply to any rope, whether platted, spiral, or straight. They are the essential conditions on which alone the coiling of a rope can take place.

I may also remark that coiling and winding are two different processes. When a rope is wound on a drum, there is no turn put in, and in unwinding there is also no turn put in. If a rope is *wound*, therefore, and then *uncoiled*, one permanent turn or twist will exist in the rope for every convolution, and such a process should never, therefore, be adopted. In the same way, if a rope be coiled and then the coil be lifted bodily on to drum and unwound, one permanent turn for every convolution of the coil will be put into the rope.

In the manufacture of short lengths of hemp or iron rope, they are wound on to drums as they proceed from the machines, and then slipped bodily off the drums for sale. If one of these were then placed on a drum and unwound, the rope, when laid out straight, would be free from all torsion, and would be exactly in the state in which it was manufactured.

These generally fall, however, into the hands of sailors and others who invariably uncoil them. Now, if they take the outer end of such a coil and uncoil, they would put a turn into the rope for every convolution, and according as the rope was wound, and the actual lay of the rope, this would either twist the rope up tighter, or else unlay it to a certain degree. By passing this end through the coil they would reverse the action. Now, as the rope will, according to the nature of its manufacture, bear either being laid up tighter rather than be unlaid, or *vice versâ*, directions for passing the end through or not, are sometimes attached to the coils, and if they are not the men soon find by practice which process brings the rope out into the most flexible state.

This necessity in the uncoiling of new ropes has led some naval officers to imagine that a submarine cable (which is never wound on a drum, but always coiled,) must leave the ship in a state of torsion. I trust, however, I have explained that this is simply a misconception on their part.

It appears to me that the following are the necessary requisites in a deep sea cable covering:—

First. That the covering of itself be unlikely to injure the core in any way when subjected to strain, or to allow any injurious strains to come on the core until it is itself destroyed.

Secondly, that it shall be a good protection to the core against any lateral pressure or strain.

Thirdly, that it shall have as high a modulus of tension as possible.

Fourthly, a moderately low specific gravity.

Fifthly, as great absolute strength as possible in proportion to its—

Sixthly, bulk, which should be as small as possible.

Seventhly, cheapness.

Eighthly, facility of manufacture, with the least possible danger to the core.

Lastly, that it shall be as durable as possible in water, whilst it will allow water to reach, at once, the core, even under low pressure, so that the core may be easily electrically tested for insulation.

The first two conditions appear to me to be met by a solid wire-covering better than by any other covering I have yet seen.

With regard to the third, hemp has the advantage over iron, but I doubt whether it has any advantages over steel.

For the fourth condition, it is possible iron or steel may be combined with advantage with hemp, but I think not in the way the Algerian cable is made, without other disadvantages which I have already pointed out.

* A pamphlet was printed and circulated in 1859, by a post captain in the navy, asserting and endeavouring to demonstrate that a number of twists in excess of what he called the normal number are put into the cable, and that "*in that state is the cable laid.*"

The fifth condition, and sixth, would be best met by steel, for it is evident that the same strength as steel cannot be obtained by iron without greater bulk, and with hemp without a very great increase in bulk.

The seventh condition, no doubt, steel would stand last, and hemp first.

The eighth, I think, has not been sufficiently considered; and I think that many of the coverings I have seen would be excessively difficult and tedious to manufacture.

The last condition is one of the most difficult to fulfil. Many of the proposed protections to an iron or steel covering would be of themselves nearly water-tight, and would thus prevent the core being tested for insulation under low pressure.

I would suggest the following pattern as a good cable for experiment:—

First. Outside the core a serving of strong and well-made strands of hemp, but made with a very long lay, much longer than the ordinary serving, which has generally been made so as to add no strength to the rope.

Secondly, a layer of solid galvanized iron wires, laid on in a shorter lay than the hemp serving, and in the reverse direction.

Thirdly, a layer of strong strands of hemp, laid in the same direction, and with the same length of lay as the first hemp serving.

I think such a cable would possess the following advantages:—

First. By adjusting the proportion between the lay of the iron and the hemp, the two might be made to take their breaking strain together; the absolute strength would be thus increased, by the hemp, in a greater proportion than the weight in water is increased, and, consequently, there would be an increase in the modulus of tension over a simple wire-covered cable; and there would, consequently, be increased safety when the cable was hanging perpendicularly from the vessel, or when being laid without slack.

The specific gravity would be also decreased by the addition of the hemp over a wire-covered cable, whilst, as I have shown, a positive increase in the modulus of tension would be obtained; consequently, with a given amount of slack the cable could be laid with *much* less strain than a wire-covered cable of the same absolute strength. From the presence of iron in the system it is evident that a greater absolute strength with a given bulk can be obtained than with a simple hemp cable.

The proper specific gravity for sinking conveniently can be adjusted by the proportion of the iron to the hemp.

The cable would also be even safer as regards mechanical protection to the core than a simple iron-covered cable.

The second coating of hemp would tend greatly to protect the iron from oxidation, whilst it would not prevent the water from penetrating to the core for testing the insulation.

The servings being both in opposite directions to the wire covering would completely counteract any slight tendency to untwist, which a heavy strain may have in extremely deep water.

Such a rope could be easily manufactured, and at a cost little greater than the ordinary cable. Of course, steel instead of iron would render the pattern still more adapted for the work, and for any important line in deep water, I believe the increased safety gained by steel would compensate for the extra expenses.

I would remark that all the practical objections which, in my opinion, exist in the Algerian pattern are here met, and I believe that it meets many of the objections to a simple hemp or simple iron covering. Still I would only submit the pattern as one for discussion and experiment.

I have, &c.

(Signed) F. C. WEBB.

F. C. Webb, Esq.
24 Aug. 1860.

To Captain Douglas Galton, R.E.,
Chairman of Committee on
Submarine Telegraphs.

Adjourned.

Tuesday, 4th September 1860.

PRESENT:

Mr. BIDDER.
Professor WHEATSTONE.
Mr. Edwin CLARK.
Mr. FAIRBAIRN.
Mr. SAWARD.
Mr. MARSHMAN.
Captain PEEL.

CAPTAIN DOUGLAS GALTON IN THE CHAIR.

Mr. WILLIAM MAYES (Master, R.N.) examined.

Mr. W. Mayes.
4 Sept. 1860.

4682. (*Chairman.*) I believe you have had considerable experience in picking up submarine cables?—Yes, I have seen a great deal of it.

4683. Where have you been employed lately in that operation?—In the Red Sea.

4684. Have you been employed on the Red Sea line entirely?—Yes, the repairs have been confined to that line.

4685. Had you had any experience before that in picking up cables?—None whatever. I was master of H.M. ship "Cyclops" when that vessel was employed taking the soundings between Valentia and Newfoundland for the Atlantic telegraph, and also when she formed part of the squadron on the first attempt to lay that telegraph.

4686. You hold the office of marine superintendent in the Red Sea company, do you not?—Yes. I was previously employed in the "Cyclops" in sounding and surveying the route of the Red Sea and India Telegraph, and accompanied the vessels laying down the Red Sea Cable. I had charge of the boats of the "Cyclops" repairing the cable when it had been broken by an anchor at Cosire.

4687. Will you have the goodness to state to the Committee the sections of the Red Sea line upon which you have been employed. I believe you are still employed by the company?—Yes.

4688. What was the nature of your appointment?—To superintend, on the part of the company, the repairs of the Aden and Suakin section of the Red Sea cable.

4689. Did you superintend the laying down of any of that cable, or were you present at the laying of it down?—I attended on board the paying-out ship, and witnessed the laying of the cable between Kurrachee and Maculla. The Red Sea line was laid before I was employed by the company.

4690. You were present on board the ship while the eastern sections were laid?—Yes, and latterly I have witnessed all the operations connected with the repairs of the Aden-Suakin section. I have been employed to some extent on every section of the Red Sea and India telegraph.

4691. Can you state the length of those sections. For example, what is the length of the Aden and Suakin section?—The present length of the Aden and Suakin section is 645 miles.

4692. The next section to that would be, I believe, the Cosire and Suakin?—Yes.

4693. What is the length of that?—477 miles.

4694. The repairs upon that you have not superintended?—No; it has always worked.

4695. The next section north of that would be the Suez and Cosire section?—Yes; that is 254 miles.

4696. From Aden to the East will you state the lengths of the sections?—Of the first from Aden to Kooria Mooria the length is 717 miles.

4697. From Kooria Mooria the line, I believe, goes to Muscat?—Yes.

4698. What length is that?—488 miles.

4699. From Muscat to Kurrachee what is the length?—477 miles.

Mr. W. Mayes.

4 Sept. 1860.

4700. Can you give us the greatest depths on those sections?—Yes. Beginning at Suez, the greatest depth on the Suez and Cosire section is about 400 fathoms.

4701. What is the nature of the bottom under that section?—It is almost entirely mud and sand on that section.

4702. What is the depth on the next section?—The next section is from Cosire to Suakin, and the greatest depth there is about 460 fathoms.

4703. What is the nature of the bottom under that section?—Mud.

4704. Then take the next section from Suakin to Aden, what is the greatest depth?—The greatest depth on that line is 250 fathoms.

4705. What is the nature of the bottom?—That is irregular; where the water is shallow, the bottom is rocky, generally with coral. In deep water, or over 150 fathoms, it is almost invariably mud.

4706. To what depth does the coral extend?—I think that the coral, properly speaking, is very near the surface, on the rocks, I should think, in not more than 10 or 15 fathoms.

4707. Did you find coral in greater depths than that?—I think not.

4708. Not in 50 fathoms?—I think not.

4709. (*Mr. Fairbairn.*) It was found chiefly in shallow water?—Yes, in the Red Sea the coral is found adhering to the upper parts of rocky elevations of the sea bottom.

4710. (*Chairman.*) From Aden to Kooria Mooria what is the greatest depth?—It is probably 800 fathoms.

4711. What is the nature of the bottom?—That is irregular; in deep water it is mud; near the shore it is chiefly sand.

4712. Is there any rock?—Occasionally, near the land, it is rocky.

4713. In the shallow water?—Yes.

4714. Are the great depths suddenly reached, or is it a shelving bottom?—In the vicinity of Kooria Mooria the rising is very abrupt.

4715. From Kooria Mooria to Muscat what are the greatest depths?—About 300 fathoms.

4716. And what is the nature of the bottom?—Generally speaking the bottom is mud upon that section or sand.

4717. Is there no rock there?—A few rocky patches near Kooria Mooria.

4718. From Muscat to Kurrachee what have you found to be the greatest depth?—The maximum is probably 1,900 fathoms.

4719. Does that extend for any considerable distance?—I cannot say exactly the extent of that deep water; about 100 miles of the line is in over 1000 fathoms water.

4720. Have you any data as to the nature of the bottom under that section and in those depths?—In the deep water it is mud, and in the shallow water it is rock and sand.

4721. Can you tell us what is the condition of the several sections, for instance, the Suez and Cosire section, is that in good working order now?—No, there is a fault apparently very near Cosire, in the anchorage.

4722. It was broken by an anchor, I believe, very soon after it was laid?—That particular section was broken in the anchorage.

4723. It has now been broken again in the anchorage?—It is defective near the anchorage.

4724. Is the Cosire and Suakin section in good working order?—It is working; it is better than it was when it was laid.

4725. Has it undergone much repair?—It has never been touched.

4726. Was it upon that section that there was a fault when it was first laid?—It was.

4727. But it improved afterwards?—Yes.

4728. That fault has never deteriorated since?—The last I heard of it was ten months after that section was laid. The insulation was better than it was immediately after it was laid.

4729. (*Mr. Seward.*) Is the fault the loss of insulation?—It is a positive fault.

4730. (*Chairman.*) Do you know whether the electricians attribute that fault to any particular circumstance?—We were aware that there was a fault in the cable as originally laid, in paying it out.

4731. But they have not detected anything farther about it?—No.

4732. From Suakin to Aden, what is the condition of that section?—It is not working; there is a fault about 270 miles from Suakin, in the middle of the Dharlac bank.

4733. What are the depths over the Dharlac bank?—They vary from very shallow, a few fathoms, to fifty fathoms.

4734. It is in the shallow water that the fault has occurred?—All the faults on that section have been in the shallow water.

4735. Have you under-run that section to any great extent?—We have under-run about 200 miles of it.

4736. Can you give the Committee a short account of your proceedings as to that section, when did you commence operations upon it?—In February last.

4737. Have you been employed upon it between February and the time when you returned?—Yes, between February and August; we commenced on that section about the 23rd February.

4738. Was it to find particular faults, or was there more than one fault?—At that time we thought that there was but one fault; the electricians spoke only of one when they commenced operations.

4739. Did you find that fault?—The line was severed about 76 miles from Aden, and they discovered a fault between that point and Aden, and also one, as they supposed, up towards Suakin, about 200 miles.

4740. Then, I suppose, they removed those faults?—They removed all the faults in the course of the repairs, but it became faulty after it had been repaired.

4741. Do you think that they injured it in repairing it?—They injured it very much in making the repairs, but they repaired those injuries.

4742. It is still faulty?—Yes; it is supposed that the cable is broken in some place where it was very tight and the wire bad.

4743. What was the condition of the wires when you raised the cable in the first instance?—In the first instance it was very good.

4744. To what was the fault attributed?—There were three faults within 76 miles of Aden. The nearest fault to Aden was in the shore end; we did not pick it up; we could not see the cause of it. Then there were two other faults; one at 18, and one at 55 miles from Aden; one appeared as though the gutta-percha had been softened by heat, and the yarn had cut into it and bared the copper wire; the other appeared to be a defect caused by something such as the flange of a drum rolling over it, cutting into the copper wire.

4745. As if it had been injured in paying out?—It was injured before it was covered with the yarn and wire probably.

4746. Do you mean at the gutta-percha works?—It is not possible to say where.

4747. In the process of manufacture?—Certainly. The next fault was 76 miles from Aden, and that appeared to be from a defect in the iron wire.

4748. In what state was it? can you describe its condition?—The cable was broken at the fault. Some of the copper wires appeared to have been just broken; others appeared corroded, as though they had been broken some time. The iron wires were much corroded at the ends, and appeared as though they had been broken a long time from tension.

4749. Do you mean tension from paying out?—From tight paying out, or a jerk in paying out.

4750. What makes you think that?—I judge from

the appearance of the cable and from circumstances which occurred in laying it down and in repairing it.

4751. Do you think that the tension could not have been put on it in raising it?—I think it could not.

4752. Why?—Because the wires were old breaks, and corroded.

4753. Was the gutta-percha injured where it had been stretched?—Yes, at the fault.

4754. Was it attenuated?—I only saw one end of it, and that was broken and dirty, as though it had been faulty some time, but the gutta-percha had sprung back on the copper wire, as usual in such breaks.

4755. The fault arose from the cable having been actually broken in two?—No; I think that we broke it in picking it up, but it had been very nearly broken for some time.

4756. Was it only holding by the copper wires?—Some of the copper wires were broken and corroded; one or two of the wires were then newly broken by the grapnel, and the iron wires appeared to have been broken some time.

4757. (*Mr. Fairbairn.*) In that case, those which had been broken for some time must have been broken in paying out?—It is probable that some of the wires were broken in paying out, and the cable lay with a great amount of strain and tension, and it bore this tension whilst the iron wire was new, but as it became corroded, more of the iron wires broke and brought the tension on the copper wire and gutta-percha, destroying the insulating properties of the latter.

4758. (*Mr. E. Clark.*) Might an anchor have done it?—I do not think an anchor could, without breaking the cable altogether.

4759. (*Chairman.*) At what depth was that?—About 25 fathoms.

4760. Had you any difficulty in picking it up in that depth?—Very little.

4761. (*Mr. Fairbairn.*) That must have been broken in paying it out, and not after it was on the ground?—It is certain that the cable was not absolutely severed till nearly the time the repairs were commenced.

4762. (*Chairman.*) How long had it been laid down?—About nine months.

4763. Do you know when the fault first appeared?—In January 1860.

4764. How long had it then been laid down?—That section was completed in May 1859.

4765. Then it had been laid down eight months before the fault appeared?—Yes.

4766. Was it in good working order up to that time?—It worked; it had become deteriorated gradually.

4767. (*Mr. Fairbairn.*) Then the breakage must have taken place after it was laid down?—Yes; it was in good working order till January 1860; when any particular fault showed itself, I do not know; but the first interruption was in the latter end of January, and it ceased to work finally in the first week of February 1860.

4768. Then it must have been worked for nine months?

(*Capt. Peel.*) It was worked from the 1st of June till the 23rd January 1860, when the first interruption occurred, which lasted till the 29th of that month. It finally ceased working on 6th February.

4769. (*Chairman to the Witness.*) Did this injury from straining appear to have been done by sudden jerks in the drums in laying down the cable, or from a continuous strain?—I think that particular one at 76 miles from Aden was caused by a jerk.

4770. The rest was not laid tightly?—The other part appeared to be in very good condition; and not very tight in this vicinity.

4771. (*Mr. Fairbairn.*) Is it your opinion that it received an injury in paying out, in the first instance, by over tension, and then gave way afterwards?—In this particular fault I think some of the wires were broken by a jerk in paying out.

4772. (*Chairman.*) In this particular case you thought it was broken by a jerk; did you observe any other cases in which you thought that there had been a continuous strain upon the cable in paying out that would have caused an injury?—Yes.

4773. What was the state of the cable in those instances?—The cable appeared tight when hauled up and in good condition generally, but at irregular intervals sometimes of a few yards, sometimes of miles, there were places in the cable with many and occasionally all the iron wires broken and corroded as though they had been broken some time.

4774. You found a few wires broken here and there as you went along?—The wires were broken together at particular places in the cable, with irregular lengths of good cable intervening.

4775. You were on board the ship in paying out the sections east of the Aden length?—Yes.

4776. At that time did you observe any very tight paying out?—Between Kurrachee and the edge of the shallow water towards Muscat the line appeared very tight.

4777. (*Mr. Bidder.*) Who regulated the strain?—Mr. Newall and his assistants.

4778. The contractor?—Yes.

4779. Was there anyone there then to supervise?—Mr. Ford, the engineer, was there, but he had no control over the work of the contractor.

4780. It was left entirely to the discretion of the contractor?—It was in his discretion; I do not know whether it was intended to be so or not.

4781. What was the use of Mr. Ford being present if he had no control?—I do not know.

4782. (*Mr. Fairbairn.*) Had they no power to regulate the tension by a break in paying out; no apparatus?—Yes, they had.

4783. Who had charge of that?—Mr. Newall and his assistants.

4784. The contractor was responsible for that?—Yes.

4785. It would appear from your evidence that the injury to the cable and the breaking of the wires must have taken place in paying out; I do not see how it could have taken place after the cable was once laid at the bottom?—As the wire became weak from corrosion, it broke, being no longer able to bear the tension originally put on it.

4786. (*Mr. E. Clark.*) You state that in several places there was evidence of great strain?—Yes, in a great many places?

4787. Was there always, in those cases, evidence of the strain by the stretching of the gutta-percha, and the copper wires inside of it, when you examined the places where those wires were broken?—The gutta-percha appeared to be stretched and the electrician stated that its insulating properties were in most cases impaired. I have not examined the conductor but have specimens in London for that purpose. The evidence that the iron wires were broken from tension is such as to place it beyond a doubt in my mind.

4788. There can be no doubt of it?—I am positive the iron wires were broken from tension.

4789. (*Chairman.*) You picked up the cable in several places, did you find the iron wire corroded to any great extent?—*I think that* the total strength of the cable was diminished by about an eighth, not more than that, from corrosion or rust.

4790. (*Mr. Bidder.*) Have you any specimens of it?—I have in London at the office, which I can furnish to the Committee.

4791. (*Chairman.*) Did you observe whether the cable had been much injured by rubbing on the bottom in places?—I have no reason to think that it had been so injured.

4792. Did you find any part of it on any of the sections where it was injured by rubbing?—Not one.

4793. Did you observe corrosion more on one part of the bottom than on another part?—No, but it has been stated that it corrodes most on muddy bottom. I think that it corrodes nearly equally on every kind of bottom.

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4794. Was there any evidence upon the cable itself to show whether there were currents which had been affecting it in any way, or the reverse?—No. I could see nothing to lead me to believe that the cable had been damaged in that way.

4795. (*Mr. Bidder.*) In short all these defects were inherent in the cable from the time of its being laid?—Yes.

4796. (*Chairman.*) You stated that in some cases you found faults, that is to say in places where the cable had been apparently tight, and where the wires were broken, and you found in some cases faults where the wires were not injured by the strain. I think you said that in some cases the electricity appeared to have injured the cable?—Yes; out of ten faults I think that four were defects in the gutta-percha, made worse by the action of electricity.

4797. (*Mr. E. Clark.*) Were those mechanical defects in every instance?—One appeared to be from some defect in the gutta-percha, but it got worse by the action of the batteries; one was from something rolling over the cable before it was covered with iron wire, and one appeared to be caused by accidental heating of the cable after it was manufactured.

4798. By defects in the gutta-percha; do you mean a natural defect or an injury?—Probably an air bubble.

4799. (*Mr. Fairbairn.*) Did you notice whether the insulation was complete in those parts that were broken, whether the gutta-percha was thinner on one side than another, or whether the conducting wires had got nearer the outside in one part than in another?—The electricians stated that the insulating property of the gutta-percha was impaired in most cases where the iron wires were broken.

4800. You have no reason to suppose but that the cable was perfectly quiescent when it was laid down at the bottom, that there was no disturbing cause?—*I think* that it lay quite still.

4801. (*Mr. E. Clark.*) Four out of the ten faults arose from defects in the gutta-percha; what were the other six?—Probably from the breaking of the iron wires, bringing the tension on the copper wires and the gutta-percha.

4802. (*Chairman.*) Did you find the iron wire much covered with marine shells or sea-weed?—In the shallow water very much so; in the deep water less.

4803. (*Mr. Fairbairn.*) Was the mud carefully examined that was taken out of the deep water?—I am not aware whether it has been chemically examined or not.

4804. I mean to ascertain whether there were any marine shells—very minute shells. It was not examined by the microscope?—Not on board the telegraph vessels that I am aware of, but specimens of the bottom on nearly every part of the line between Suez and Kurrachee were sent by Captain Pullen, of H.M.S. "Cyclops," to the Hydrographic Department of the Admiralty for examination; that officer also examined many specimens himself.

4805. (*Chairman.*) You stated that you underrun the cable to a considerable extent; what arrangements did you adopt for that purpose; any peculiar ones?—We generally placed it over the boat, and under-run it by men pulling on the cable, and pulling the boat along by it; that was found to be the safest way.

4806. Did you try any other apparatus?—We sometimes sailed the boat and sometimes towed her.

4807. Was there not a sort of tube that you used?—On one occasion we tried the plan of towing a tube on the bottom, the cable passing through it.

4808. Did that answer?—It broke the cable almost immediately, and that part of the cable was good.

4809. What sized tube was it?—The diameter of it was about six inches.

4810. Not much larger than the cable?—Very much larger, the cable being only six-tenths of an inch in diameter.

4811. Where the tube was suspended, near the bottom?—The cable was passed through the tube, and the tube was lowered on the bottom, and towed by a rope at a distance from the ship, about 100 yards.

4812. Did you recover the tube afterwards?—Immediately; when the cable broke it came to the surface. Luckily it was tried in shallow water.

4813. It was a patent of Mr. Gordon's, was it not?—I think not, it was devised on the spot, and made of two pieces of the trough in which the cable runs in paying out.

4814. (*Mr. E. Clark.*) What was the thickness of the animalculæ which you found on the cable when it was raised?—About a foot in diameter.

4815. Do you mean continuously a foot in diameter?—No, here and there, perhaps every few yards; the greater part of the cable was about two inches in diameter on rocky bottom. On sandy bottom it was generally clean with light coraline structures adhering to it. On muddy bottom the cable appeared to have been sunk and was covered with mud when brought to the surface and almost without any marine animals or plants adhering to it.

4816. (*Mr. Fairbairn.*) What was the original diameter of the cable?—Six-tenths of an inch; that which was on the cable was very fragile, it would easily knock off.

4817. (*Mr. Marshman.*) Was the smell very offensive?—Generally it became so after it had been a little time out of water.

4818. (*Mr. E. Clark.*) But it is not decaying matter when it is at the bottom?—I think not.

4819. (*Mr. Fairbairn.*) What was the diameter of the cable when you took it up?—Sometimes there were masses of a foot in diameter adhering to it.

4820. (*Mr. Bidder.*) When there was a great aggregation of animalculæ, did you find that the corrosion had been more or less?—I could not see any difference in that respect.

4821. (*Mr. Fairbairn.*) What state was the cable in when you left in August?—We have four of these sections interrupted at present.

4822. There are two in operation?—Yes.

4823. Which two were in operation?—Between Cosire and Suakin, and between Kooria Mooria and Muscat.

4824. Were they working satisfactorily when you left?—Yes.

4825. (*Chairman.*) Were you employed in repairing the cable between Suakin and Aden, and between Aden and Kooria Mooria?—In repairing between Suakin and Aden only.

4826. Have you not visited the cable between Aden and Kooria Mooria?—I saw it laid down.

4827. In what condition is that section now?—There is a fault in it, interrupting communication.

4828. At what distance is that from Aden, and is there more than one fault or only one?—Said to be one fault, about 200 miles from Aden.

4829. Is it in shallow water or deep water?—I think in shallow mater.

4830. (*Mr. Fairbairn.*) Will you be good enough to state whether that accumulation which you spoke of as being a foot in diameter in one place and two inches in diameter in another, was greater in the shallow water or in the deep water?—In the shallow water, on the rocky bottom, it was the worst.

4831. What was the greatest depth—400 fathoms?—I have never seen a piece of cable from that depth. I know that the bottom is muddy almost always in that depth.

4832. (*Chairman.*) Did you observe whether the lay of the wire was at all elongated at the places where you picked it up?—Comparing it with new cable it appeared elongated and the iron wire seemed firmly nipped round the yarn and gutta-percha.

4833. (*Mr. E. Clark.*) Did any actual breaks occur from over tension in the act of laying the cable down?—I did not see the section between Aden and Suakin laid down. I think there were no breaks.

4834. Mr. Ford was present, was he not?—Mr. Gisborne was present on that particular section.

4835. (*Mr. Marshman.*) Was there no break or injury in the laying down between Suakin and Aden?—(*Capt. Peel.*) I believe not.

(*The witness.*) A fault occurred in the laying, but not a break.

4836. (*Mr. Bidder.*) Was the cable, at any one time, from Suez to Kurrachee, ever perfect all through. Could they ever communicate uninterruptedly from one end to the other?—They did, for a few days, while the Aden-Suakin section was under repair.

4837. (*Mr. Marshman.*) Did you not communicate from Aden to the Governor-General at Kurnaul and get an answer back in 24 hours?—I do not know. I am not aware.

4838. (*Chairman.*) Did you in any case of underrunning the cable break it while underrunning it?—Several times.

4839. Were you able to recover both ends and go on again?—Yes, in shallow water.

4840. Did you find it fixed at the bottom in any way?—On the north edge of the Dhalac bank the cable was foul round a rock at the bottom, and it was broken in trying to clear it, and the north end could not be found.

4841. At what depth?—About 50 fathoms. We broke it also north of Perim, and could not find the other end in 80 fathoms water.

4842. Was it adhering to the bottom?—I think not.

4843. What was the reason of the breaking?—The iron wire was broken, and had been broken for some time. There was no strain upon it at the time.

4844. (*Mr. Bidder.*) Did you break it in overhauling it by manning the boat in the way you have described?—It was broken in recovering or picking up the cable into the ships.

4845. You put no strain upon it?—No.

4846. (*Chairman.*) You did not break it in consequence of the cable getting round a rock?—Not in the latter instance, but we have done so in the course of the repairs.

4847. Did you apply a great strain to it?—Yes; on the north edge of the Darlak bank we broke it once or twice, round rocks; but found the other end again. Where the cable was in average condition it bore a good deal of strain.

4848. Do you think that the marine animals and shells did not cause it to adhere to the bottom?—Not at all.

4849. (*Mr. Fairbairn.*) But these animals do adhere to the bottom where there is rock; they fasten to it?—I think they are too fragile to exert any force in holding the cable. I saw nothing to lead me to think that they do.

4850. (*Mr. Saward.*) The breakages arose from the cable being unable to bear its own weight owing to the oxidation?—I think it was unable to bear the tension put on it in laying it down.

4851. (*Chairman.*) Do you know what strain the Red Sea cable is supposed to bear?—(*Capt. Peel.*) Four tons.

4852. (*To the Witness.*) There were only four faults in that section due to any cause except injury to the outer covering?—That is my opinion.

4853. You cannot give any evidence as to how those faults originated; whether they arose from the use of high battery power, or from some original fault in the cable?—No other evidence than I have already given. The battery power is under strict regulation, and it has never been increased until the communication has been quite interrupted with the ordinary power.

4854. Do you know what amount of battery power they use?—I cannot give positive information on this point, as my duties and knowledge relate chiefly to the mechanical condition of the cable.

4855. (*Mr. Marshman.*) Do the contractors leave directions with regard to the battery power that should be used at each station?—I think the general superintendent has instructions about it.

4856. (*Mr. Saward.*) Do you know what description of batteries are used; are they Daniell's batteries?—They are a form of Daniell's battery.

4857. On Daniell's principle?—(*Capt. Peel.*) Yes.

4858. (*Mr. Marshman to the Witness.*) Is the usual way of testing the cable, or of ascertaining the faults, to increase the power in the battery?—The electricians, while the repairs were going on, used large battery power in making their tests.

4859. (*Chairman.*) In what depths did you grapple the cable?—As much as 50 fathoms.

4860. Did you not grapple it in greater depths?—Never.

4861. Would there be any difficulty in grappling it in greater depths than 50 fathoms?—The difficulty depends upon the depth of water, the nature of the bottom, and the precision with which the position of the cable is known, of which an irregular rocky bottom presents the greatest difficulty.

4862. On a rocky bottom could you not grapple it at a greater depth than 50 fathoms?—No, not without very great difficulty.

4863. It would depend upon the nature of the bottom a good deal?—Yes.

4864. In any cases did you grapple for it, and were you unable to find it?—On the north edge of the Darlak bank we grappled for some time and could not find the cable, and to the north of Perim it could not be found.

4865. (*Mr. E. Clark.*) Did you lose your grapnels occasionally?—Very frequently.

4866. (*Mr. Fairbairn.*) I believe they grappled the Atlantic cable at 150 fathoms?—I do not know what the limit would be, if the bottom were smooth and the position of the cable well known.

4867. (*Chairman.*) Was the position of the cable very carefully laid down on the charts to enable you to grapple it?—Perhaps as well as it could be far from the land, but near the land, where the position could be defined by landmarks, it might have been better plotted.

4868. Had you very careful charts in laying down the cable?—The best that are published.

4869. (*Mr. Fairbairn.*) Was the cable of the same diameter or thickness in shallow water as in deep water?—Yes, except at the entrance to a port; there it is made larger.

4870. Suppose the cable was larger and stronger at a greater depth from the shore, say at 60, or 70, or 80 fathoms, would that meet all the objections that have been made as to breakages?—It would be a great improvement; but I have said that the breaks have occurred from the cable lying tight along the bottom. It appears to me that the breaking up of any cable so laid and liable to corrosion is only a matter of time.

4871. (*Chairman.*) Do you know to what depth No. 2, the shore end, is laid?—At Suez, Aden, and Kurrachee, the outer ends of the No. 2 cable are in about 12 fathoms water. At Kooria Mooria they are in about 30 fathoms; at Muscat in about 35 fathoms. At Suakin and Cosire they are probably in deeper water than at the other ports.

4872. (*Mr. E. Clark.*) Have the shore ends failed in any case?—There is a fault in the shore end at Aden.

4873. (*Chairman.*) In the anchorage?—In the shore end; not in the anchorage, but near the shore.

4874. (*Mr. Fairbairn.*) The object of my question was to ascertain whether, if the Red Sea cable were laid down of this size (*showing a specimen to the witness*), at a depth of 100 fathoms, or a greater depth than 100 fathoms, and then afterwards made thicker, as it got nearer to the shore, whether that would not obviate the difficulties you have had to contend with?—A line so laid would doubtless be more lasting, because it is difficult to lay a cable tight except in shallow water, where, by such an arrangement, it would be protected by stouter wire to bear the tension.

4875. In your experience of the cable in question

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have you ascertained whether, if the cable were laid in deep water, and carefully paid out, there is any reason to suppose that the oxidation would be increased or not, as compared with what would occur in shallow water?—I have not seen the cable got up from greater depths than 50 fathoms. I cannot say what takes place after that depth.

4876. You have not examined it much deeper than that?—Not at all.

4877. (*Mr. Bidder.*) You have not here specimens of the gutta-percha?—No.

4878. Who has them?—The engineer in charge of the repairs.

4879. (*Chairman.*) The repairs were done by the contractor?—(*Capt. Peel.*) Yes.

4880. Mr. Mayes attending on behalf of the company?—Yes.

4881. (*Chairman to the Witness.*) Has your experience in picking up this cable led you to think that it is desirable in any future cables to cover the iron wire at all with any outer covering?—I do not think that any covering I have seen would protect it long.

4882. Why not?—I think the iron would rust unless it were covered with something impervious to water.

4883. The iron would decay?—It would suffer from ordinary corrosion generally, and probably from galvanic action on particular kinds of bottom. I should prefer using stout iron wires to covering with smaller ones.

4884. Was it very much corroded where the marine animals were found?—No, no more than in other places.

4885. (*Mr. Bidder.*) Is there a perceptible amount of corrosion on the cable?—Certainly, it can be seen.

4886. (*Chairman.*) You said, I think, to the extent of as much as an eighth?—Yes, of the total strength of the cable.

4887. (*Mr. Fairbairn.*) Did you notice any injury or decomposition of the insulators?—None whatever.

4888. Suppose that the cable was entirely destitute of iron outside, what do you think would be the effect?—I think it would sustain no injury, except near the shore.

4889. When you get into deep water I suppose, as far as your experience enables you to judge, you think it would be as well without the iron?—Quite as well; but you could not repair it if any faults should develop themselves in the insulator.

4890. (*Chairman.*) Your experience leads you to think that it is advisable to lay down a cable of such strength that it can be repaired?—I think so. Wire covering is also a great safeguard against accidents in handling the cable and in paying it out.

4891. (*Mr. E. Clark.*) Were those places in which the iron wire was quite gone bare and exposed to the water, I mean by straining?—Yes.

4892. In those parts was the insulation good?—It was not defective from the gutta-percha being chafed.

4893. For what length would the gutta-percha be bare in such places as you have mentioned?—In some places a foot.

4894. (*Chairman.*) Could you bring the cable up to the surface, holding by the gutta-percha alone?—In several cases all the wires were broken, and the gutta-percha bore the strain of recovery in 20 and 30 fathoms water.

4895. (*Mr. E. Clark.*) Are you certain in that case that the breaking of the wire was not your own act in drawing the cable up?—The wires were doubtless broken in that way sometimes, but by far the greater part were old breaks.

4896. (*Mr. Fairbairn.*) Could you distinguish between a new breakage and an old one?—Yes; the difference was quite plain.

4897. (*Mr. Saward.*) If the cable were laid down without any iron surrounding it at all, it would be easily picked up, would it not?—No; I am afraid there would be great difficulty in picking it up and doing any repairs, in consequence of its having no strength.

4898. Would not its lightness enable it to be easily raised?—No; in many cases a strain would come upon it from other causes than its weight.

4899. From surrounding rocks?—Yes; and possibly a boat may be riding by it; there are many accidents in paying it out that the unprotected gutta-percha would be liable to.

4900. Supposing it were surrounded with hemp, what should you think of that?—I think that a hempen cable might be used in deep water, where the cable has not to be recovered.

4901. You think that it would give no greater facility for recovering than the iron?—Not so much. I do not think any cable could be made so strong in proportion to its weight in water as a hemp-cord. In carrying out deep sea-soundings, stout sounding-lines were frequently broken, and it is certain that they were broken from other causes than their weight.

4902. (*Mr. E. Clark.*) Was the gutta-percha and yarn, when bare, covered with any vegetation or any animalculæ?—The comparatively small quantity of gutta-percha exposed was not much covered with weed; the weed seemed to adhere to the iron more than to the gutta-percha.

4903. It had adhered to the gutta-percha to some extent?—Slightly.

4904. Showing that it was an old exposure?—Yes, the yarn was soddened and dirty; as though it had been exposed for some time.

The witness withdrew.

Adjourned.

APPENDIX.

APPENDIX No. I.

On the CIRCUMSTANCES which influence the INDUCTIVE DISCHARGES of SUBMARINE TELEGRAPHIC CABLES.—By C. WHEATSTONE, F.R.S.

Report by Professor Wheatstone.

I.

On the circumstances which influence the inductive discharges of submarine telegraphic cables.

The introduction, in late years, of submarine telegraphic cables, and the substitution of subterranean lines of considerable lengths for the aërial lines almost exclusively employed formerly, have brought into evidence certain conditions of electrical charge which so materially influence the rapidity and frequency of the signals transmitted as to make it an object of the highest importance that they should be strictly investigated, in order that it may be ascertained whether it be possible to obviate the evil effects arising therefrom, or in any degree to alleviate them.

These conditions could not fail to come under the observation of persons employed in telegraphic operations, as soon as such lines were constructed; but before they were made the subject of investigation by Mr. Werner Siemens* and Dr. Faraday,† only vague notions respecting their cause prevailed.

When a metallic wire is enveloped by a coating of some insulating substance, as gutta-percha or india-rubber, and is then surrounded by water or damp earth, the system becomes exactly analogous to a Leyden jar or coated pane: the insulating-covering represents the glass, the copper wire the inner metallic coating, and the water or moist earth the external coating. The electricity with which the wire is charged, by bringing the pole of an active battery in contact with it, acts by induction on the opposite electricity of the surrounding medium, which in its turn re-acts on the electricity of the wire, drawing more from the source, and a considerable accumulation is thereby occasioned, which is greater in proportion to the thinness of the insulating covering.

One mile of copper wire, one-sixteenth of an inch in diameter, presents a surface of 85·95 square feet, and, the inductive circumstances being assumed to be the same, receives the same charge from a source of the same tension as a Leyden jar having an equal number of square feet of tinfoil coating. There is, however, one material difference between the two cases. Though both are discharged in a time inappreciably minute to the senses, the discharge from the wire occupies a comparatively much longer interval than that from the coatings of the jar.

A wire on insulating supports, in the open air, when it is unconnected with the earth, receives also a charge, but very much smaller in amount, the inductive action of distant surrounding bodies exerting but little influence upon it.

Although certain general conditions of this inductive action in telegraphic wires have been made the subject of experiment, and are now well known, our knowledge in regard to this subject still remains very limited.

It was therefore thought desirable that a series of experiments should be instituted, under circumstances as closely resembling those which take place in actual telegraphic lines as possible, in order to determine the amount of inductive discharge in wires of very considerable lengths, according to variations in the lengths of the wires, their diameters, the material and thickness of the insulating substance, and in the temperature and pressure of the medium by which they are surrounded.

For the purpose of these experiments the following coated wires were procured:—

Sixteen miles of copper wire, 1/16 of an inch in diameter, with a coating of gutta-percha 3/32 of an inch in thickness.

Nine wires, each one mile in length, of various diameters coated with gutta-percha of different thicknesses. Duplicates were provided of five of these wires.

Fifteen wires, each one mile in length, and 1/16 of an inch in diameter, coated with different insulating materials, prepared by different manufacturers, as hereafter described.

In all, 45 miles of insulated wire.

These lines were formed into large coils, and placed in a tank filled with water. The ends of each mile were brought into the operating room, for the purpose of being connected with the battery, the instruments, and the earth communications, as occasion required.

The battery, which was on Daniell's principle, consisted of 512 pairs of plates; and during the course of the experiments it was so arranged that either 1, 2, 4, 8, 16, 32, 64, 128, 256, or 512 elements might be brought into action at pleasure. Every care was taken to have the battery well insulated.

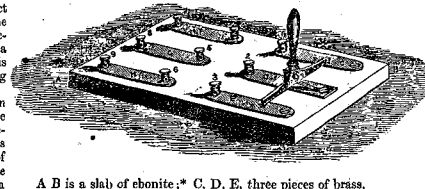
2.

Instrument for charging and discharging Wires.

Apparatus for charging and discharging wires.

The charging and discharging of the wires were effected by the simple apparatus represented by fig. 1.

Fig. 1.



A B is a slab of ebonite;* C, D, E, three pieces of brass, furnished with binding screws 1, 2, 3; to the middle piece is attached a double lever key, by means of which it may be brought at will into metallic communication with either of the external pieces. The part of the key to be touched by the finger is also made of ebonite. F, G, H, are three insulated pieces of brass, each having two binding screws, 4 7, 5 8, and 6 9.

A short wire from one pole of the battery, the other being connected with the earth, is to be attached to the binding screw 7; the insulated wire, whose charge or discharge is to be measured, to 8; and 9 is to be connected by a wire to the earth.

To ascertain the amount of *charge* communicated to the insulated wire, a galvanometer must be inserted between 1 and 4. On bringing the key into contact with C, the charge of the battery is communicated to the wire, and the amount is indicated by the galvanometer.

To measure the *discharge* from the insulated wire, the galvanometer must be placed between 3 and 6. The key is first brought into contact with C, by which the wire is charged; it is then brought into contact with E, which operation discharges the wire through the galvanometer into the earth.

If the galvanometer be interposed between 2 and 5, the currents arising both from the charge and discharge are successively indicated by the galvanometer.

The binding screws 1 4, 2 5, 3 6, when not connected by a galvanometer, must be united by short wires.

* Annales de Chimie et de Physique, 3ème serie, tome 29, 1850.
† Proceedings of the Royal Institution, January 29, 1854, "On electric induction, associated cases of current, and static effects."

* Ebonite is an indurated preparation of india-rubber, and is an excellent insulator.

App. No. 1.

Report by Professor Wheatstone.

Galvanometers employed in experiments.

Galvanometers of very different constructions were employed at various times in these experiments, which lasted several months. As the tension of the battery was subject to considerable variations at different times from a variety of causes, and as the needles of the galvanometers were occasionally affected by powerful currents sent through them, which occasioned changes in the directive power, no comparison can be safely made between experiments with the same instruments made at distant times. Every precaution, however, was taken to ensure the perfect comparability of the experiments of each group made at the same time.

The galvanometers employed were as follows:—

A galvanometer with a pair of astatic needles suspended by a silk fibre without torsion. Length of wire 80 yards; diameter of wire 1/40 of an inch - - - - - A.

A similar galvanometer, the weight of the needles only being different - - - - - B.

A galvanometer with very long wire, the resistance of which is stated by its constructor (Mr. Henley) to be equal to 300 miles of the ordinary telegraphic wire - - - - C.

Another galvanometer, with very long wire - D.

A galvanometer with needles not astatic suspended by a silk fibre, having 30·500 coils of wire 1/500 of an inch in diameter - - - E.

A torsion galvanometer with astatic needles suspended by a glass thread. Length of wire 130 yards; diameter of wire 1/40 of an inch - F.

The currents produced during the charging or discharging of wires, even of many miles in length, are of such extremely short duration, that they cannot affect the needle of a galvanometer in a permanent manner; we are, therefore, obliged to take as their measure the instantaneous maximum deflection of the needle which they occasion.

The forces which produce these deflections are, as in the similar case of the ballistic pendulum, generally assumed to be as the chord of the arc through which the needle passes. The arcs themselves, when they do not surpass 30° or 40°, may, for these experiments, which do not admit of the utmost accuracy, be considered as sufficient approximations.

4.

Influence of the Electro-motive Force of the Battery on the amount of Discharge.

Influence of the electro-motive force of the battery on the amount of discharge.

A variety of experiments were made with lines varying from one to 16 miles in length, and with batteries varying from 16 to 512 elements; they all led to the same result, that the amount of discharge is proportionate to the electro-motive force of tension of the battery. I will record one series of experiments, in which the galvanometer A was employed, in proof of this:—

Number of Elements.	One Mile.	Eight Miles.	Sixteen Miles.	Peltier's Electrometer.
	°	°	°	°
16	..	2·5	5	..
32	..	5·	10	..
64	..	10·	20	14
128	2·5	20·	41	28
256	5·	41·	88	..
512	10·	88·	..	..

It will be found that the chords of these angles are proportionate to the electro-motive forces, or number of elements of the battery.

The fifth column gives the tensions of the battery ascertained by Peltier's electrometer; the high tensions are not given, because beyond 30° the forces are no longer in proportion to the angles.

5.

Influence of the Length of the Wire on the Inductive Discharge.

Influence of the length of the wire on the inductive discharge.

In the following experiments the discharge was taken successively from 1, 2, 3, and 4 miles of copper wire 2/32 of an inch in diameter, covered with gutta-percha, 3/32 of an inch in thickness. A battery of 504 elements was employed to charge the wires, and two short wire galvanometers (A and B) were used for obtaining the measures. The numbers in the first and second columns were obtained with the same galvanometer, but the tension of the battery was different in the two cases: the galvanometer B had a more sensitive needle.

	Galvanometer A.		Galvanometer B.
1 mile - - -	7·5	7	11·
2 miles - - -	15·	14	22·5
3 miles - - -	22·5	21	33·3
4 miles - - -	30·	28	43·

The discharges from one and two miles were also compared when the wires had different diameters and different thicknesses of insulating covering.

Diameter of Copper Wire.	Thickness of Gutta-percha.	One Mile.	Two Miles.
2/32	3/32	8	16
2/32	6/32	6	12
2/32	12/32	5	9·5
4/32	6/32	8·5	17
8/32	6/32	13	25·5

From these experiments, it is evident that the discharge is directly as the length of the wire.

In the galvanometers employed when the deviations do not exceed 30°, the angles may, without sensible error, be substituted for the chords.

When galvanometers with very long wires are employed, equally correct results are not obtained; they give for ascending degrees numbers considerably in excess, as the following table will show. A battery of 72 elements was employed with the galvanometer C, and one of 144 elements with the galvanometer D.

	Galvanometer C.	Galvanometer D.
1 mile - -	24, 24, 23	10, 10, 10, 11.
2 miles - -	49, 49, 52, 49	23, 24, 23
3 miles - -	89, 89	35, 36½, 38
4 miles - -	134, 134, 132	52, 52, 50

Assuming that the discharges are proportionate to the lengths of the wire, and that the chords of the angles accurately measure these discharges, the angles should have been:—

	Galvanometer C.	Galvanometer D.
1 mile - -	24	11
2 miles - -	49	22
3 miles - -	77	33
4 miles - -	110	45

6.

Simultaneous Discharges.

Simultaneous discharges.

The following experiments show that when a discharge is effected simultaneously from a number of wires united at their discharging ends, the effect in producing the instantaneous deflections on a galvanometer is the same as when the wires are all united together to form one continuous length. No doubt when four miles of wire are simultaneously discharged, the discharge is effected in one-fourth of the time that it is from the same wires united continuously; but as the intensity is at the same time increased four times, the total quantity, which is the intensity multiplied by the time, remains the same, and produces the same deflection of the needle of the galvanometer.

The battery of 504 elements and the galvanometer A were employed for these experiments.

	In continuous length.	Side by side.
1 mile - -	7·5	7·5
2 miles - -	15·	15·
3 miles - -	22·5	22·
4 miles - -	30·	30·

The duration of discharges from very long wires is too continuous to be measured by the maximum instantaneous deflections of the needle of a galvanometer.

7.

Influence of the Conductivity of the Wire.

Influence of the conductivity of the Wire.

An iron wire, one mile in length, 4/32 of an inch in diameter, and with a covering of gutta-percha 3/32 of an inch in thickness, was compared with respect to the amount of its discharge, with a copper wire of the same diameter, and similarly coated. Employing the same battery force and and the same galvanometer, the discharge was in all cases found to be the same in both lines. It appears from the particulars furnished by the Gutta-percha Company, that the weights of the gutta-percha coatings on the two wires were not precisely the same, that on the iron wire being 150 lbs. to the mile, while that on the copper wire was only 134 lbs. to the mile; the difference of thickness hereby occasioned would not, however, materially influence the result.

The conductivity of the metal, other circumstances remaining the same, therefore, does not seem to influence the amount of the discharge.

Iron affords eight times greater resistance to the passage of the voltaic current than copper does. It has been proposed in the construction of submarine cables to substitute for the copper conducting wire an iron wire, increasing its thickness so as to render its conductivity equal to the former; but though the force of the current would remain the same, the induction would increase with the diameter o the wire, and a positive disadvantage would ensue.

Care should be taken to procure for submarine telegraphic lines copper wire of the *highest* conductivity. It will be seen in Dr. Mathieson's report that the copper wires of commerce vary extremely, from 100 the standard to 14·24. By employing a bad conductor of the same diameter, the resistance of the circuit is increased, while the induction remains the same.

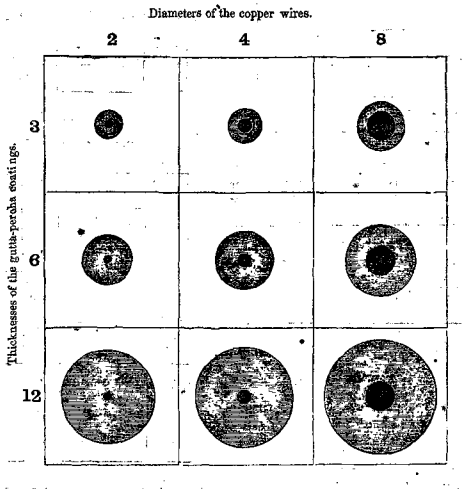
8.

Influence of the Diameter of the Wire and the Thickness of the Insulating Coating on the Amount of Discharge.

Influence of the diameter of the wire and the thickness of the insulating coating on the amount of discharge.

In order to ascertain in what manner the diameter of the wire and the thickness of the insulating coating affects the amount of discharge, the Gutta-percha Company was requested to prepare nine experimental lines of copper wire covered with gutta-percha, each one mile in length, the wires having various diameters, and the gutta-percha covering various thicknesses, as shown in the following *table*. The figures in the horizontal row indicate the former, and those in the vertical row the latter; the unit is the thirty-secondth of an inch.

Fig. 2.



When the lines were manufactured, the quantities of gutta-percha in the various coverings were found not to be so exact as they ought to have been. To enable corrections to be made, I have given in the following table the weights of the gutta-percha coverings given by the company, and also the relative weights corresponding to the thicknesses intended, the line 2/3 being taken for the point of comparison. The upper figures represent the calculated weights, those below them the weights given by the company, and the lowest their differences positive or negative. The actual weights were determined in the following manner:—The coils of coated wire were weighed, and the weight of the copper wire having been previously calculated, it was deducted from the gross, and the remainder given as the weight of the gutta-percha. "It is almost impossible," says the manager of the company, "to put gutta-percha on "the wires with sufficient accuracy to ensure two miles "having precisely the same weight; copper wire will vary "too in the same length."

In this table the weights are given in pounds:—

Gutta-percha coating.	Copper wire. 2	4	8
3	100	140	220
	100	134	159
	0	6	61
6	320	400	560
	338	408	579
	−18	−8	−19
12	1100	1280	1600
	1120	1300	1636
	−20	−20	−36

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Influence of the diameter of the wire and the thickness of the insulating coating on the amount of discharge.

The table which follows gives the ratios of the two numbers, or the calculated divided by the actual weights. The greatest discrepancy is in the line 8/3.

	2	4	8
3	1·000	1·044	1·382
6	·946	·980	·967
12	·982	·984	·978

To control in some degree the weights given by the company, a six-inch length of each of the lines was cut off, and their gutta-percha coatings were accurately weighed. The following table gives these weights in grains, compared with the relative weights corresponding with the calculated thicknesses. The upper figures represent the calculated weights; as before 2/3 is taken as the point of comparison.

	2	4	8
3	66 66 0	92·4 93· −·6	145·2 143· 2·2
6	211·2 229· −17·8	264· 265· −1	369·6 385· −15·4
12	739·2 712· 27·2	844·8 878· −33·2	1056 1090 −34

The next table shows the ratios of the calculated and actual weights given in the above table.

	2	4	8
3	1·000	·993	1·053
6	·922	·996	·960
12	1·038	·962	·968

It will here be seen that, in the case of 8/3, the actual weight more nearly corresponds with the calculated weight than that rendered by the company. This fact, added to the results of the experiments hereafter recorded, inclines me to think that there is some error in the statement of the gross weight of this wire given by the company; but on this point I have not been able to obtain any satisfactory information.

Many series of experiments were made at various times to measure the discharges from these nine lines. Battery powers from 32 to 512 elements were employed, and the measures were made with galvanometers of very different constructions. The following tables exhibit some of these results. Those on the left-hand side give the angular deflections of the needles, and those on the right-hand side the corresponding chords of these angles. The arrangement of these tables allows the influence of the diameter of the wire and of the thickness of the gutta-percha coating to be separately seen by a simple inspection.

First Series.

Battery of 512 elements—Galvanometer A.

	2	4	8		2	4	8
3	7·2	12·0	15·7	3	62	104	136
6	5·0	7·9	12·0	6	43	69	104
12	3·8	5·0	7·9	12	32	43	69

Second Series.

Battery of 512 elements—Galvanometer F.

	2	4	8		2	4	8
3	11·2	18·0	24·0	3	97	156	207
6	7·8	11·8	18·0	6	67	102	156
12	5·8	8·2	12·0	12	50	71	104

Third Series.

Battery of 128 elements—Galvanometer D.

	2	4	8		2	4	8
3	15·9	25·0	31·0	3	138	216	267
6	11·2	16·0	25·0	6	97	139	216
12	9·0	12·0	16·6	12	78	104	144

Fourth Series.

Battery of 128 elements—Galvanometer E.

	2	4	8		2	4	8
3	25·5	40·0	53·0	3	220	342	446
6	18·7	27·0	41·0	6	162	233	350
12	14·0	19·5	27·0	12	121	169	233

Fifth Series.

Battery of 64 elements—Galvanometer C.

	2	4	8		2	4	8
3	13°	22	30	3	113	190	253
6	8·5	13°	22	6	74	113	190
12	6	9	13·5	12	52	78	117

Sixth Series.

Battery of 512 elements—Galvanometer B. with changed needles.

	2	4	8		2	4	8
3	9	14	18·5	3	78	121	160
6	6	9	14·5	6	52	78	126
12	5	6·5	9·5	12	43	61	82

Comparing all these groups of experiments together, we shall not be far from the truth, if we assume, as an empirical law, that the amount of discharge from wires of different diameters, with coverings of various thicknesses of the same insulating material, is directly as the square root of the semi-diameter of the wire, and inversely as the square root of the thickness of the insulating envelope. At any rate, this law is approximatively true for every proportion that can be practically employed in telegraphic operations.

If we look more minutely to the results, it appears that the discharge increases a little more rapidly than the square root of the radius of the wire, and decreases a little more rapidly than the square root of the thickness of the gutta-percha; and that when the wire and gutta-percha increase in the same proportion, the discharge does not remain exactly the same, but increases slightly as the total diameter increases; but these differences may have arisen from variations in the diameter of the wires and thickness of the insulating material, in the manufacture of the wires.

There is one practical consequence of some importance to be inferred from these results. It is seen that by increasing the diameter of the wire and the thickness of the insulating covering in the same proportion, the amount of inductive discharge remains the same. According to Ohm's

law, the force of the current in a voltaic circuit, when the length of the circuit remains the same, and the resistance of the battery is so inconsiderable as to be disregarded when compared with that of the metallic portion of the circuit, increases as the square of the diameter of the wire. We have, therefore, a means of increasing the strength of the current without augmenting the effects of induction. If it be found inconvenient in practice to increase both the wire and insulating covering proportionably, greater advantage will be obtained by increasing the diameter of the wire than the thickness of the gutta-percha, for while the latter remains the same, the inductive discharge increases only as the square root of the diameter of the wire, while the force of the current increases as the square of the diameter; whereas, if the diameter of the wire remains the same, and the thickness of the gutta-percha be made to vary, the strength of the current will remain the same, while the induction will only decrease as the square root of the thickness. If the resistance of the battery be considerable, the advantage arising from the thickness of the wire will not be so great.

9.

On the Influence of the Insulating Material on the Amount of Discharge.

Influence of the insulating material on the amount of discharge.

Gutta-percha is the insulating material which has been hitherto almost exclusively employed in the manufacture of submarine and subterranean telegraphic lines. Wires coated with india-rubber have, however, in some places been brought into use, and recently numerous patents have been obtained for compound materials, into which either gutta-percha or india-rubber largely enters, or for interposing other substances between the wires and the gutta-percha or india-rubber coating, or between different coatings of these materials.

To ascertain the comparative advantages and disadvantages of these various materials with respect to their inductive and insulating properties, the manufacturers of them were requested by the Committee to furnish, for the purposes of experiment, wires one mile in length and one-sixteenth of an inch diameter, coated according to their respective processes. The following is a list of the manufacturers who responded to the application, and upon whose coated wires the comparative experiments were made:—

	Thickness of coating.
1. The Gutta-percha Company:	
An improved preparation of gutta-percha	3/32
Standard gutta-percha	3/32
Do., with coating of double thickness	6/32
Ten coatings of gutta-percha, alternating with 10 coatings of Chatterton's compound	6/32
2. Messrs. Silver and Co.:	
Masticated india-rubber, cut into narrow slips, which are coiled spirally round the wire, and are cemented by a heat not exceeding that of boiling water - A	3·86/32
Do. - B	5·20/32
Do. - C	6·25/32
3. Messrs. Hall and Wells:	
Three coatings of pure india-rubber covered externally with vulcanized india-rubber thread, with cotton thread interposed between the wire and the coating - A	3/32
One coating next the cotton covered wire of pure india-rubber, three coatings of manufactured india-rubber, and one externally of vulcanized india-rubber thread - B	3/32
4. Mr. Leonard Wray:	
Wray's compound, consisting of india-rubber, shell-lac, powdered flint, and gutta-percha	6/32
5. Mr. Hughes:	
Two coatings of gutta-percha, with a viscid substance, said to be prepared from coal, interposed between the two coatings	6/32

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	Thickness of coating.
6. Mr. Radcliffe:	
A preparation of gutta-percha with carbon, for the purpose of increasing its durability - A	6/32
Do. - B	6/32
7. Mr. Godefroy:	
A compound of gutta-percha and pounded cocoa-nut shell	3/32

As standards with which to compare the lines above enumerated, two wires coated with ordinary gutta-percha, one 3/32 and the other 6/32 of an inch, were similarly experimented upon at the same time.

In the following table the wires coated with different materials are arranged according to the thickness of their coatings. In all cases the copper wire was one-sixteenth of an inch in diameter.

Column A. gives the discharges according to my own experiments. A battery of 512 elements and the galvanometer were employed. I made a great number of experiments to measure the discharges from these lines, employing batteries of different powers and galvanometers of various kinds: the results were in satisfactory accordance, especially so far as the order of the inductive capacities was concerned.

Column B.—Another series of measures of discharges. These results are taken from Mr. Bartholomew's tables, temperature 52° F. A battery of 128 elements and the galvanometer were employed.

Column C.—Loss of insulation for dynamic electricity at 52° F., extracted from Mr. Rowland's table.

Column D.—The number of minutes and seconds in which the line lost half its charge of static electricity. Taken from Mr. Bartholomew's table; temperature 52° F.

—	Thickness of coating, unit=1/32 of an inch.	A.	B.	C.	D.
		○	○	○	′ ″
Silver's C. - - -	6·25	4·5	10·3	0′	79 0
Improved gutta-percha C. -	6	4·5	10·7	0′	11 15
Wray's - - -	6	4·5	11·4	0′	50 0
Gutta-percha alternate -	6	6	14·4	″43	6 40
Hughes' - -	6	6	15·1	16″3	0 40
Standard gutta-percha -	6	6	15·5	0′16	0 55
Radcliffe's B. - -	6	6·5	16·6	14′16	0 30
Radcliffe's A. - -	6	7	17·0	10′16	0 29
Silver's B. - -	3·2	3	11·2	0′	23 2
Silver's A. - -	3·86	6	—	—	—
Improved gutta-percha B. -	3	7	15·8	4′6	4 57
Improved gutta-percha A. -	3	6·5	16·1	5′	3 55
Hall and Wells' B. - -	3	6·5	16·2	3′	3 42
Hall and Wells' A. - -	3	8·25	22·4	9′	0 51
Standard gutta-percha -	3	9	20·	15′	0 18
Godefroy's - -	3	9·5	22·4	18′16″	0 21

The extra thickness of Silver's india-rubber covered wire C was due, to an external covering of tape.

By comparing the results given in the preceding table several interesting conclusions may be deduced.

1. That india-rubber surpasses all other materials in the smallness of the amount of its inductive discharge and the perfectness of its insulation. In the former respect a coating of india-rubber is fully equal to a coating of ordinary gutta-percha of double its thickness, and in the latter respect its comparative advantage is still greater.

2. That two other materials, viz., Wray's compound, formed by the addition of other highly insulating materials to india-rubber; and the newly manufactured preparation of pure gutta-percha, closely resemble india-rubber in both particulars.

3. That two artificial compositions, formed by the addition of other highly insulating materials to india-rubber or gutta-percha, viz., Wray's compound and the improved gutta-percha, closely resemble india-rubber in both these respects.

4. That the mixture of imperfectly conducting materials with gutta-percha, as carbon in Mr. Radcliffe's composition, or pounded cocoa-nut shell as in Mr. Godefroy's, has the disadvantage of greatly reducing the insulation and increasing the induction.

5. That the interposition of cotton thread between the wire and india-rubber coating, as in Hall and Wells' preparation, also considerably increases the induction and diminishes the insulation. The induction is augmented, because the cotton thread, which is a bad insulator, increases the surface of the conductor; and the insulation is impaired, not only because the insulating coating is dimi-

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nished by the thickness of the cotton, but probably also in consequence of the greater inductive action. The interposition of cotton between two layers of gutta-percha is equally disadvantageous, as is proved by the experiments on Mr. Hearder's short line of 440 yards. See Table II.

6. That the interposition of a viscid insulator between two coatings of gutta-percha neither decreases the induction nor improves the insulation of the line. If Mr. Hughes' process should be found to possess any advantage, it will be in the tendency of the viscid fluid to fill up air holes or flaws in the gutta-percha coatings.

7. Generally speaking the more perfect the insulating property of the material is, the less is its inductive capacity. There are, however, several apparent exceptions to this rule among the experiments quoted, but there are so many causes to affect the experiments on insulation, that we are not warranted to infer, from these exceptions, the entire independence of the two properties.

10.

Influence of Temperature on the Amount of Discharge.

Influence of temperature on the amount of discharge.

Table II. shows the influence of temperatures varying from 32° F. to 92° F. on 29 coated wires of different descriptions. I will confine myself to a few remarks on the results there given.

The imperfectness of insulation increases with the temperature, but there is a great difference in the rate of increase, according to the insulating substance which is made the subject of experiment. Silver's india-rubber maintains an insulation almost perfect up to 92° F., whilst gutta-percha is very considerably affected. The degree in which other substances are affected will be seen in the Table.

Temperature does not, in general, appear to affect the charge in an immediate manner. In most of the lines in which the insulation is but slightly influenced by temperature, the amount of charge remains apparently the same; but when the augmentation of heat occasions a loss of insulation, the charge also increases with the temperature. Wray's compound is the most remarkable exception to this rule; between 32° F. and 92° F. its insulation is very little affected, but its induction varies about 3°. On the contrary, Silver's india-rubber, which retains its high insulation with little or no change, preserves nearly the same amount of discharge from 32° F. to 165° F.

From Mr. Bartholomew's tables it is obvious that the amount of the discharge is generally less than that of the charge. This difference increases as the temperature becomes higher, and if the temperature remains the same, the difference is greater in proportion to the loss of insulation. It cannot be doubted that this difference between the charge and discharge arises solely from imperfect insulation, whether owing to the quality of the material or the increase of temperature. The *charge* is effected instantaneously, and the current arising from imperfect insulation is added to it; while the *discharge* is made after a short interval, during which some portion of the charge has had time to escape. In the best insulating lines, as Silver's C., Wray's, and the improved gutta-percha C., the difference is unapparent at any temperature.

11.

Influence of Pressure on the Inductive Discharge and Insulation.

Influence of pressure on the inductive discharge and insulation.

It does not appear evident that pressure exerts any influence on the amount of the inductive discharge. Comparing together the results recorded in Table VII. of the experiments made with the wires not subjected to pressure with those made with the same wires when the water in which they were immersed was subjected to a pressure of three tons on the square inch, there appear to be greater discrepancies between the experiments made under the same circumstances of pressure at different times than there do between the comparative experiments made under pressure and without pressure.

The influence of pressure on the static insulation is, however, very decided. Pressure greatly improves the insulation, but its effect is more obvious as the material is a worse insulator. There appears to be less difference in india-rubber, when subjected and not subjected to pressure, than upon any other of the materials experimented upon.

12.

Influence of interposed Resistances on the Time of charging or discharging.

Influence of interposed resistances on the time of charging or discharging.

When the charge is communicated to the wire through a very considerable resistance intervening between it and the battery, or when the discharge is effected through a great resistance to the earth, the time of charging or discharging the wire is very greatly augmented.

A wire from one of the poles of a battery of 512 elements was attached to a binding screw on an insulated mahogany frame, the other pole communicating with the earth; to another binding screw, about half an inch apart from the first, was attached the end of an insulated wire two miles in length. The charge was allowed to accumulate in the wire during different intervals of time, and the discharge current was conducted to the earth through the long wire galvanometer (B)

After	¼ minute	the deflection was 12°
	½ minute	„ 19°
	1 minute	„ 32°
	2 minutes	„ 42°
	4 minutes	„ 55°

The experiment was not tried beyond. When the mahogany frame was removed from the circuit the deflection was immediate and beyond 90°.

In like manner, on interposing a very imperfect conductor between the charged wire and the earth the discharge was retarded so as to last upwards of two minutes. The effect was particularly striking, and presented some instructive particulars, when the discharge was effected through one of Mr. Gassiot's small vacuum tubes.

From these experiments it might be inferred that the interposition of a galvanometer wire, especially when it is of great length, would sensibly retard the time of charging or discharging. It no doubt does retard it, but when the current is so instantaneous as only to produce a momentary deflection of the needle the same quantity affects the needle in the same manner whether the time of charging or discharging be greater or less. A direct experimental proof of this is given in § 6. The following experiment is also in accordance with this explanation.

One mile of wire 2/3 was charged by a battery of 144 elements, and then discharged to the earth through the long wire galvanometer (B); the needle deflected 67°. Four, eight, and 16 miles of similar wire were successively interposed between the galvanometer and the earth, but in all cases the discharge occasioned precisely the same deflection of 67°.

Great as was the resistance of sixteen miles of wire added to that of the galvanometer wire (stated by the constructor to be equal to 300 miles of ordinary telegraphic wire), it was very small in comparison with the resistances first mentioned. When the time of charging or discharging is less than the time of deflection of the needle that deflection is a measure of the quantity of dynamic electricity in the instantaneous currents; but when the time is greater, as in the first quoted examples, the quantity is to be measured by the intensity indicated by the permanent deviation of the needle multiplied by the time.

13.

Discharges from one end of a Wire when the other communicates with the Earth.

Discharges from one end of a wire when the other communicates with the earth.

The current produced by discharging to the earth the static electricity of a charged insulated wire has been considered in the preceding sections, and it has been shown to be governed by simple and well-ascertained laws. But in the actual transmission of telegraphic signals this condition of the insulation of the wire at its remote end never obtains. When a telegraph is in operation the wire is always connected with the earth at its opposite extremity. The state of tension is in this case never arrived at, because the current continues to flow, and at the moment of discharging the wire only one portion of the electricity at that moment existing in it is discharged through the new path, the other portion passing from the opposite end of the wire to the earth, with which it is permanently connected. It is of great practical importance that the proportion of the discharge from a wire connected with the earth, as compared with the discharge under the same circumstances from the same wire disconnected from the earth, should be determined. For this purpose I instituted a number of experiments with the wires with which the previous experiments were made; but I met with the most anomalous results, from causes to which I shall hereafter refer. I therefore found it necessary, in order to obtain reliable results, to make the comparison on an actual submarine or subterranean telegraphic line. With this view, W. Andrews, Esq., the Engineer of the Submarine Telegraph Company, kindly placed at my disposal four subterranean wires extending from London to Dover, a distance of 89 miles. They were insulated by gutta-percha, and enclosed in the same protective covering. The instruments employed in the ensuing experiments were the following:—The torsion galvanometer; a Peltier's electroscope; and the discharging key, Fig 1. The

wires were marked respectively 2, 4, 5, and 10; and a battery of 81 Daniell's elements, which had been some time in action, was employed.

The electroscope applied to a short wire attached to the copper terminal of the battery, while the zinc terminal was connected with the earth, gave a deviation of 11°. When either of the wires, No. 2, 4, or 5, insulated at its opposite end, was attached to the battery, and the electroscope touched at the point of junction, the deviation remained the same, viz., 11°. The wires retained no charge when removed from the battery. The battery, however, supplied the electricity of tension faster than it was dissipated from the wires. The battery was very imperfectly insulated; the elements were contained in porcelain jars standing in wooden boxes with covers.

To ascertain the leakages of the respective wires for dynamic electricity, the torsion galvanometer (E) was interposed in succession between Nos. 5, 4, and 2, and the battery, the opposite ends of the wires remaining insulated. The following were the deflections and corresponding degrees of torsion:—

	Deflections.	Degrees of torsion.
No. 4	81°	570
No. 2	51½°	80
No. 5	47°	61

No. 4 leaked, therefore, above three times more than No. 5, the force of the current being as the square root of the torsion.

I then proceeded to ascertain the amount of discharge from each wire when insulated at its opposite end. The same torsion galvanometer was employed to measure the instantaneous deflections, and the charging and discharging was effected by the discharging key, fig. 1. The following were the results:—

	Instantaneous Deflections.*
No. 4	72, 85, 80, 87
No. 2	130, 130, 130
No. 5	141, 140, 141

It is obvious here that the wire which was the worst insulated gave the smallest amount of discharge, owing, no doubt, to the escape of a portion of the charge.

It appears also from the above experiments, that when the insulation is imperfect, the amount of the discharge is less constant.

The ends of the wires at Dover were then connected with the earth, and similar experiments were made, with the following results:—

	Instantaneous Deflections.
No. 4.	18°, 20°, 31°, 20°.
No. 2.	17°, 18°, 20°.
No. 5.	14°, 12°, 18°, 12°, 15°.

The amount of discharge is much less than when the opposite ends of the wires are insulated, in the case of No. 2 it is about seven times less. There is also greater variability when the insulation is tolerably good, and imperfect insulation, instead of reducing the discharge, appears to augment it. The discharge from a perfectly insulated wire connected with the earth is by no means so accurate a measure of the inductive action as the discharge from a charged wire disconnected from the earth.

Wishing to make similar experiments with a double length of wire, Nos. 2 and 5 were connected together at Dover, without communicating with the earth there; the two ends of the conductor were, therefore, at London. The battery and key were first applied to the end of 5, and afterwards to the end of 2; in the first case the end of 2 remained insulated, and in the second case the end of 5.

The following measures were obtained:—

	Instantaneous Deflections.
The end of 2 insulated; the battery and key applied to the end of 5	242, 206, 220.
The end of 5 insulated; do. to the end of 2	175, 172, 166.

The first gives an amount of discharge nearly double that of a single wire, as might be expected; the second shows a result considerably less. The cause of this discrepancy, from the complicated conditions of the experiments, it is difficult to explain; the wires were not equal in insulation, and they might exert some inductive action on each other from being close to each other in the same sheaf. Corresponding experiments were then made with the ends previously insulated communicating with the earth.

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The end of 2 connected with the earth; the battery and key applied to the end of 5	97°, 84°, 110°, 112°.
The end of 5 connected with the earth; do. to the end of 2	70°, 69°, 71°.

The diminution of the discharge from the earth connexion, though considerable, is not so great as when half the length of wire is employed. The same proportion is kept between the measures obtained when the battery is applied to the ends 5 and 2 as in the preceding experiments.

14.

True and apparent Discharges.

Anomalous results which occur in obtaining a discharge from a wire in connexion with the earth, when made from charged wires previously insulated at their two ends.

I will now revert to the anomalous results which occur when it is attempted to obtain a discharge from a wire in connexion with the earth, under the same circumstances as when the discharges are made from charged wires previously insulated at their two ends, as detailed in sections 1–9.

In general, strong deviations of the galvanometer are produced very much stronger than those resulting from the charge of the same wire when insulated at its ends; but these deflections are capriciously variable, occurring sometimes in the deviation of the true discharge current, and at other times in the opposite direction. These effects have been observed by Mr. Webb, and Mr. Fleming Jenkins, and they have also attracted the attention of Professor W. Thomson. All these gentlemen attribute them to a conflict between the true discharge and a discharge of an inductive effect arising from the mutual action of the coils of the wire upon each other; the experiments from which they have drawn their conclusions having been made on large coils of insulated wire.

To ascertain the real causes of these perplexing phenomena, I instituted a series of experiments, which determined the following points:—1st, the length of wire remaining the same, the deviations of the galvanometer increased with the electro-motive force of the battery up to a certain limit. When the electro-motive force was small, *i.e.*, when one or two elements were employed, the deviations were generally constant in one direction; but when it was increased, the variations of direction became very capricious. 2nd, I compared the true discharge from wires of 1, 8, and 16 miles in length, insulated at their ends, with the apparent discharge from the same wires in connexion with the earth. The following Table shows the results:—

No. of Elements of the Battery.	One Mile.	Eight Miles.	Sixteen Miles.
	°	°	°
	True Discharge.		
32		5	10
64		10	20
128	2·5	20	41
256	5	41	87
	Apparent Discharge.		
1	90	70	25

While the true discharge increases with the length of the wire, the apparent discharge decreases; the latter therefore cannot depend on an inductive action. It has been proved in section 13 that the discharge from a wire with its end in connexion with the earth is much less than that of the same wire disconnected from it; but the above Table shows that the apparent discharge from one mile of wire, when one cell of the battery is employed, is incomparably greater than the real discharge from the same wire when 32 or even 256 cells are brought into action.

The real origin of these currents will become evident, if instead of momentarily depressing the discharging key, which is all that is necessary in experiments on the real discharge, the key be kept permanently depressed; a series of

* When a torsion galvanometer is employed, the force of the instantaneous current is not as the chord of the angle of deflection, as in the case of an ordinary galvanometer; but, as I have ascertained by experiment, it is directly as the angle of torsion with a small constant subtracted.

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Anomalous results which occur in obtaining a discharge from a wire in connexion with the earth when made from charged wires previously insulated at their two ends.

changes will then be observed which will leave no doubt of the cause of the phenomena. In the following experiments a battery of 512 elements, the galvanometer (A) and the discharging key, fig. 1, were employed; the copper pole of the battery was connected with the earth, the zinc pole to 1; one mile of wire 2/3 in the water tank was connected with 2, the other end remaining insulated; the binding screw *b* of the galvanometer was connected with 3, and the binding screw *a* with another earth wire different from that of the battery. The real discharge current was ascertained to be 7°·2 towards the left. The insulated end of the mile of wire was then connected with the earth, 512 elements still continuing to be employed. Pressing the discharging key, and keeping it depressed for some time, a very strong deflection of the needle of the galvanometer towards the left occurred, striking the pin at 90°; the needle then fell continuously, but occasionally with slight interruptions towards 0°, but very much more slowly than the needle left to itself would fall; it took 28 seconds, and then slowly deflected, with occasional jerks towards the right, and acquired a permanent deviation of 36°. The key may be relieved so as to disconnect the wires 1 and 2 from each other without completing the connexion with the battery; if this be done before the inversion has taken place, that is, before the needle has receded to 0°, on renewing the contact the needle will again move towards the left, showing that the effect has a certain permanence in the wire 2; but if this contact be interrupted, and then restored after the deflection has taken place towards the right, the deflection always remains on the right, provided the battery contact be not renewed. Similar experiments were tried with 256, 128, 64, 32, 16, 8, 4, and 1 cells of the battery. The greater the number of cells the greater is the first deflection of the needle towards the left, and of longer continuance. With one cell the first deflection towards the left was 80°, it fell in a second or two to 0°, and afterwards attained a permanent deflection of 15° towards the right. If the wire 2 has been charged with a considerable number of cells, and a strong permanent deviation towards the right been attained, charging it with fewer cells, and then discharging it through the galvanometer, will occasion no deflection towards the left, the first effect from a few cells not being sufficient to overcome the second effect from a larger number.

The following experiment places it beyond doubt that the inductive influence of the coils of the cable in the tank on each other had nothing to do with the effects in question. I substituted for the coiled mile of insulated wire in the tank four feet only of uncoiled wire; the first deflection of the needle was 90° towards the left, which was followed by a permanent deflection of 65° towards the right; a similar experiment was made with a single cell with like results, the deflections only being less.

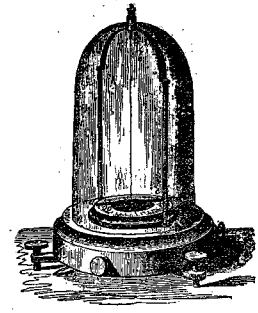
The cause of these effects is the polarization of the plates or masses of metal connecting the ends of the wires with the earth. The first effect is produced by the polarization of the terminal which connects the telegraphic wire with the earth, the second by the polarization of the terminal which connects the galvanometer with the earth. When the wire is sufficiently long for the discharge current from it to affect the galvanometer, the first effect is a complex one, the discharge current being added to the current of polarization. The electro-motive forces of the currents arising from polarization can never exceed that of one or two cells of a voltaic battery, and as the strength of these currents diminishes with the length of the wire, they are not observable in long telegraphic lines unless very delicate indicating instruments be employed.

15.

The Magnetic Rheometer.

The magnetic Rheometer.

The method, already described, of measuring inductive charges and discharges by the momentary deflections of the needle of a galvanometer, requires the employment of batteries of considerable electro-motive force, or wires of great length, in order that the charge may be accumulated, or galvanometers of extreme multiplying power. It is desirable to have a means by which short lengths of wire may be conveniently experimented upon, and I have employed with advantage for this purpose Marianini's rheo-electrometer, or magnetic rheometer. A description of this instrument, and the details of numerous experiments made with it to measure the momentary currents resulting from the discharge of a Leyden jar, will be found in "Memorie di fisica sperimentale scritte dal Professore Marianini dopo il 1836." The instrument I employed was thus constructed:—Two yards of insulated copper wire, 1/120th of an inch in diameter, were coiled round a cylindrical bar of soft iron, three inches in length and one-eighth of an inch in diameter.



This bar was fixed horizontally over a graduated circle at a right angle to the line joining its zero points; at the centre of the bar there was an aperture to allow the free passage of a silk fibre, from which was suspended an extremely delicate magnetic needle, weighing less than half a grain. At the top of the glass cage the upper end of the fibre was attached to a pin, which, by means of an adjusting screw, might be raised or depressed without causing it to rotate, in order to regulate the distance from the needle to the electro-magnetic bar. The ends of the coil of wire were connected to binding screws placed outside the frame, and the graduated circle with the bar was moveable by means of a lever *b*, placed beneath the frame, in order that the zero point might be accurately brought to the magnetic meridian without moving the instrument bodily. Four small pins *c*, *c*, were fixed on the circle in the vicinity of the bar, in order to prevent the contact of the needle with the bar, which it would be otherwise sometimes difficult to disengage from each other.

The mode of using this instrument is as follows:—The zero line of the scale is brought, by means of the lever, into the magnetic meridian; if then the bar is entirely deprived of magnetism, the needle will place itself in that direction. On a discharge being made through the coils, the bar becomes magnetic in proportion to the amount of the discharge; the two poles of the needle are attracted towards the two ends of the electro-magnet, causing an instantaneous deflection, and subsequently a permanent deviation, owing to the residual magnetism, to a less amount. To prepare the instrument for a fresh experiment the bar must be deprived of its magnetism, which is easily effected in the following manner:—One of the poles of a very weak single cell of a voltaic battery is to be attached to one of the binding screws of the instrument, while the other pole is to be brought into successive momentary contacts with the other binding screw, the current being in the opposite direction to that which produced the magnetism of the bar; each contact causes the approach of the needle towards zero, and when it has arrived at that point the magnetism of the bar is completely destroyed, but should the zero be overpassed the same operation must be repeated after inverting the current.

With this instrument the following experiments, among others, were made:—

A Leyden jar, having 380 square inches of tin-foil surface, was charged by a Daniell's battery of 512 elements. On discharging the jar, the rheo-electrometer showed 10° instantaneous and 4° permanent deflection.

One hundred yards of the standard wire, coated with gutta-percha, and immersed in water, were charged by a battery of 60 elements; when the discharge was made through the rheo-electrometer, a deflection of several degrees was shown. In this case the discharge current could not be detected by any of the galvanometers employed in the course of these investigations, except the suspension galvanometer (F) with 30,500 coils employed in Mr. Bartholomew's experiments.

The permanent deflection occasioned by the discharge from one mile of the standard insulated wire when charged by 144 cells of the battery was 8°·25.

16.

Means of accumulating the effects of Charges and Discharges.

Means of accumulating the effects of

The deflection of the needle of a galvanometer increases with the number of discharges in a given time so long as

the time of a single discharge is less than that of a single contact; when a greater rapidity is attained, the deflection remains constant, for then, although each charge or discharge occupies a less time, there is a greater number in the same time, and these two circumstances compensate each other. A succession of instantaneous currents, therefore, produces a permanent deviation of the needle of a galvanometer as a continuous current does, provided that the above condition be fulfilled, and that they follow each other so rapidly that the needle has not time to fall in the intervals between them.

It is, therefore, possible to accumulate the effects of a succession of charges or discharges, and to produce a very considerable deviation of the needle of a galvanometer, when a single charge or discharge would scarcely produce any appreciable motion. For this purpose I have constructed an instrument which I call the *accumulating discharger*, which will also be found useful for a great variety of other experiments.

Fig. 4.

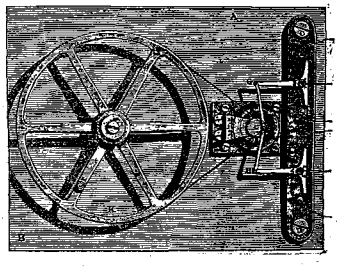
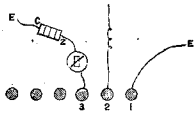


Fig. 4, A, B, is a mahogany board; C and D, two brass pillars which support an insulating plate of ebonite, in which are fixed six brass binding screws 1, 2, 3, 4, 5, 6. A vertical axis carries a small fly wheel E, a pulley F, and, at its upper end, a circular disc, at the circumference of which is attached two ebonite links G, H, freely moving round their common axis; the other ends of these links are attached respectively to two brass arms, which have the pins of the binding screws 2 and 5 for their axes, and each carries a cross piece terminating with springs, which, during the motion of the instrument, press alternately against the binding screws 1 and 3, and 4 and 6. The distances of the springs and pins are so adjusted that each contact lasts during one-fourth of a revolution of the eccentric. A catgut band connects the pulley F with the multiplying wheel K, which is provided with a handle for the purpose of setting it in motion. When this wheel is turned, the eccentric is caused to revolve rapidly, and, in consequence of the arrangements above described, the binding screws 2 and 5 are brought alternately, in rapid succession, in metallic contact, the former with the binding screws 1 and 3, the latter with 4 and 6.

With this instrument, as constructed, it is easy to make 68 double contacts in a second; by employing extra multiplying wheels, a far greater number may be obtained; and, if needful, mechanism with a maintaining power may be employed to regularize the motion.

By connecting, in various manners, the poles of the battery, the wires to be experimented upon, the earth, and the galvanometers, or other measuring instruments, with the binding screws of this apparatus, it may be disposed for obtaining a variety of different results, some of which I will briefly enumerate.

1. To accumulate the *charges* of a wire.



Connect 1 with the earth, 2 with the insulated wire to be charged, 3 with the galvanometer, which is then to be connected with one of the poles of the battery, the other proceeding to the earth. The instrument being put in motion, when the arm connects 2 and 3, the wire becomes charged, and the needle of the galvanometer is acted upon by the electricity in motion; when it connects 2 and 1, the electricity in the wire is discharged to the earth without acting on the galvanometer. The wire is thus alternately charged and discharged, but the accumulated *charges* only act on the galvanometer.

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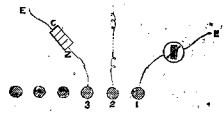
Report by Professor Wheatstone.
Means of accumulating the effects of charges and discharges.

If the earth wire be removed from 1, and the time of charging is less than the duration of a single contact, as is generally the case, no effect is produced on the galvanometer, except from the first contact, because the wire afterwards remains permanently charged.

2. To accumulate the *discharges* of a wire.



Connect the insulated wire with 2; interpose the galvanometer between the earth and 1, and the battery between the earth and 3. The contacts with 3 charge the wire, and the alternate contacts with 1 discharge it. The galvanometer being placed between the earth and 1, is affected only by the accumulated discharges.

With the short wire galvanometer A and 512 cells, the following results were obtained:—

Wire covered with Wray's compound, 170 yards - 3°

Wire with Hearder's covering, 440 yards - - 20°

With the long wire galvanometer (D) and 512 cells, the accumulated discharges from two inches of gutta-percha covered wire, coated externally with tin-foil, were rendered evident.

The following results were obtained with the nine experimental wires of different diameters, and gutta-percha coatings of different thicknesses. The measures were made with the torsion galvanometer (E), and two cells of the battery only were employed. Three readings were taken for each wire.

	2	4	8
3	4·7 4·7 4·6	7·0 7·0 7·0	9·0 9·0 9·0
6	3·2 3·1 3·1	4·9 4·9 4·9	7·0 7·0 7·0
12	2·0 2·0 2·0	3·0 3·0 3·0	4·5 4·5 4·5

With the same measuring instrument no indication would have been obtained with a single discharge.

Experiments were made to ascertain if, by this means the discharge currents could be rendered evident from one or two miles of wire, without any insulating covering suspended freely in the air by means of the ordinary insulating posts of a telegraphic line. For these experiments the long wire galvanometer (D) was employed. With 512 cells of the battery, and two miles of wire, a slight tremor only of the needle was observed on a single discharge; but the intermitting discharger occasioned a permanent deflection of 75°; this, however, was by no means constant; repetitions at different times during the same afternoon gave 65° 70°, 70°, and even 40°. There is not the same steadiness in these results as when a wire with an insulating covering, and immersed in water, is experimented upon. Similar experiments, both with two miles and one mile of wire, were made, employing different battery powers; but with the same variable results. All the measures in the following table were obtained within the four hours in the same day:—

O

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NUMBER OF CELLS OF THE BATTERY.

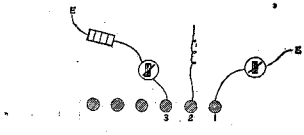
	512	128	64	32
2 miles	40 75 65 70 70	13 32 40	15 10	
1 mile	22 60 65 55 65 70	28	11 9 10 7	4

The accumulated discharges from one mile of gutta-percha covered wire immersed in water, when 64 cells of the battery were employed, gave 65° with the same galvanometer.

During these experiments trials were made of the insulation of the aërial wire. The insulation for dynamic electricity measured by the same galvanometer employed for the discharges was very variable; the escape would sometimes be nil, and after the lapse of some time would attain to 72° when 512 cells of the battery were in action.* This amount of loss would vitiate any result obtained by repeatedly *charging* the wire, for the rapidly reiterated contacts of the battery with the wire produces an effect similar to that of a permanent contact, and the needle of the galvanometer would be affected by the current arising from the escape of electricity along the wire; it would be difficult therefore to distinguish, in this case, what portion of the effect should be attributed to the current arising from imperfect insulation, and what to the current produced by the charges. But with repeated *discharges* the case is different, the action on the galvanometer is then entirely due to the discharges of the electricity retained in the wire after contact with the battery has ceased.

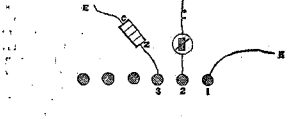
The state of insulation for static electricity of the aërial wire was also ascertained during those experiments. The tension of the battery of 512 cells was shown by a Peltier's electrometer to be 58°. When two miles of the wire were connected with the battery, the same tension was maintained. On removing the battery from the wire and electrometer, the index of the latter fell immediately to 42°, and after the lapse of two minutes to 40°; this experiment was repeated with nearly the same result.

3. To accumulate the charges and discharges of the wire separately at the same time.



Connect the insulated wire with 2; interpose a galvanometer between the battery and 3, and another similar one between the earth and 1.

4. Effect of the alternate *charging* and *discharging* of a wire upon the needle of a galvanometer.



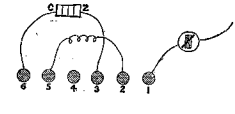
Connect the earth with 1; interpose the galvanometer between 2 and the insulated wire; and connect 3 with one pole of the battery, the other proceeding to the earth.

If the charges and discharges be of equal intensity, we might expect that the rapid succession of the currents in opposite directions occasioned thereby would produce no change in the position of the needle of the galvanometer; and such is the case when either current separately deviates the needle only a few degrees. But a very anomalous effect takes place when the current is stronger. The needle then assumes indifferently a permanent position on either side the zero point; thus with a battery of 512 cells, a wire one mile in length, and the short wire galvanometer (A), this deflection was 75°. It will soon be perceived that the direction of the deflection is determined by the initial impulse. The cause of this singular effect is not difficult to explain. When the needle of the galvanometer is parallel to the coil, currents in either direction act with equal energy, though in opposite directions, on the needle; but when the needle is in any other position, the currents in opposite directions act with unequal energy upon it, and the effect is the same as if it were influenced by two series of alternating currents of different intensities.

I had formerly observed a similar effect in experimenting with Becquerel's differential galvanometer; this instrument is provided with two intermingled coils, each of which transmits a current in the opposite direction. The differential arrangement of an ordinary galvanometer, which I have described in my memoir on "New Instruments and Processes for determining the Constants of a Voltaic Circuit,"* is, however, free from this defect; the neutralization is in that arrangement effected in the coil itself, and all action on the needle is prevented; whereas in the other two instances the needle is influenced, either successively or simultaneously, by two actions in opposite directions.

In the preceding cases, where the wire is charged by one of the poles of the battery only, no more than three of the binding screws are employed; were the use of the instrument limited to these experiments, the other three binding screws, with the parts in connexion with them, might be dispensed with. But some very useful and interesting information may be obtained by charging the wire simultaneously by the two poles of the battery; and then it will be requisite to bring more of the binding screws into use. I will briefly indicate a few of these applications.

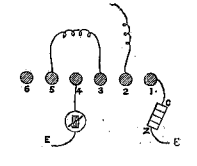
5. Currents produced by discharge from one or both ends of the wire after its separation from the battery.



Connect the battery poles with 3 and 6, the insulated wire to 2 and 5, and interpose the galvanometer between the earth and either 1 or 4; or galvanometers may be interposed between both.

This experiment must be made with a very long wire. The contact of the battery poles with the two ends of the wire give rise to discharge currents in the same direction with respect to the wire; the severance of the wire from the two poles of the battery, and its subsequent contact with the earth wires give rise to return discharge currents.

6. To ascertain what portion of its charge one charged wire communicates to another.

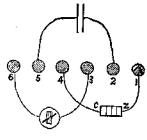


* It must be observed that the galvanometer employed in these experiments was one of extreme sensibility, and that this amount of escape would be insensible with the instruments ordinarily employed in telegraphic communication.

* Philosophical Transactions, 1843, Part II.

One of the poles of the battery being in communication with the earth, the other is to be connected with 1; the wire to be first charged is to be connected with 2, the two ends of the wire to be secondarily charged with 3 and 5, and the galvanometer is to be interposed between the earth and 4. A wire coated with any insulating material may in this manner have its charge divided with an equal wire, or with one the inductive conditions of which are different. By employing copper wires of the same lengths and diameters, the relative specific inductive capacities of different insulating materials may be determined.

7. To obtain an intermitting current from the accumulated discharges of a Leyden jar or condenser.

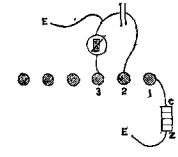


Connect one pole of the battery with 1 and the other with 4; the Leyden jar must be insulated, and a wire must connect its inner coating with 2, whilst another wire must unite its outer coating with 5; the galvanometer must be interposed in a wire uniting 3 and 6.

8. The accumulated discharges from a Leyden jar may be converted into an intermitting current, even when one of the poles of the battery communicates with the earth.

App. No. — Report by Professor Wheatstone. — Means of accumulating the effect of charges and discharges.

The following is the arrangement for effecting this purpose:—



Interpose the battery between the earth and 1; let a wire proceed from 2 to the inner coating of the jar, while the outer coating communicates with the earth and 3; the galvanometer must be interposed between 3 and the earth.

The "accumulating discharger" was completed and used in April 1860. Mr. Latimer Clarke's "inductometer" described in the "Engineer" of September 28th, 1860, which resembles this instrument in many respects, and was suggested by it, was not made by the Messrs. Elliots until the beginning of September 1860.

Mr. Siemens has also constructed a self-acting instrument for obtaining a rapid succession of charges and discharges, which he exhibited at the meeting of the British Association at Oxford in July 1860.

Though I was unaware of the fact at the time, an instrument for accumulating discharges for a special purpose (the same as that described in paragraph 7), was invented in 1849 by M. Guillemin,* and this gentleman has recently applied the same principle to measure the accumulated discharges from short lengths of coated telegraphic wires, covered externally with tin-foil.† To M. Guillemin, therefore, the priority of this useful method must be conceded.

An Account of some Experiments made with the Submarine Cable of the Mediterranean Electric Telegraph.—By Charles Wheatstone, F.R.S.—*From the Proceedings of the Royal Society*, March 29, 1855.

Experiments made with the submarine cable of the Mediterranean electric telegraph.

The following results were obtained between May 24 and June 8 in last year with the telegraphic cable manufactured by Messrs. Kuper & Co., of East Greenwich, for the purpose of being laid across the Mediterranean Sea from Spezia, on the coast of Italy, to the island of Corsica. The manufacturers, in conjunction with Mr. Thomson, the engineer of the undertaking, kindly afforded me every facility in carrying on the experiments. The short time that elapsed between the opportunity presenting itself and the shipping of the cable for its destination, prevented me from determining with sufficient accuracy some points of importance, respecting which I was only able to make preliminary experiments; but the following, which I was able to effect with the means at hand, may possess sufficient interest to be made public. They present, perhaps, nothing theoretically new, but I am not aware that experimental verifications of some of these points have been made before. I assume that the reader is acquainted with the experiments of Dr. Faraday, described in the "Philosophical Magazine," N. S., Vol. VII., p. 197.

The cable was 110 miles in length, and contained six copper wires, one-sixteenth of an inch in diameter, each separately insulated in a covering of gutta-percha one-tenth of an inch in thickness. The whole was surrounded by 12 thick iron wires twisted spirally around it, forming a complete metallic envelope one-third of an inch in thickness. A section of the cable presented the six wires arranged in a circle of half an inch diameter, and one-fifth of an inch from the internal surface of the iron envelope.

The cable was coiled in a dry well in the yard, and one of its ends was brought into the manufactory. The wires were numbered 1, 2, 3, 4, 5, 6, and the ends in the well were indicated by an accent; the ends 1′2, 2′3, 3′4, 4′5, 5′6, were connected by supplementary wires, so that the electric current might be passed in the same direction through all the six wires joined to a single length, or through any lesser number of them, the connexions being made at pleasure in the experimenting room.

The rheomotor employed was an insulated voltaic battery consisting of twelve troughs, each of twelve elements, which had been several weeks in action.

First Series.

The following experiments show that the iron envelope of the compound conductor gives rise to the same phenomena of induction which occur when the insulated wire is immersed in water, as in Dr. Faraday's experiments.

Experiment 1.—One end of the entire length, 660 miles, was brought in connexion with one of the poles of the battery, the other end remaining insulated. The wire became charged with negative electricity when its end touched the zinc pole, and with positive electricity when it communicated with the copper pole. A current, indicated by a galvanometer placed near the battery, existed as long as the charge was going on, and ceased when it arrived at its maximum. [The feeble current attributed to imperfect insulation, which continues as long as the contact with the battery remains, is here left out of consideration.] When the wire was charged, and the discharge effected by a wire communicating with the earth, the current produced was in the same direction, whether the discharge was made near the battery, or at the opposite end; *i.e.*, the current in both cases proceeded from the wire to the earth in the same direction.

Experiment 2.—On bringing one end of the wire in contact with one of the poles of the battery, the other pole having no communication with the earth, the wire remained uncharged. A very slight and scarcely perceptible tremor was observed in the galvanometer needle interposed between the battery and the wire.

Experiment 3.—To each of the poles of the battery was attached a wire 220 miles in length, and similar galvanometers were interposed between the two wires (the remote extremities of which remained insulated) and the battery. So long as one wire alone was connected with the battery no charge was communicated to it; but on connecting the other wire with the opposite pole, both wires were instantaneously charged, as the strong deflection of both needles rendered evident. On bringing the free end of one of the wires in communication with the earth, it alone was discharged, the other wire remaining fully charged.

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* Courant dans une pile isolée et sans communication entre les deux pôles. Comptes Rendus de l'Académie des Sciences, Nov. 12, 1849.
† Comptes Rendus, Oct. 8, 1860.

App. No. I.

Report by Professor Wheatstone.

Experiments on the Mediterranean electric telegraph cable.

SECOND SERIES.

Experiment 4.—One pole of the battery was connected with the earth and the other with 660 miles of wire, which had an earth communication at its opposite end; three galvanometers were interposed in the course of the conductor; the first near the battery, the second in the middle of the wire, *i.e.*, 330 miles from each extremity, and the third at the remote end near the communication with the earth. When the connexion of the battery with the wire was completed, the galvanometers were successively acted upon in the order of their distances from the battery, as in the experiments recorded by Dr. Faraday. When the earth connexion at the remote extremity of the wire, on the contrary, was completed, the disturbance of equilibrium commenced at this end, and the galvanometers successively acted in the reverse order, *i.e.*, the galvanometer which was the most distant from the battery was the first impelled into motion. In the latter case, before the completion of the circuit, the needles of the galvanometers had assumed constant deflections to a limited extent, owing to a feeble current arising from the uniform dispersion of the static electricity along the wire.

Experiment 5.—The two extremities of the 660 miles of wire were brought into connexion with the opposite poles of the battery. When one of the ends previously disconnected from the battery was united therewith, the galvanometers at the extremities of the wire, and consequently which were at equal distances from the poles of the battery, were immediately and simultaneously acted upon, while that which was in the middle of the wire was subsequently caused to move. When the wire disconnected in the middle instead of near one of the poles of the battery was again united, the middle galvanometer, which was the most remote from the battery, was the first acted upon, and those near the poles subsequently.

The comparison of the two above-mentioned experiments show that the earth must not be regarded simply as a conductor, which many suppose to be the case. Since in the first experiment there were not many yards' distance between the two earth terminations, did the extent of ground between them act only as a conductor, the two galvanometers at the extremities of the wire should have acted simultaneously, as in the second experiment, and as would have been the case had a short wire united the two extremities which proceeded to the earth.

THIRD SERIES.

Experiment 6.—One pole of the battery was connected with the earth, and the opposite pole with one extremity of the 660 miles of wire, the other end remaining insulated; a delicate galvanometer was interposed near the battery. Notwithstanding there was no circuit formed, the needle showed a constant deflection of 33½°; the feeble current thus rendered evident is not so much to be attributed to imperfect insulation, as to the uniform and continual dispersion of the static electricity with which the wire is charged throughout its entire length, in the same manner as would take place in any other charged body placed in an insulating medium. The strength of the current thus occasioned appears to be nearly, if not exactly, proportional to the length of the wire added, as the following table will show; the first column indicates the number of miles of wire subjoined beyond the galvanometer, and the second the corresponding deflections of the needle.

Miles.	Degrees.
0	0
110	6½
220	12
330	18
440	23½
550	28
660	31

Experiment 7.—One end of the 660 miles of wire was now allowed to remain constantly in contact with one of the poles of the battery; but the galvanometer was successively shifted to different distances from the battery. The strength of the current was now shown to be inversely as the distance of the galvanometer from the battery, becoming null at its extremity, as shown in the following table. The first column shows the distance from the battery at which the galvanometer was placed, and the second column the corresponding deflection of the needle.

Miles.	Degrees.
Near the battery	35½
110	31
220	25
330	15
440	12
550	5
660	0

The deflections of the needle of the galvanometer employed in these experiments were, when they did not surpass 36°, very nearly comparable with the force of the current. This I ascertained in the following way:—I took six cells of the small constant battery described in my paper, "On "new Instruments and Processes for determining the Constants of a Voltaic Circuit," printed in the "Philosophical "Transactions for 1843," and placed in the circuit formed of 660 miles of wire, the earth, and the galvanometer, successively 1, 2, 3, 4, 5 and 6 cells. Leaving out of consideration the resistances in the cells themselves and in the earth, which were very inconsiderable in comparison with that in the long wire, the force of the current should be approximately proportionate to the number of the elements; and since the deflections of the needle nearly indicated this proportionality, as the following table will show, it may be assumed that the force of the current, when the deflection of the needle did not surpass 36°, nearly corresponded with the angular deviation.

Cell.	Degrees.
1	6
2	14
3	19
4	28
5	32
6	36

From the preceding experiments (6 and 7), it seems to result that whatever length of wire is connected with the battery, if a galvanometer is placed at the farther extremity of the wire, and a constant length added to the other termination of the galvanometer, its indication remains always nearly the same. Thus, the galvanometer indicated 6½° when it was placed close to the battery, and 110 miles of wire were subjoined beyond it; and 5° when 550 miles were interposed between the battery and galvanometer, the same length, 110 miles, being subjoined. In like manner, when 220 miles were added beyond the galvanometer placed near the battery, the indication was 12°, precisely the same as when 440 miles were interposed and 220 added. So also when 330 miles were added, the deviation of the galvanometer was 18°, and 15° when 330 miles were interposed and 330 added. I have no doubt that the correspondence would have been closer had it not been for the fluctuations of the battery.

It would appear from this that whatever be the length of wire attached to the insulated pole of a battery, it becomes charged to the same degree of tension throughout its entire extent, so that another insulated wire brought into connexion with its free extremity exhibits precisely the same phenomena in kind and measure as when it is brought into immediate connexion with a pole of the battery. Some important practical consequences flow from this conclusion, which I will not develope at present, as I have not yet had an opportunity of submitting them to the test of experiment.

Note on the submarine telegraph.

Note on the Submarine Telegraph, from a Pamphlet published by Mr. Wheatstone in 1855.

A submarine electric telegraph was, from the commencement of Mr. Wheatstone's experiments, a prominent object in his thoughts. He has several letters, dated in the spring of 1837, from gentlemen acquainted with his plans, referring to this project. The first occasion on which any allusion to this subject appears is in the Fifth Railway Report of the Select Committee of the House of Commons. Mr. Wheatstone was examined before this Committee on February 6th, 1840; and Sir J. Guest, who was previously acquainted with his plans, put the question, "Have you "tried to pass the line through water?" to which he replied, "There would be no difficulty in doing so, but the experi- "ment has not yet been made." The Chairman (Lord Seymour) then asked, "Could you communicate from Dover to Calais in that way?" His answer was, "I think it perfectly practicable." Shortly after this, having been furnished with the necessary hydrographic information by his friend Sir Francis Beaufort, and received much useful counsel from the late Captain Drew of the Trinity Board, Captain Washington, and other scientific naval friends, he prepared his detailed plans, which were exhibited and explained to a great number of visitors at King's College, among whom were the most eminent scientific men and public authorities. He also made the subject known in Brussels. In a notice of his new telegraphic instruments, by Prof. Quetelet, published in the "Bulletin of the "Académie Royale de Bruxelles" for October 7th, 1840, it is stated,—"On sera sans doute charmé d'apprendre "que l'auteur a trouvé le moyen de transmettre les signaux

" entre l'Angleterre et la Belgique, malgré l'obstacle de
" la mer. Son voyage se rattachait en partie à cette im-
" portante opération, qui mettrait l'Angleterre en rapport
" immédiat avec notre pays, la France, la Hollande,
" l'Allemagne, et même la Russie." And in "Le Fanal,"
a Brussels paper of September 30th, 1840, it is observed,—
" M. Wheatstone pense qu'il est possible de communiquer
" avec son appareil entre Douvres et Calais; il répète en
" ce moment ses expériences à l'Observatoire de Bruxelles,
" en presence de plusieurs savans littérateurs."

Mr. Wheatstone's plans were also shown in 1841 to some of the most distinguished scientific men in Paris, who came to see his experiments at the College de France.

In the agreement entered into by Mr. Cooke and himself in April 1843, it was stipulated that certain limitations therein expressed "should not extend to prevent the said " Charles Wheatstone from establishing electric telegraph " communication between the coasts of England and " France, which he is hereby expressly authorized to do if " he shall so please, and for his own exclusive profit."

The agreement made with Mr. Cooke in October 1845, by which he undertook that the Company to whom he was about to sell the Patents should assist Mr. Wheatstone in carrying his project into effect, is given at length in the text, p. 51.

The Abbé Moigno was in England in the spring of 1846, whilst Mr. Wheatstone's experiments were in preparation, and he published an account of what he had seen in " L'Époque " of October in that year. This notice he afterwards reproduced in the first edition of his " Traité " de Télégraphie Électrique " (Paris, 1849). It is as follows :—

App. No. 1.

Report by Professor Wheatstone.

Note on the submarine telegraph.

" M. Quételet avait annoncé, dès 1840, que M. Wheat-
" stone avait trouvé le moyen de transmettre les signaux
" entre l'Angleterre et la France, malgré l'obstacle de la
" mer. J'ai vu de mes yeux, j'ai touché de mes mains la
" conducteur qui, en se reposant au fond des mers, unira
" étroitement les côtes d'Angleterre aux côtes de France.
" Ce conducteur est parfait, il remplira pleinement son
" but; tout homme sérieux qui l'aura vu et touché comme
" moi ne pourra pas même conserver l'ombre d'un doute
" sur un succès devenu palpable. Avant deux mois, des
" machines puissantes l'auraient produit dans toute sa
" longueur, mais partagé en section de deux kilomètres et
" demi. Huit jours suffiraient aux officiers de marine,
" qui s'y sont préparé par une étude approfondie, pour le
" mettre en place, et après quelques semaines Paris et
" Londres se toucheraient; il n'y aurait plus ni abîme, ni
" distance, le génie de l'homme aurait tout vaincu."

In consequence of Mr. Cooke's non-fulfilment of his engagement, and the proceedings on the part of the Company referred to in the pamphlet, Mr. Wheatstone was obliged to relinquish an object which had been a cherished one with him for many years. The Company, instead of giving him the assistance he relied upon, placed obstacles in his way, and his previous arrangements with Mr. Cooke precluded him from attempting to accomplish it through other channels. The result was that, for a time, the subject was in abeyance; but five years afterwards it was taken up from Mr. Wheatstone's starting-point, and was successfully accomplished by the enterprise and skill of other parties, unconnected either with the Company or with himself.

Appendix No. 2.

App. No. 2.

Remarks Mr. L. Clark.

Report by Mr. Latimer Clark.

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On Quantity and Tension.

On quantity and tension.

1. In describing the following experiments instituted to discover the phenomena and laws which regulate the transmission of electric currents through submarine cables, a clear definition of the sense to be attached to the words quantity, intensity, and tension, as here applied, is first necessary.

2. The term *quantity* has a well understood meaning; it conveys an analogous idea when applied to electricity, to that which it does when applied to water or to a gas; thus, two similarly charged Leyden jars, or two miles of submarine cable, contain twice the quantity that one does, and if the charges of both can be placed into one, the quantity remains unaltered, the only thing affected being the intensity or tension. If a pair of galvanic plates one inch square give a certain quantity of electricity in a unit of time, a pair of two square inches will produce twice the quantity, provided the plates are connected by very short and thick wire, for if connected by a very fine or long wire, the quantity will be smaller, and if not connected at all, none will be produced. In the latter case the quantity produced will be exactly that quantity which the connecting wire can convey from one to the other, and hence it arises that the quantity produced by a battery is generally proportional to the length and size of the connecting wire.

3. The effect exerted by electricity is in almost every case proportional to the *quantity* of electricity *in action*, and has no relation whatever to its intensity or tension.

On *quantity alone* depends the action of all telegraphic apparatus, which operates with more or less vigour according to the greater or less quantity of electricity passing through. If we increase intensity, we do so only with the view of making a larger quantity pass through the line, the quantity increasing in the same ratio that the intensity increases.

Electroscopic attraction and repulsion have not been hitherto considered by writers on electricity to be in any degree simply dependent on quantity, but to depend on the tension, or, as it is termed, the intensity of the charge; but a careful examination of the phenomena can scarcely fail to lead to the conclusion that the force varies *directly as the tension, directly as the quantity, and inversely as the distance.*

4. *Intensity* signifies difference or degree of tension, or that effect which is produced by increasing the number of cells in a battery; but in its more popular acceptation it has a very different value, and the effects of quantity are entirely confounded with those of intensity. For this reason I have throughout the remainder of this report discarded the word intensity and used only the word *tension*, a term which will, I think, not be liable to any misinterpretation; it has the same signification as the *potential*, as employed by Green and Professor W. Thomson, or as *electro-motive force.*

5. Every condition and quality of electricity is expressible by the relations of quantity and tension, and they are equally applicable to all its states, whether static or dynamic. The earth is always referred to as the standard of tension.

6. The earth, however, does not always exhibit the same tension at all places, nor does it always have the same tension at any given place. What are familiarly known to telegraphists as "earth currents" in their wires are evidences of this, and are produced by the flow of electricity between

App. No. 2. Report by Mr. L. Clark.

two points on the earth's surface, which are, at the time in an unequal state of tension; this flow continues until the tension has equalised itself. During the prevalence of aurora these electrical compensations proceed incessantly, and sometimes rise to a magnificent scale. Vast floods of electricity flow through the earth and the ocean, chasing each other to and fro in the most capricious manner, the currents being sometimes so great as to fill the telegraph lines with currents as powerful as those yielded by a battery of 300 or 400 cells.

On quantity and tension.

7. The mean tension of the earth, however, when free from these exceptional disturbances, may be taken as a standard of tension, its zero, like that of heat, being unattainable; we cannot determine the absolute tension of electricity, and, therefore, in speaking of the tension of objects, we mean their relative tension.

8. Wherever there are differences of tension and conductors there are currents of electricity set up, and these continue until the tension has equalised itself, and this inequality of tension is the essential condition and cause of all electric currents. If we connect the negative pole of a battery of, say, 100 cells to earth, its positive pole instantly exhibits a tension of 100 elements plus, or above that of the earth; if we reverse the battery, its negative pole presents a tension of 100 elements minus, or below that of the earth.

9. If a battery of, say, 200 cells be perfectly insulated, it is impossible to predicate what may be the tension of either of its poles; all we know for certain is, that the tension of its positive pole is 200 elements above that of its negative, and that the tension regularly increases from cell to cell. If we touch any part of the battery with an earth wire, that part instantly acquires the same tension as the earth; thus, if touched at the centre, its poles are respectively 100 elements above and below that of the earth; if at 50 cells from the positive end, the poles respectively acquire tensions of 50 plus and 150 minus, and so on; all this time no current is flowing and the tensions are purely statical.

10. If one end of a long piece of wood or string be applied to the pole of a very powerful battery of, say, 500 cells, and the other end to the ground, the one end will have the same tension as the battery, the other will have that of the earth; and if we apply an electrometer at successive points along the rod, we shall find every possible degree of tension exhibited from that of the battery to zero. This is an exact illustration of what takes place in a telegraph cable (see 28).

11. The amount of electricity which flows through a wire depends wholly on the relative tension of its two ends, and not on its absolute tension with respect to the earth. For example, a single cell of a battery connected by a wire transmits a given quantity of electricity when it is in its ordinary condition, and its tension therefore zero. If this element and its galvanometer be placed on the prime conductor of an electric machine, and the tension of the whole system be raised to two or three thousand elements above that of the earth, the quantity of electricity which passes is not affected, nor is the indication of the galvanometer. Again, if we connect a powerful battery with a very long wire, and place a galvanometer in circuit, first at the end near the battery, and afterwards at the end near the earth, or in any other part of the wire, we shall find its deflection precisely the same in all positions, although the tension at the extreme ends has been shown to be so different. We thus see that the indications of the galvanometer are not at all affected by the absolute tension of the point of the line at which it is connected.

12. If we form a galvanometer with only one turn of very thick wire, and note the deflection caused by one cell of moderate size, and then successively increase the number of cells, arranging them in series, we shall not, by so doing, find any increase whatever in the deflection of the needle, for the thick wire conveys freely all the electricity which one cell can produce, and the additional cells can *only increase* the tension, they cannot increase the quantity. If, on the other hand, a galvanometer be formed of a great number of turns of very fine wire, the deflection will be immensely greater with numerous cells. With a great length of fine wire the tension of one cell is only sufficient to transmit a small proportion of the quantity which it is capable of producing; by increasing the number of cells, however, the tension becomes sufficient to overcome the resistance; and whatever the *length* or *fineness of the* wire, there is some number of elements sufficiently great to cause as much electricity to pass in a unit of time as the one original plate caused to pass through the thick wire.

13. With dynamic electricity, the *quantity* of electricity which passes in a given time determines the amount of galvanic deflection, or electro-magnetic attraction, or chemical decomposition, or of the attraction of one current upon another, or of heat generated in a wire or a voltaic arc, and the *difference of tension* determines the velocity of electricity, or, in other words, the quantity which flows through a circuit in a given time. The tension, therefore, indirectly enhances all the phenomena above mentioned.

With static electricity, the *quantity* of electricity on a given surface determines the amount of electroscopic attraction and repulsion, and it determines the degree of tension, while the *tension* reciprocally determines the amount of attraction and repulsion, and also the quantity of electricity which will be stored up inductively on a given surface. And in all the above cases the degree of action varies, *cæteris paribus*, in simple proportion to the quantity, or, in the case of repulsion, to the quantity and tension combined.

On the Transmission of the Current through a Wire.

On the transmission of the current through a wire.

14. We come now to the consideration of the laws of the transmission of the current through a long wire or cable, neglecting, in the first instance, the consideration of induction. It will be of the greatest assistance to the reader, in thinking of the electric current, to regard it, which it probably is, as the flow of a fluid or matter like water, imponderable, it is true, but having a material existence, and following definite laws, like other fluids. In fact, if we could deprive air of its elasticity, or water of its weight, the laws of their motion would be almost identical with those of electricity.

15. Sir Humphrey Davy first stated that the conducting power of a metallic wire was inversely as its length and directly as its section, and this law has been confirmed by subsequent investigations. Ohm deduced the same laws from theoretical investigation, and beautifully explained the motions of the galvanic current.

16. The principal points in Ohm's theory in relation to telegraph circuits, are—

1. That the quantity of electricity which passes through a wire in a given time is directly proportional to the difference of tension at the two ends of the wire.

2. That the quantity is inversely proportional to the length of the wire, so that two miles of wire will only convey half as much as one mile; or, two wires, side by side, each ten miles in length, will convey the same quantity as one wire five miles in length.

3. That the quantity is directly proportional to the sectional area of the wire, or, in other words, to the square of its diameter, or to its weight per mile.

4. That the quantity is directly proportional to the specific conductivity of the material of which the wire is composed; or, as it is frequently stated, it is inversely proportional to its specific *resistance*. Thus, the conductivity of ordinary copper is about five or six times as great as that of iron or steel, and consequently a copper wire, of a given size, will convey six times as much electricity.

5. The tension of a wire is highest at the positive pole of the battery and lowest at the negative pole, and falls uniformly through its whole length.

17. If T represents the electro-motive force, or the difference of tension at the two ends of the wire, L is length, and S its sectional area, then the quantity of electricity passing—

$$Q \text{ will be} = \frac{TS}{L}$$

If we take the specific conductivity of the material C into account, the formula becomes—

$$Q = \frac{TSC}{L}$$

It is, however, more usual to regard the conductivity inversely, or as specific resistance R, the formula then becomes—

$$Q = \frac{TS}{LR}$$

If the cells of the battery are very large, the tension varies almost exactly as the number of elements e, and it is usual in practice to substitute this number for the tension T, the value of which is not readily obtained in practice; the formula is then—

$$Q = \frac{neS}{LR}$$

The resistance of the battery itself has to be taken into account when dealing with short circuits, but the telegraphist may generally neglect this resistance. A coefficient for temperature might also be added to the formula.

18. The foregoing laws apply equally to metallic wires, and to all other conducting, and so-called non-conducting materials, for all conduct to a certain extent and all obey the same laws. Ohm introduced the system of referring all resistances to a common standard, as, for example, to a copper wire of a given section; and taking a length of this wire which offered the same resistance as any circuit which he wished to measure, he called that length the re-

duced length of the circuit. It is now more commonly spoken of as the "*resistance*" of the circuit, and is in constant use in telegraphy.

Copper is usually employed as the standard, but its conductivity is liable to vary greatly from impurity. Silver and mercury have also been employed. Gutta percha might be usefully employed as a standard for insulating materials if its quality were more uniform.

19. The quantity of a battery Q varies as $a\,q$, that is, as the surface area of the plates a, and as the specific quantity q, or that quantity which a given surface can produce in a given time with a minimum resistance; the specific quantity varies greatly with different metals and different liquids, even although their specific tension (when no current is passing) and resistance are the same; it is also affected by temperature; the quantity of course varies inversely as the resistance, or as $\frac{a\,q}{r}$.

20. The efficient working value of any current varies as $T\,Q$, or as its tension and its quantity combined. The work done, or effect produced, will vary as Q directly, as before explained; and it will also vary as T directly, for if a current of a given tension will decompose an ounce of water in a given time, or work an electro-magnet with a given force, then a current of ten times that tension will decompose simultaneously ten ounces of water in ten separate vessels, or will work ten such electro-magnets at once.

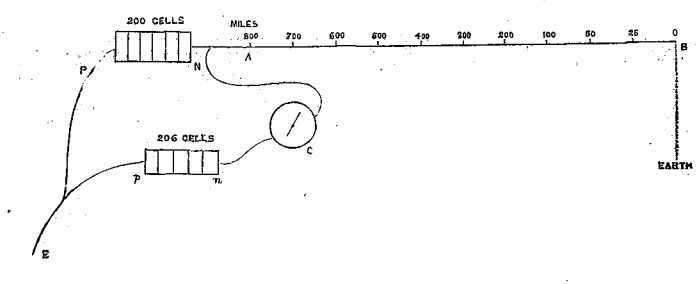
21. According to Ohm's law the quantity of electricity conveyed by a wire varies directly as the battery power, or as the difference of the tension of the current at the two ends of the wire, or at any other two points in its length. It is essential, however, that the area of the plates of the battery should be so large as to suffer no sensible diminution of their tension from the abstraction of the current by the conducting wire; this is a condition seldom or never obtained in practice. With the ordinary sized battery plates (about four inches *by three*), and a tension of 200 cells, the addition of a circuit equal to 2,000 miles of ordinary No. 8 line wire will produce a reduction in the tension of the battery current equal to about 10 cells, and a circuit of 100 miles will reduce the tension or the *effective working power* of the battery from 200 to 100 cells. This is a very important consideration, and is almost universally lost sight of by practical telegraphists, who too frequently assume that, because they are using a battery of 100 cells, they are working with the tension of 100 elements. It should be borne in mind that with a battery of 100 cells the full tension of 100 elements can only be rigorously obtained while the poles are disconnected; if they be connected by an ordinary conducting wire, although it be even 1,000 miles in length, the difference of tension of the two poles is very sensibly diminished, and as the wire gets shorter and shorter, or it may be thicker and thicker, the tension falls, until at last, when the connecting wire is at some certain length, the 100 cells only maintain a tension equal to, say for example, one element; and at this length the 100 cells cause exactly the same quantity of electricity to flow through the wire and produce the same deflection as one cell would do, if that cell were of infinitely large dimensions. If the conducting wire be still shorter, the tension becomes proportionately less, until with an extremely short and thick connecting wire the battery exhibits no tension at all; in fact, whatever rise in tension the battery commences to form is instantly destroyed by the flow of the electricity through the conductor. These effects are well exhibited in the following experiments:—

22. December 23, 1859.—A carefully insulated battery P N (see Fig. 26.), of 200 cells, was arranged with its positive pole to earth, and another similar battery $p\ n$, of equal size and tension, was arranged beside it with its positive pole also to earth, and their two negative poles were joined together and connected to a line wire A B, formed of resistance coils. A delicate vertical galvanometer C was placed between the secondary battery $p\ n$ and the line wire. The line wire A B was then disconnected, and the batteries were therefore opposing each other, and had they been equal, no current would have passed through the galvanometer. From some accidental inequality in their tensions, it required 205 cells in the secondary battery $p\ n$ to balance 200 cells in the primary.

23. The line wire A B (having a resistance of 800 miles of No. 16 copper wire) was now added, and a current from both batteries passed through it, that of the secondary battery deviating the galvanometer 35°; the number of cells in the secondary battery was now gradually reduced until the needle became vertical, and no current passed from it through the galvanometer. To obtain this result it was necessary to reduce the number of cells from 205 to 191, and the tension of the battery while giving off no current was now just equal to that of the primary after it had supplied the flow into the line; if one cell were added, it began to send a feeble current into the line; if one were taken off, the primary battery was enabled, after supplying the line, to send a feeble reversed current back through the secondary battery. The following table gives similar results with various lengths of the line A B, the primary battery being constantly maintained at 200 cells. The resistances represent miles of ordinary No. 16 copper wire:—

Number of Cells in the Primary Battery.	Number of Cells in the Secondary Battery.	Resistance of Line A B attached to the Primary Battery.
200	205	Infinite.
200	191	800 miles.
200	189	700 „
200	187	600 „
200	183	500 „
200	180	400 „
200	173	300 „
200	162	200 „
200	135	100 „
200	103	50 „
200	70	25 „
200	0	0 „

25. In the preceding experiment it is shown that with a battery of 200 cells the actual working tension might be anything between 200 elements and nothing, the greater or less difference being dependent on the length or size of the conducting wire; but whatever the difference may be, if the wire be of uniform section, the tension falls regularly and proportionately throughout the whole circuit. This is illustrated in the following experiments:—



26. December 23, 1859.—A battery (P N) of 200 cells was prepared; the positive pole was connected with the earth, and the negative with a series of resistance coils of a length equivalent to 800 miles of No. 16 copper line wire. This battery we will term the primary battery. A secondary battery ($p\ n$) was also prepared and joined on to the same line in the manner described in the preceding experiment. They were now jointly connected on to the 800 miles of line, and of course a current from each of them flowed through the wire. The number of cells in the secondary battery ($p\ n$) was now gradually reduced until no current passed through the galvanometer. In this state of

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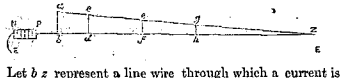
On the transmission of the current through a wire.

things the primary battery transmitted its full current through the line in exactly the same manner as if the secondary battery had no existence, the tension of the secondary battery being exactly the same as that of the line itself at the point of its connexion; the number of cells necessary to counterpoise the tension at this point was 192. If 191 cells were used, the primary battery had sufficient tension to overpower and send a slight reverse current back through the secondary battery, deflecting the galvanometer to the left; and on the other hand, if the number of cells were increased to 193, the tension of the secondary battery was a little in excess of the line at the point, and a feeble current passed through the galvanometer into the line deflecting it to the right. Having thus established the tension at this point, 800 miles from the distant end of the line, the secondary battery was now connected at 400 miles, and its tension was of course sufficient at this point to send a powerful current into the line. The secondary battery was now reduced until its tension exactly equalled that of the line at this point, the needle of the galvanometer giving no deflection; the number of cells requisite to effect this was found to be 94, or half the previous number. The same operations were repeated at 200 miles from the end, and here the number of cells requisite was 47, or one-fourth of the original number. At 100 miles the number of cells required to equalise the tension of the line was 23, or one-eighth of the first number. At 50 miles the number required was 12, and at 25 miles 6; at 0 miles of course there was no tension, and the feeblest battery power would have sent some current. The results are given in a tabular form below, and they show conclusively that the tension in a line wire of uniform section varies directly as the distance from the end of the wire connected with earth or inversely as the distance from the battery

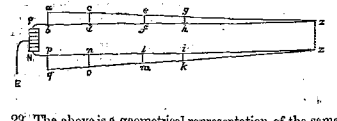
Distance of Secondary Battery from the end of the Line connected with Earth.	Number of Cells in Secondary Battery required to counterpoise the Tension of the Line.
800 miles.	192 cells.
700 ,,	167 ,,
600 ,,	143 ,,
500 ,,	118 ,,
400 ,,	94 ,,
300 ,,	70 ,,
200 ,,	47 ,,
100 ,,	23 ,,
50 ,,	12 ,,
25 ,,	6 ,,

28. The following diagram will illustrate this law geometrically:—



Let $b\ z$ represent a line wire through which a current is flowing, and let the perpendicular $a\ b$ represent the tension in the line near the battery as determined experimentally in the manner just described.

The tension at the distant end z, where it is connected with earth, is of course 0. The tension then at d, or at any other point along the line, such as f, h, &c., will be directly proportional to the length of the perpendiculars $c\ d$, $e\ f$, or $g\ h$.



29. The above is a geometrical representation of the same battery applied to a line wire of the same length, both its ends being connected with the battery without the use of an earth circuit. In this case all the positive tensions, as $a\ b$, $c\ d$, &c., are represented by perpendicular lines above the line wire, and all the negative tensions are represented by other lines, as $l\ m$, $n\ o$, &c., below the line. As the line wire and battery are of the same length as before, the difference of tension at the two poles of the battery will be the same as before, that is to say, the sum of the two perpendiculars $a\ b$, $p\ q$ will together equal the length of the perpendicular $a\ b$ in the preceding figure.

30. If we assume the whole tension of the battery to be 20, and the tension of the centre pair of plates to be 0, then the tension at the point $a\ b$ will be 10 plus or positive, and that at the point $p\ q$ 10 minus or negative. At the middle of the line, or $z z$, the tension will be 0. In other words, if we were to apply a galvanometer at the point b, we should obtain a current flowing from the wire to earth with a tension of 10; if at f, we should observe a current flowing to the earth with a tension of 5. If, on the other hand, we were to apply the galvanometer at p, we should perceive a current flowing from the earth into the wire, with a tension of 10; if at the point l, the current obtained would have a tension of 5 cells, but at z we should get no current whatever.

31. It will be seen that the tension of the line wire relatively to the earth depends entirely on the position of the earth wire E. We have shown it connected at the middle of the battery P N, but if we remove it to the end N we alter all the conditions immediately. The tension at $p\ q$ becomes zero. The tension at $a\ b$, as in the first figure, becomes 20. The tension of z, instead of being 0, becomes 10. In like manner if we connect the earth wire to the other end of the battery at P, then the tension at a becomes 0, the tension at N becomes 20 minus, and the tension at z 10 minus.

On the Effect of Temperature on the Conduction of Cables.

32. This influence is very sensible and even powerful. According to Becquerel,* a copper wire when raised 180°, viz., from 32° to 112°, has its conductivity diminished from 100 to 70·9, or, what is the same thing, its resistance increased from 100 to 140·9. This is about one per cent. increase of resistance for every 4·4 degrees increase of temperature, or an addition of ·0022 of the original resistance for each degree Fahr. of elevation. The effect of this increased conductivity is sensibly felt on overground telegraphs at night, and must of course affect the speed of transmission in cables lying in tropical waters. Some experiments on this subject were commenced, but owing to an accident led to no reliable result.

On the Effect of Pressure on the Conduction of Cables.

On the effect of pressure on the conducting wire of cables.

33. As it was desirable to ascertain what effect the superincumbent weight of the ocean might exert on the conductivity of deep submarine cables, a length of 220 yards (one-eighth of a mile) of gutta-percha wire containing a copper conductor $\frac{1}{18}$th of an inch in diameter was enclosed in hydraulic pipes connected with a force pump as described at section 118, and by the differential arrangement known as Wheatstone's parallelogram,† the resistance of this wire was accurately balanced by that of a similar length of wire not under pressure.

34. In this condition the current dividing itself, passed through both wires, and since they both had the same resistance the quantity in both was equal. A pressure of 3 tons on the square inch was now applied, by means of the hydraulic press and force pump, to the wire in the pipes, but, contrary to expectation, there was not any sensible alteration in the conductivity of the wire; the pressure was repeatedly applied and removed suddenly without effect. To test the sensibility of the appliances in use, the length of the wire under pressure was increased from 220 yards to 220 yards and 3 inches; this addition produced a very sensible increase in the resistance of the wire, which was readily observable on the galvanometer; even 2 inches produced a visible deviation. The pressure of 3 tons on the square inch is equivalent to that of a depth of 2,550 fathoms of sea water. In an Atlantic cable 3,000 miles in length, the alteration of conductivity due to the superincumbent pressure would therefore not be equal to that produced by an addition of one mile to its length.

35. The only metals adapted for use in submarine cables are copper, iron, and steel. Aluminium, from its lightness and strength, might be in some circumstances a very valuable material, but its cost precludes its introduction.

Aluminium, according to Dr. Matthiessen,‡ has a conductivity of 33·76, silver being 100, and ordinary copper from 80 to 90, the specific gravity of aluminium is about 2·5, that of copper being about 8·5. Iron wire, from its cheapness and strength, is almost universally employed for overground telegraphs. Its conductivity is about 13·1, silver being 100, but owing to its low conductivity, its sectional area has to be increased five or six times to make its conductivity the same as that of copper, and the con-

* Traité d'Electricité, vol. i. p. 87.
† Phil. Trans., June 1843.
‡ Phil. Trans., December 1860.

sequences are that about five times the weight of material has to be employed, and since the area exposed to induction is increased in the ratio of the surface, the insulating coating has to be enlarged in proportion. These disadvantages quite preclude its use in submarine telegraphy. The same disadvantages appertain to steel wire, which has a conductivity of about 14·4, although from its great strength it would be a desirable material.

36. The best material practically available is therefore copper. It has long been known that the conductivity of metals is frequently very much affected by a small portion of alloy, as was shown by Sir William Snow Harris. In the early history of telegraphy, from the shortness of the circuits employed, this fact was totally disregarded. When, however, the Atlantic Telegraph Company was formed, I called attention to this circumstance, and pressed the importance of taking steps to ensure that no brass or zinc should be alloyed with the copper employed for the intended cable. Professor W. Thomson, F.R.S., wrote a paper upon it in the Proceedings of the Royal Society for June 1857. The conductivity of the whole of the wire employed in the Atlantic telegraph was in consequence carefully tested. Dr. A. Matthiessen has recently published papers in the Phil. Trans., and has made experiments on this subject for the committee, his valuable report is included in the appendices.

37. Dr. Matthiessen proposes a process for purifying copper by the injection of hydrogen gas into the fused metal, the cost of which has been roughly estimated at about twopence per pound, or ten shillings per mile, for ordinary telegraph wire. The extreme importance of this subject will be appreciated by remembering that by increasing the conductivity of a telegraph cable 10 per cent. we increase, in the same ratio, the number of messages which can be transmitted through it in a given time.

On Insulation and Leakage.

On insulation and leakage.

38. Insulation is evidently merely the converse of conduction, and the insulation of a cable means really the resistance to conduction of the material of which it is composed. For the sake of brevity we shall hereafter speak of the loss of insulation, or the conduction of the exterior coating of a cable, as the "leakage." In a moderately well insulated cable the leakage will, it is obvious, with the same tension of the battery, vary directly as the length.

39. In considering the effect of variation in the thickness of the coating, it must be remembered that the nature of conduction is the same in all bodies, and that it follows the same law in shellac or in gutta-percha as in copper, the difference being only one of degree. In regarding, therefore, the effect of the increase of thickness of coating in a cable, we may consider the material a conductor, as we should do in the case of iron or copper. Let us then confine our attention to an inch of an imaginary cable, and assume the diameter of the copper conducting wire to be an inch, and the thickness of the coating also an inch, the total diameter being three inches. According to the well-known law of Ohm (viz., that the amount of electricity passing through a conductor varies directly as its sectional area and inversely as the length), the leakage from one inch of this cable may be assumed to be the same, while it forms part of a cable, as it would be if it were unrolled from the copper wire, that is to say, it would conduct the same quantity of electricity as a truncated pyramid of the same material, having the area of its base equal to the external surface of our cable (nine inches), and of its summit equal to that of the surface of our conductor, that is to say, three inches, its height being assumed to be everywhere one inch. Now this pyramid has the same conductivity as a cylinder of the same mean sectional area and the same height, that is to say, as a cylinder of five inches sectional area and one inch in height.

40. Let us now consider what would be the effect of doubling the thickness of our external coating, and see what extra resistance it will add. Our imaginary inch of cable will now be five inches in diameter, the area of the internal surface of the newly added portion will be nine inches, and of the external 15 inches, the mean will be 12 inches, or the resistance of the new portion will be equal to that of a cylinder one inch long and 12 inches in area.

The resistance of the inner cylinder will, of course, remain unaltered, and the resistance of this cylinder is to be added to that of the former one; now both the coatings are of the same thickness, viz., one inch, but the last coating has an area of 12 inches instead of five, and their respective conductivities are inversely as these numbers, viz., as 5 to 12; our second coating, therefore, although of the same thickness, does not oppose so much resistance, or even half so much resistance, as the first; and by following this reasoning we can perceive that each successive addition to the thickness of the insulator has less influence than the preceding, and that by doubling the thickness of the coating of the wire we do not, by any means, double its insulation, or, in other words, halve its leakage; and if we pursue the calculation further, we shall find that no thickness whatever will prevent the conduction altogether. It results from this, that whatever the quantity of gutta percha put on the cable, it can never be made a perfect insulator.

41. The foregoing law expressed in other language is as follows:—Let s be the area of the interior surface of the insulator in contact with the conducting wire, and S the exterior surface of the cable, and t the thickness of the coating, the conduction will vary as the mean sectional area of the outer and inner surfaces divided by the thickness, or—

$$\frac{\frac{S+s}{2}}{t}$$

In order to make the formula applicable to a complete cable,* we have to add co-efficients for the specific conductivity of the material, for the length, and for the temperature.

42. Some curious results follow from the above law. In the first place, if we double and triple the diameter of the conductor, and, at the same time, double or triple the thickness of the insulator, the insulation will remain constant, because in this instance the mean sectional area and the thickness constantly bear the same proportion to each other.

Again, if the conducting wire is infinitely small, the insulation of the wires would be the same whatever the thickness of the gutta percha, for s being infinitely small may be neglected; and since $\frac{S}{2}$ and t both vary in the same proportion, the conductivity or insulation remains constant. In the large table at section 44, under the column $\frac{a}{t}$, will be found the conductivity calculated according to the above formula, for each sample of wire, and in illustration of the above we may refer to wires Nos. 1 and 5. The diameter of the copper is very small in both cases, but the thickness of the gutta percha is in one case $\frac{1}{32}$ and in the other $\frac{12}{32}$, the external diameter of the one being a quarter of an inch, and that of the other more than three-quarters. At first sight it might be expected that the insulation would be very different, but, according to the formula, their calculated conductivity is only as 5 to 3·5, and their observed insulation, as given in the column of averages, confirms the calculation. Again, on the principle previously explained, the wires Nos. 1 and 3 should give the same insulation, which is found to be the case.

43. Since the leakage of a cable or its conduction follows the same law in gutta percha as in metals, it varies directly as the tension of the battery or the number of cells. A few experiments were made on this subject, but it is not necessary to record them.

It has been previously shown that in a cable connected wit the earth at one end and conveying a current, the tension is at the maximum near the battery, and falls gradually and regularly to the end connected with earth where it is at zero. In estimating, then, the leakage of such a cable, we must first find the tension of the wire near the battery (section 26), and deduce therefrom the tension, and, consequently, the leakage at all or any of the other points of the wire.

44. The subjoined tables give the results of a long series of observations upon the insulation of cables of various materials made by Messrs. Bartholomew and Rowland for the Committee, at temperatures varying from 32° to 92°, and with different instruments; the length experimented upon was in each case one mile. (See section 48.) These experiments are given at full length in the Appendix.

In Table II. the observations were made at the temperature 92° and 52°, by two different observers, with different instruments. Their freedom from slight error, through working in a moist atmosphere, cannot in either table be guaranteed, nor are the values of the deflections directly comparable with each other, they may nevertheless serve for rough comparison.

The following description refers to Table I.

* The term *cable* signifies any length of wire covered with an insulating coating.

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TABLE No. I.—INSULATION OF VARIOUS WIRES at various Temperatures, from 32° to 92°.

Dimensions all given in 32nds of an Inch.	Diameter of Copper Wire in 32nds of an Inch.	Thickness of Insulating Material = d in 32nds of an Inch.	Mean Sectional Area of Insulating Material = a in 32nds of an Inch.	$\frac{a}{d}$	Mean of several Observations. — Positive Current.	Temp. 32°.	Temp. 42°.	Temp. 52°.	Temp. 62°.	Temp. 72°.	Temp. 82°.	Temp. 92°.	Temp. 52°.	Average at all Temps.
1. PLAIN GUTTA PERCHA -	2	3	15	5	Suspended Galvanometer, 128 cells (Mr. Bartholomew).	·2	·3	·4	·0	2·1	3·9	8·4	85·2?	22·6
					Horizontal Galvanometer, 512 cells (My. Rowland).	5·0?	4·9	15·0	15·2	27·8	40·1	63·8		
					Tension fell to ½ in seconds, viz., 512 cells to 256 cells (Mr. Bartholomew).	35	33	18	15	9	5	4		
2. PLAIN GUTTA PERCHA -	2	6	24	4	Suspended Galvanometer.	·4	·0	·0	1·0	1·5	2·5	5·4	6·1	11·7
					Horizontal Galvanometer.	1·3	1·4	6·2	7·2	15·7	25·3	30·6		
					Tension fell to ½ in seconds.	213	134	65	26	13	7	5		
3. PLAIN GUTTA PERCHA -	4	6	30	5	Suspended Galvanometer.	-	·0	·4	·9	2·1	4·8	9·5	10·1	20·5
					Horizontal Galvanometer.	3·0	4·0	13·2	14·6	27·6	40·8	51·0		
					Tension fell to ½ in seconds.	95	86	87	18	10	5	5		
4. PLAIN GUTTA PERCHA -	8	6	42	7	Suspended Galvanometer.	-	·5	·6	1·0	2·2	4·4	9·7	14·8	22·0
					Horizontal Galvanometer.	4·2	4·1	14·9	17·0	27·3	41·1	52·2		
					Tension fell to ½ in seconds.	102	72	39	20	14	7	5		
5. PLAIN GUTTA PERCHA -	2	12	42	3·5	Suspended Galvanometer.	·2	·0	·3	·8	1·6	2·5	5·4	9·6	13·0
					Horizontal Galvanometer.	1·0	1·2	7·0	7·0	14·9	24·5	33·6		
					Tension fell to ½ in seconds.	69	48	23	19	10	6	5		
6. Average of the above GUTTA PERCHA wires -	-	-	-	5	Suspended Galvanometer.	·2	·2	·34	·64	1·9	3·6	7·7	11·1	17·8
					Horizontal Galvanometer.	2·9	3·1	11·3	12·5	22·6	34·3	45·1		
					Tension fell to ½ in seconds.	103	75	32	19	11	6	5		
7. GUTTA PERCHA COMPANY'S SPECIAL MATERIAL A. Composition unknown; resembles Gutta Percha - - - - -	2	3	15	5	Suspended Galvanometer.	·4	·0	·0	·0	·4	·8	1·0	14·2?	6·5?
					Horizontal Galvanometer.	·0	·4	5·0	3·5	5·0	6·5	17·8		
					Tension fell to ½ in seconds.	254	405	245	109	54	42	28		
8. GUTTA PERCHA COMPANY'S SPECIAL MATERIAL B. Composition unknown; laid on in coatings alternating with thin layers of Chatterton's Compound - - - - -	2	3	15	5	Suspended Galvanometer.	·2	·0	·0	·0	·2	·2	·1	4·5	2·9
					Horizontal Galvanometer.	·0	·0	4·6	2·4	3·3	3·1	5·1		
					Tension fell to ½ in seconds.	85?	555	298	177	102	60	30		
9. GUTTA PERCHA COMPANY'S SPECIAL MATERIAL C. Composition unknown; laid on in coatings alternating with thin layers of Chatterton's Compound - - - - -	2	6	24	4	Suspended Galvanometer.	·3?	·0	·0	·0	·0	·0	·0	·0	·06
					Horizontal Galvanometer.	·0	·0	·0	·0	·25	·25	·0		
					Tension fell to ½ in seconds.	1060	1296	675	720	700	650	597		
10. RADCLIFFE'S SPECIAL MATERIAL A. Composition unknown; prepared by Messrs. Hancock and Co.; stated to be pure Gutta Percha, only chemically prepared - -	2	6	24	4	Suspended Galvanometer.	·0	·3	·7	·5	1·4	3·2	2·5	7·0	11·9
					Horizontal Galvanometer.	2·0	3·5	10·2?	9·3	14·0	21·8	26·8		
					Tension fell to ½ in seconds.	25	46	30	22	15	7	5		
11. RADCLIFFE'S SPECIAL MATERIAL B. Composition as above (No. 10) -	2	6	24	4	Suspended Galvanometer.	·1	·3	·8	·5	1·2	1·8	3·2	-	14·7
					Horizontal Galvanometer.	3·5	3·7	14·2?	10·6	15·0	25·0	31·8		
					Tension fell to ½ in seconds.	25	42	30	21	13	8	6		
12. GODFROY'S SPECIAL MATERIAL, called "Godefroy's Improved Gutta Percha," a mixture of 70 per cent. Gutta Percha, 20 per cent. Ground Cocoa-nut Shells, and 10 per cent. India Rubber, laid on in two coatings - -	2	3	15	5	Suspended Galvanometer.	·6	·4	·6	·8	2·3	4·2	-	30·0?	23·2
					Horizontal Galvanometer.	3·5	5·2	18·2	19·0	28·7	44·0	49·1?		
					Tension fell to ½ in seconds.	35	42	22	15	9	5	5		
13. HEARDER'S SUBMARINE CABLE, covered with Gutta Percha in two coatings, with a layer of Hemp Fibre, saturated with Tar between the coatings. See Phil. Mag., May 1859 - - - - -	4	6	30	5	Suspended Galvanometer.	·4	-	-	-	-	-	-	20·0	17·9
					Horizontal Galvanometer.	·4	1·6	4·0	12·0	20·3	33·0	52·6		
					Tension fell to ½ in seconds.	260	160	40	42	17	10	8		
14. Twenty alternate coatings: ten coatings of Gutta Percha, alternating with ten coatings of Chatterton's Compound -	2	6	24	4	Suspended Galvanometer.	·3	·0	·0	·1	·2	·6	1·0	3·0	3·2
					Horizontal Galvanometer.	·0	·25	·4	2·0	5·1	6·5	8·1		
					Tension fell to ½ in seconds.	about 900	793	460	125	53	33	22		

Table No. I.—Insulation of various Wires, &c.—*continued.*

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Dimensions all given in 32nds of an Inch.	Diameter of Copper Wire in 32nds of an Inch.	Thickness of Insulating Material = t in 32nds of an Inch.	Mean Sectional Area of Insulating Material = a in 32nds of an Inch.	$\frac{a}{t}$	Mean of several Observations. — Positive Current.	Temp. 32°.	Temp. 42°.	Temp. 52°.	Temp. 62°.	Temp. 72°.	Temp. 82°.	Temp. 92°.	Temp. 52°.	Average at all Temps.
15. Hughes' Fluid Cable wire covered with two coats of Gutta Percha, and enclosed loosely in an exterior Percha tube, the intervening space being filled with a fluid resembling tar	·2	6	24	4	Suspended Galvanometer.	·2	·0	·3	·0	1·2	4·2	9·5	10·1	18·3
					Horizontal Galvanometer.	1·0	1·0	10·3	10·0	25·5	30·1	49·2		
					Tension fell to ½ in seconds.	200	102	40	14	8	5	5		
16. Silver and Co.'s India Rubber B. The India Rubber is masticated, cut into strips, wound spirally on in many layers, and rendered homogeneous by heat	·2	5·24	21·7	4·1	Suspended Galvanometer.	·1	·1	·0	·0	·0	·1	·0	4·6	·7
					Horizontal Galvanometer.	·0	·0	·0	·0	·12	4·0	1·25		
					Tension fell to ½ in seconds.	348	332	1382	1267	630	382	270		
17. Silver and Co.'s India Rubber C. (For description see No. 16.) The exterior was covered with an adherent coating of Cotton Felt	·2	6·2	24·6	3·99	Suspended Galvanometer.	·0	·0	·0	·0	·0	·0	·0	·0	·0
					Horizontal Galvanometer.	·0	·0	·0	·0	·0	·12?	·0		
					Tension fell to ½ in seconds.	453	1740	4740	2270	850	450	230		
18. Hall and Wells' India Rubber A. Wire covered with Cotton, and lapped with spiral layers of Para India Rubber masticated, then covered with vulcanized India Rubber thread	·2	3	15	5	Suspended Galvanometer.	·3	·3	·0	·4	·4	1·3	4·9	35·1?	14·4?
					Horizontal Galvanometer.	2·0	2·2	9·0	9·4	11·0	27·8	43·2		
					Tension fell to ½ in seconds.	148	98	52	36	24	12	6		
19. Hall and Wells' India Rubber B. Copper Wire (No. 18) covered with Cotton. The first layer of pure cut Para bottle India Rubber not masticated, then with masticated Rubber and elastic thread as above (18)	·2	3	15	5	Suspended Galvanometer.	·0	·0	·0	·3	·1	·3	5	3·1	3·7
					Horizontal Galvanometer.	·25	2·0	3·0	3·2	4·2	6·2	7·8		
					Tension fell to ½ in seconds.	88	330	232	250	366	85	40		
20. Wray's Special Material A. A mixture of India Rubber, Shellac, powdered Flint, and about one-ninth Gutta Percha, in several layers, coated by a die	·2	6	24	4	Suspended Galvanometer.	·1	·1	·0	·0	·0	·1	·4	1·9	1·2
					Horizontal Galvanometer.	·0	·0	·0	·25	3·0	3·0	1·9		
					Tension fell to ½ in seconds.	about 10·000	9·100	3000	2027	520	240	128		
21. Wray's Special Material B., 176 yards only. First three coatings same Composition as A., but without any Gutta Percha; applied in strips by pressure; then one coat of the same material as A., put on by a die machine	·2	6	24	4	Suspended Galvanometer.	–	–	–	–	–	–	–	–	·0
					Horizontal Galvanometer.	–	–	–	–	–	–	–		
					Tension fell to ½ in seconds.	–	–	10·800	15·300	6·600	2580	450		
Dates of the Observations	–	–	–	–	1860.	Mar. 18	Mar. 22	Mar. 30	April 5	April 13	April 19	April 25	May 29	

Table No. II.—Insulation of various Wires.

Battery Power, 512 cells; Pos. to Cable.

1860.	Mr. L. Clark, on Suspended Galvanometer. April 27. Temp. 92°.	Mr. O. Rowland, on Horizontal Galvanometer. April 26. Temp. 92°.	Mr. L. Clark, on Suspended Galvanometer. May 23. Temp. 52°.
1. Plain Gutta Percha, $\frac{2}{32}$.	46·5	53·2	
	46·0	53·3	
	45·8	53·3	
	45·7	53·5	
Average	45·5	53·3	5·7
7. Gutta Percha Company's Special Material A.	8·0	5·	
	7·5	5·	
	7·2	5·25	
	6·7		
	7·3	5·1	2·5
8. Gutta Percha Company's Special Material B.	2·9	17·8	
	2·7	17·7	
	2·7	18·0	
	2·7	17·8?	1·2

1860.	Mr. L. Clark on Suspended Galvanometer. April 27. Temp 92°.	Mr. O. Rowland on Horizontal Galvanometer. April 26. Temp. 92°.	Mr. L. Clark on Suspended Galvanometer. May 23. Temp. 52°.
9. Gutta Percha Company's Special Material C.	2·5	·0	
	2·0	·0	
	2·5	·0	
	2·0		
	2·2?	·0	·1
10. Radcliff's Material A.	—	31·0	
	—	32·0	
	— (Injured.)	31·5	(Injured.)
		31·5	
11. Radcliff's Material B.	—	27·0	
	—	26·8	
	— (Injured.)	26·7	
		26·8	7·5

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Table No. II.—*continued.*

1860.	Mr. L. Clark on Suspended Galvanometer. April 27. Temp. 92°.	Mr. O. Rowland on Horizontal Galvanometer. April 26. Temp. 82°.	Mr. L. Clark on Suspended Galvanometer. May 23. Temp. 82°.
Godefroy's Material	43·2	49	
	42·9	49	
	42·7	49·2	
	43·0		13·7
	42·9	49·0	
		8·0	
14. Twenty alternate Coatings.	5·9	8·3	
	5·9	8·0	
	5·9		·5
	5·9	8·1	
	5·9		
		49·0	
15. Hughes' Fluid Cable	46·0	49·3	
	46·0	49·2	
	46·0		4·5
	45·0	49·2	
	46·0		
		1·25	
16. Silver and Co.'s India Rubber B.	·4	1·25	
	·4	1·25	
	·4		·1
	·4	1·2	
	·4		
		·0	
17. Silver and Co.'s India Rubber C.	·2	·0	
	·2	·0	
	·15	·0	
	·15		·0
	·2	·0	
		43·7	
18. Hall and Wells' India Rubber A. (probably faulty).	43·4	44·0	
	43·4		
	43·6	44·0	
	43·8		32·5
	43·5	43·9	
		7·8	
19. Hall and Wells' India Rubber B.	3·0	8·0	
	2·7	7·7	
	2·8		
	2·7	7·8	·6
	2·8		
		2	
20. Wray's Compound	·7	1·75	
	·6	2	
	·55		
	·65	1·9	·1
	·6		

On insulation and leakage.

45. The three first columns speak for themselves. The fourth column contains the mean sectional area of the insulating material a, and is obtained by adding the circumference of the conductor to that of the exterior circumference of the insulator, and dividing the product by two, on the principle explained at section 39. The fifth column is headed $\frac{a}{l}$, and is obtained by dividing the mean sectional area by the thickness as given in the third column; it represents the calculate conductivity of each of the wires, supposing them to be made of gutta percha. Where the observed results, as given in the last column of averages, do not agree with this calculated formula, the difference must be set down either to defects, or to errors in the observations or to the difference of the specific conductivity of the material; and if we neglect the errors of observation, the specific conductivity of each material may be known by comparing its observed conductivity with that given by the calculation in the column.

The sixth column contains three headings opposite to each material. The top series, headed "Suspended Galvanometer," were taken by Mr. E. G. Bartholomew, at different dates and temperatures, while making observations on the induction of wires, and are extracted from a table which will be found in another part of this report. They were taken with a suspended galvanometer, wound with 30,500 turns of coil wire, but since the battery power used was comparatively small (128 cells), the readings are generally so low that but little dependence can be placed on them for comparison.

46. The second series of observations, headed "Horizontal Galvanometer," were made by Mr. O. Rowland, with the special object of determining the insulation. The galvanometer on which they were measured is a horizontal galvanometer, manufactured by Mr. Henley; the needle is 3·18 inches in length, ·242 inches broad, and ·017 inches thick. It is supported on a fine steel axis between agate bearings, and the index is a light steel wire; the graduated circle is 5 inches in diameter. The two coils are of the ordinary construction, and the cheeks are formed of hard vulcanized india rubber or vulcanite; they are each 5·6 inches long and 1·4 inches broad externally. The depth from top to bottom is 2·35 inches. The coil wire is of copper, number 40 gauge, or about ·002 inches diameter, and the number of convolutions is probably about 15,000. Its resistance is about equal to that of 300 miles of the ordinary number 16 copper wire. Although the needles are not astatic, it is an instrument of great delicacy. As it was not used as a sine galvanometer, the indications are not strictly proportionate to the quantity of leakage, and they must only be regarded as approximate.

The difficulty of making correct and accordant observations on insulation are much greater than would be supposed. In order to render the leakage measurable, it is necessary to use a very high battery power; in the present instance 512 cells were employed, and with this high tension the care required to prevent conduction over the surface or through the material of substances usually considered insulators is so great that the best experimenters are extremely liable to fall into error, and few, indeed, are competent to make such observations with reliable accuracy. Such substances as ivory, and dry wood, as mahogany and deal, are wholly inadmissible. India rubber, glass, and even vulcanite and gutta percha, unless carefully and frequently dried or varnished, acquire a film of moisture and an invisible deposit, which are sufficient to give sensible indications on the galvanometer and vitiate observations. Errors are readily caused by the accidental proximity and motion of iron or other magnetic bodies, and there are variations in the battery power, and in the magnetism of the galvanometer, and many other sources of error so numerous and insidious even in experienced hands, that the results given in the tables must be considered as approximate only and not strictly to be relied upon.

47. The third series of observations on each material was taken, like the first, by Mr. E. G. Bartholomew. Each wire was charged by contact with the battery of 512 cells, and the static tension noted by the divergence of an accurate electrometer. The battery was then removed, the number of seconds were counted which elapsed while the tension, as indicated by the electrometer, fell to half its original amount, that is to say, from 512 to 256 cells. This is a very useful and accurate method, and the only one available in many cases, as many of the wires were so perfect in their insulation, that not the slightest leakage could be detected by the galvanometer. The electrometer or instrument by means of which the observations were taken is almost identical with that known as "Peltier's electrometer," and is described further on under the name of Milner's electrometer. The principle of measuring insulation, by the fall of the static tension, as given on the electrometer, is one of great convenience and value, and has the advantage that in theory, and to a great extent in practice, it is independent of the length of a cable, and the observations of insulation are the same for one foot as for one mile. The inutility of the ordinary galvanometer for such observations will be apparent by the inspection of some of the observations in the table. It will be seen that in some cases the leakage was so small as to take three or four hours before half the charge given by 500 cells escaped through the insulating coating.

48. The remaining columns of the table speak for themselves, and give the temperature at which the several observations were made. The increase was extremely gradual, and occupied in all two months, so that there was ample time for the wire to acquire the same temperature as the water. The wires were all laid in a tank in loose coils about 2 feet 6 inches in diameter, and the water in the tank was first cooled by ice to a temperature of 32°, and then slowly raised by the gradual circulation of warmer water with agitation day and night, the whole experiment occupying two months; the two ends of each coil were brought up through the floor into the experiment room, and the wires attached to terminals fixed on a broad sheet of gutta percha. This plan answered sufficiently well at first, but in a short time the surface of the gutta percha got soiled, and it became necessary to remove the ends of the wire and leave 12 inches of their length standing free, the ends being varnished with a composition of shellac dissolved in alcohol.* The india-rubber wire had a great tendency

* The best varnish for such purposes is made of amber dissolved in chloroform; spirit varnish is apt to contain a little water.

to attract moisture on the surface, in the same manner as glass, and it became necessary in all cases to renew the varnish frequently. In making each observation the cable was first charged by direct contact with the battery, the wire being detached from the galvanometer to avoid the disturbance caused by the first entrance of the charge; it was then attached to the galvanometer, and the amount of the current passing through the galvanometer into the wire after contact had continued 20 seconds was noted; this manipulation was performed by a key.

49. The insulation of the several kinds of wire will be more particularly referred to when we come to treat of them individually.

The difference in the degree of insulation is very marked; gutta percha and some of its compounds, including Radcliff's, Godefroy's, Hearder's, and Hughes' Fluid Cable, stand lowest in the scale, and form a group, all the members of which have about the same amount of insulation. The wire with 20 alternate coatings of Chatterton's compound has a much higher degree of insulation. The special material of the Gutta Percha Company is remarkably excellent. The remaining group contains those wires which are of india rubber or have india rubber in their composition. The lowest in the scale is Hall and Wells' india-rubber wire; but as the insulation of india rubber is known to be very high, the defective insulation of this wire may possibly be due to some mechanical cause rather than to its electrical conductivity. The india-rubber wire of Messrs. Silver and Co. appears to stand next, the group is completed by Wray's compound and the wires of vulcanized india rubber manufactured by Messrs. Hooper and Co. and Mr. Daft, neither of which latter appears in this table. For convenience of comparison a column is given showing the average insulation at all temperatures; but in forming an opinion too much reliance should not be placed on this column alone.

50. The galvanometer, in the manner in which it is ordinarily used, is an instrument which has but a small range, and the value of a degree at different angles of deflection is extremely different. As the needle is more deflected from its parallelism with the coil, three circumstances co-operate to weaken the electro-magnetic influence; first, the forces do not act at right angles to its length, but obliquely; secondly, the distance of the needle from the wire rapidly increases; and, thirdly, the opposing power of the earth's magnetism or gravity (as the case may be) becomes greater in the ratio of the sine of the angle. All these causes combined make it impossible to institute a numerical comparison between the different indications of the ordinary galvanometer, such, for instance, as those given in the table at section 44. If the instrument be employed as a sine galvanometer, two of the foregoing sources of variation are removed, namely, the distance of the needle from the coils and its angle with respect to them; and the third element, varying as the sine of the angle of deflection, allows the relative value of the indications to be easily calculated; but the sine galvanometer possesses the disadvantage of having a very small range, much less, indeed, than when used in the ordinary form. If the instrument be made an astatic galvanometer, either by the use of two needles or the employment of an external permanent magnet to neutralize the magnetism of the earth, the foregoing effects are immensely increased and defy all calculation.

51. The following method, however, of using the galvanometer can be confidently recommended, not only for telegraphic purposes, but for all other scientific measurements, and is one which will probably some day come into general use. Although the sine galvanometer is best adapted for the purpose, it is by no means essential. Any ordinary galvanometer may be employed, whether astatic or otherwise.

The method is as follows. Let a galvanometer be chosen sufficiently delicate to indicate a sensible deflection with the most feeble current that has to be measured, and ascertain the resistance of the coils in the usual manner. Fix upon some deflection, say 10° as the standard. When a more powerful current is passed through the instrument, the deflection will of course be greater, but instead of reading this greater deflection, join an ordinary resistance coil to the two terminals of the galvanometer in such manner that a portion of the electricity may pass through the resistance coil as well as through the galvanometer, and by increasing or diminishing the length of the coil, alter its resistance, until the needle of the galvanometer stands at the same angle as before, and note the number of miles of resistance in the coil. Then, by the well known law that the quantity of electricity passing through two circuits is inversely proportionate to their resistance, we have a very exact means of comparing the value of the two observations. Thus, for example, if the wire of a galvanometer gives a resistance of 80 miles, and if the coil also gives a resistance of 80 miles, we should be sure that the quantity which passes through the galvanometer would be the same as that which passed through the coil. The total quantity would be twice as great as before. But let us suppose the resistance of the coil had been 40 miles instead of 80, the electricity passing through the two would divide itself in an inverse ratio to these numbers; that is to say, 40 parts go through the galvanometer and 80 through the coil, and the whole of the electricity passing may be supposed to be divided into 120 parts, and of these 120 parts we know from the nature of the experiment 40 are equal to the quantity of electricity which we originally set out with. Therefore the whole of the electricity passing through the galvanometer and coil or through the two circuits may be represented by $\frac{120}{40}$, if 40 represent the original current; that is to say, the total quantity is three times as great. Another instance. Let the resistance of the galvanometer equal 80 and that of the coil 240, then the proportion of galvanometer and coil will be 240 and 80 respectively. The total strength of current is obtained, as in the former instance, by dividing the sum of these two quantities by the quantity which passes through the galvanometer, that is to say, $\frac{320}{240}$, or $1\frac{1}{3}$. Or let the resistance of the coil be now taken as one mile, then the proportions through galvanometer and coil will be as 1 to 80, or total $\frac{81}{1}$, or 81 times as much as in the first measurement. To put the subject into the form of a rule: the quantity of electricity passing through the galvanometer will be represented by a fraction, of which the resistance of the coil forms the denominator, and the sum of the resistances of the coil and galvanometer forms the numerator.

52. The foregoing method is obviously applicable to the measurement of quantities of electricity, however large; such, for instance, as the leakage of 1,000 miles of cable, and it is preferable to the method of measuring leakages by resistances, because it can be readily comprehended by the mind. Thus, by the latter method, the leakage on a mile is represented by the resistance of many thousands or perhaps millions of miles of wire; while the leakage of 2,000 miles of cable, instead of being represented by a larger quantity, is represented by a much smaller one. But by the method described the leakage will be directly proportional to the mileage, and thus the difference of lengths of cable can be readily compared with each other. A certain length of the wire should be set aside as a constant standard of reference, and the battery power may be varied to make its leakage constant. A method similar to this will be described under the heading *Induction*.

It is well to measure the resistance of the instrument anew each day, as inequality of temperature produces great changes. It is not often that the altered resistance of the galvanometer has to be taken into account, but when necessary, it is obtained by the usual formula; viz., multiply the resistance of the galvanometer by the resistance of the coil, and divide the product by the sum of the two resistances. Thus, in the last case quoted, the joint resistance of the instrument and resistance coil would be ·987, or,

$$\frac{80 \times 1}{80 + 1} = \cdot 987 \text{ miles.}$$

On the Effect of Temperature on Insulation.

53. It is well known that the conductivity of copper and other metals is very greatly diminished by increase of temperature. It is singular that all the substances employed as insulators have, without exception, the reverse property, their conductivity being increased by heat. Both properties conspire against telegraphy in hot climates: the copper wire conducts less electricity and the leakage becomes greater. The amount of increase varies materially in different substances, but is apparent in all. In gutta percha the increase is so serious as to render that material ill adapted for use in the waters of warm or tropical climates. It is constantly noticed, that in submerging cables, as they pass through the ship's hold into the cooler water, the insulation improves.

The cause of this increase and the laws which regulate it are not understood, but the effect appears to go on increasing up to and beyond melting point. The tabular statement at section 44 will give the means of judging of the amount of this increase in different materials. But even at the highest climatic temperatures many of the cables exhibit such a very high degree of insulation that there is no difficulty in establishing efficient telegraphs in any climate.

The table annexed shows the difference between the insulation of gutta-percha cables, each one mile in length and of similar dimensions, at the respective temperatures of 52° and 92°. They are measured on a horizontal galvanometer with 512 cells, positive current, but from the nature of the

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instrument they are not numerically comparable. They were taken by Mr. O. Rowland.

On the effect of temperature on insulation.

Diameter of Copper Wire.	Thickness of Gutta Percha.	Deflection at Temperature 92°.	Deflection at Temperature 52°.
Inches.	Inches.		
$\frac{3}{32}$	$\frac{3}{32}$	53·3	7·9
$\frac{3}{32}$	$\frac{6}{32}$	30·7	6·0
$\frac{3}{32}$	$\frac{9}{32}$	51·0	8·2
$\frac{3}{32}$	$\frac{12}{32}$	38·7	5·0
$\frac{3}{32}$	$\frac{0}{32}$	52·2	8·9
Average -	—	45·2	7·2

On the Effects of External Pressure on Insulation.

On the effects of external pressure on insulation.

54. It became a matter of much interest to ascertain what might be the effect of the enormous external pressure to which deep sea cables are necessarily subjected, and many experiments were made on this point. The uniform result obtained was that the insulation was greatly and instantly increased, or, in other words, the conductivity of the insulator was diminished by pressure, and the improvement in insulation thereby induced appeared permanent, so long as the pressure continued to be applied, but no longer. Wires of different materials were subjected to pressure for lengthened periods, but in no case could any evidence of permanent injury or change be obtained. India rubber in a masticated state is known to be absorbent of water, but the effect did not appear to proceed with greater rapidity under pressure than under ordinary circumstances. Very great difficulty was experienced in conducting these experiments satisfactorily, principally owing to the difficulty in making the joints. The impression derived from them was very decided that there is no reason to apprehend any difficulty in deep sea telegraphy, as regards the insulation of the material generally, or of the joints. A great number of experiments will be found in another part of the Appendix, which were conducted under the care of Mr. O. Rowland, but the following, among others, were performed by myself. The apparatus is described further on, and is figured in one of the plates in the Appendix.

55. (3rd May 1860). A length of No. 16 copper wire, covered with Wray's compound, of the diameter of ¼ inch, and 220 yards in length, was drawn into the hydraulic apparatus described at section 118, and the ends being secured, a pressure of three tons on the circular inch = 8,556 lbs., or 3 tons 16 cwt. on the square inch, was applied by means of the force pump. This pressure is equivalent to a depth of 3,190 fathoms of sea water. A battery power of 512 cells was applied, and the insulation measured by the fall of the static tension as shown on a "Milner's electrometer." The deflection of this instrument with a tension of 512 cells was 52½°. The time was noted in which the needle fell from 52½ to 49°, the temperature was 47°, and the time midnight. Before the pressure was applied the length of wire covered with Wray's compound took 12 minutes in falling from 52½° to 49°. The pressure was then applied, and the insulation immediately measured again, and it now required 25 minutes for the tension to fall, as before, from 52½° to 49°; the insulation was twice as perfect as before. The pressure was then removed, and a third observation was made, when it was found to occupy 18 minutes in falling to the same tension.

56. The following experiment was made on a piece of gutta percha wire 110 yards in length and of the same dimensions. The pressure applied in this case was three tons on the square inch, or equivalent to 2,555 fathoms of sea water. The temperature was 54° and the time midnight. The insulation was measured by the Milner's electrometer, and the time was noticed, during which the tension fell 10°, this was equivalent to a fall of the tension from 512 cells to 384 cells. Before the pressure was applied five observations were made, viz.:—

Time occupied in falling 10° on Milner's Electrometer.
22 seconds.
23 ,,
23 ,,
23 ,,
23 ,,

A slight but unknown pressure was then applied, and the time of falling increased to 33 seconds; a little more weight was put on and the time increased to 60 seconds; with a further weight the time increased to 75 seconds; the weight was then taken off and three observations were made, and each gave 25 seconds as the time for falling 10°. A portion of the weight was again added, and another observation gave 87 seconds. The whole pressure of three tons on the square inch was now added and three observations taken, which gave—

Time of falling 10°.
110 seconds.
100 ,,
115 ,,

The pressure was kept on 15 minutes and taken off for seven minutes; five observations were then made, the results were as follows:—

Time of falling 10°.
22 seconds.
22 ,,
22 ,,
23 ,,
23 ,,

The pressure being again put on, three observations were taken of the time, which were as follows:—

Time of falling 10°.
120 seconds.
128 ,,
132 ,,

The pressure was continued 15 minutes and was then removed for 10 minutes, when five more observations were taken, with the following results:—

Time of falling 10°.
24 seconds.
25 ,,
27 ,,
26 ,,
27 ,,

57. In another experiment made on the 31st of July, 220 yards of gutta-percha wire were submitted to pressure in the same manner as before. Before the pressure was put on three observations were made on the insulation, in each case the time occupied in falling 10° on Milner's electrometer was 15 seconds. A pressure of three tons on the square inch was then applied, and the insulation immediately taken again; in three successive observations the time of the tension falling 10° was 52 seconds. The pressure was then removed, and three more observations gave—

Time of falling 10°.
17 seconds.
17 ,,
16 ,,

58. In one of the above cases the insulation of the gutta percha was increased five times, and in the other three times, and this effect was constant in all experiments, though not quite uniform in amount. The special material of the Gutta Percha Company gave similar results. India rubber improved also in the same manner, but it required care in measurement on account of surface moisture. These experiments, when conducted with the ends of the wire exposed to the air, could not be relied upon; it was necessary to lead them into a metal box with a glass cover, dried by chloride of calcium, and containing the electrometer, which was read through the glass, suitable air-tight sliding wires being provided for making and breaking the contact.

On the Effect of Lengthened Application of the Battery Power on Insulation.

On the effect of lengthened application of the battery power on induction.

59. It has been observed from the earliest experience that the conductivity or leakage of a percha cable is greatly diminished if a positive current be applied for a considerable length of time, and, on the other hand, it is increased by the continued action of a negative current. No satisfactory explanation of the cause has been given; one supposition is, that it is owing to the electrolytic decomposition of the water contained in the percha, and the consequent opposite polarization of the copper wire and the earth plate, the one being covered with a film of, perhaps, "nascent" hydrogen, and the other with oxygen, forming an opposing influence to that of the battery; but this

would not sufficiently account for the change; it is accompanied by a weak spontaneous discharge after the removal of the battery, and its study is interesting, as it may throw light on some of the causes of failure in cables.

60. (May 23.) A mile of percha wire $\frac{3}{16}$nds of an inch in diameter, containing a copper conductor $\frac{3}{72}$nds of an inch in diameter, was submitted to the influence of a battery for several hours, and observations made every 30 seconds. The following table will sufficiently explain the results. The battery power employed was 512 cells, and the leakage on the first application of the positive current was 19° on the suspended galvanometer:—

	Deflection of Galvanometer.		Deflection of Galvanometer.
First application of the battery with positive current.	19·0	End of 14th minute	12·9
		„ 15th „	12·8
		„ 16th „	12·6
End of half minute -	18·9	„ 17th „	12·3
„ 1st „ -	17·8	„ 18th „	12·1
„ 2nd „ -	16·7	„ 19th „	12·0
„ 3rd „ -	15·8	„ 20th „	12·0
„ 4th „ -	15·5	„ 21st „	11·8
„ 5th „ -	14·7	„ 22nd „	11·6
„ 6th „ -	14·4	„ 23rd „	11·5
„ 7th „ -	14·1	„ 24th „	11·4
„ 8th „ -	14·0	„ 25th „	11·3
„ 9th „ -	13·7	„ 26th „	11·2
„ 10th „ -	13·4	„ 27th „	11·1
„ 11th „ -	13·2	„ 28th „	11·1
„ 12th „ -	13·0	„ 29th „	11·0
„ 13th „ -	13·0	„ 30th „	11·0

The battery was now disconnected, and the cable put to earth one minute, it then gave 11·8°, and after being to earth a minute more 11·9°, showing that the effect of the positive current was passing away rapidly; it was then left disconnected for five minutes longer, when the leakage was found 12·8. During the interval spontaneously formed discharges were taken from the cable every 30 seconds, the swing of the needle being at first 2°, and decreasing regularly to about 1°. There appears to be a relation between the decreasing strength of these discharges and the falling off of the "polarization" of the cable. The battery was now applied for one minute, and the leakage fell to 12·5.

61. The cable was connected to earth for three minutes, and the connexions of the battery being changed, the *negative* current was applied to the cable, the deflection was—

Negative current first applied -	15·0
End of half minute - -	15·0
„ one „ - -	15·2
„ two „ - -	15·6

The poles were again reversed, and the *positive* current at its first application gave 15·0, and at the end of half a minute 14·0. The poles being again changed, the *negative* current was applied for 10 minutes; the leakage was as follows:—

First application of negative current -	16·3
End of 1st minute - -	16·1
„ 2nd „ - -	16·0
„ 3rd „ - -	16·3
„ 4th „ - -	16·4
„ 5th „ - -	16·5
„ 6th „ - -	16·7
„ 7th „ - -	16·8
„ 8th „ - -	17·0
„ 9th „ - -	17·1
„ 10th „ - -	17·4

The cable was discharged and left disconnected 15 minutes, it then gave no spontaneous discharge, but the insulation on the first application of the *negative* current was 19·5, and continued the same after 30 seconds' application. The *positive* current now gave 17·5, and at the end of 30 seconds 16·5, decreasing in a few minutes to 15·8 and 13·6. The *negative* current was now applied for 30 minutes, viz.:—

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	Deflection of Galvanometer.		Deflection of Galvanometer.
First application of negative current.	18·9	End of 15th minute	21·1
		„ 16th „	21·1
End of 1st minute -	19·5	„ 17th „	21·2
„ 2nd „ -	20·0	„ 18th „	21·2
„ 3rd „ -	20·0	„ 19th „	21·2
„ 4th „ -	20·2	„ 20th „	21·2
„ 5th „ -	20·2	„ 21st „	21·2
„ 6th „ -	20·3	„ 22nd „	21·3
„ 7th „ -	20·4	„ 23rd „	21·4
„ 8th „ -	20·4	„ 24th „	21·4
„ 9th „ -	20·5	„ 25th „	21·4
„ 10th „ -	20·5	„ 26th „	21·4
„ 11th „ -	20·6	„ 27th „	21·5
„ 12th „ -	20·8	„ 28th „	21·5
„ 13th „ -	20·9	„ 29th „	21·5
„ 14th „ -	21·0	„ 30th „	21·5

The battery was now applied for 60 minutes longer, during which time the leakage gradually increased to 22·0, which appeared to be its maximum.

62. The battery was again reversed and the *positive* current applied for 30 minutes, viz.:—

	Deflection of Galvanometer.		Deflection of Galvanometer.
First application of positive current.	20·1	End of 15th minute	16·6
		„ 16th „	15·4
End of 1st minute -	19·5	„ 17th „	15·2
„ 2nd „ -	19·0	„ 18th „	15·0
„ 3rd „ -	18·5	„ 19th „	14·8
„ 4th „ -	18·0	„ 20th „	14·6
„ 5th „ -	17·7	„ 21st „	14·4
„ 6th „ -	17·2	„ 22nd „	14·3
„ 7th „ -	17·0	„ 23rd „	14·1
„ 8th „ -	16·8	„ 24th „	14·0
„ 9th „ -	16·6	„ 25th „	14·0
„ 10th „ -	16·4	„ 26th „	14·0
„ 11th „ -	16·2	„ 27th „	14·0
„ 12th „ -	16·0	„ 28th „	13·9
„ 13th „ -	15·9	„ 29th „	13·9
„ 14th „ -	15·8	„ 30th „	13·9

The deflection appeared to attain its minimum after a further application of the battery for 50 minutes, when the deflection was reduced to 11·0, exactly one-half the number of degrees attained with the opposite current. The cable was connected to earth for 15 minutes, and then gave, with the *positive* current, 11·2, decreasing in one minute to 11·1. On the application of the negative current it immediately gave 15·1, increasing to 15·2; the experiment was then discontinued.

On the Retardation of Signals.

On the retardation of signals.

63. There is no phenomenon in electricity that has a more important bearing on the electric telegraph than that of induction, and none which interferes more with the commercial success of telegraphic enterprise. If it were not for this evil presenting itself in the form known as retardation of the current, any telegraph cable, however long, could be worked at almost any speed; and although much may be done to reduce its effects, there is at present no known method of avoiding them altogether. The effects of induction were foreseen long before the telegraph came into practical use.

64. In April, 1850, M. Werner Siemens first described the effects of induction as they had been observed by him in the subterranean lines of Prussia. These effects were the reception of charge and its retention after the cessation of contact with the battery.

65. The retardation of current,—the peculiar feature, however, which forms the great difficulty in modern telegraphy,—was not encountered or foreseen by him. This phenomenon was first witnessed by myself on the 20th of March 1852, during some experiments made by me at the works of the Gutta Percha Company, in the presence of Dr. Scoffern and other gentlemen.

On that occasion 100 miles of wire were immersed in a canal, and 175 miles were stored up dry in the manufactory, and an endeavour was made to work an electro-magnet and

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Bain's printing apparatus through the whole length. In doing so it was found there was no difficulty in working even with a very small battery power through the whole length, but it was instantly noticed that the current took a very perceptible time in travelling the 100 miles. The experiment was varied in several ways, both with the electro-magnet and with Bain's instrument, but the result was uniform, that when even the current had to travel through the whole length of the wire before acting on the magnet, retardation was perceived. In one experiment the battery and the electro-magnet were placed at one end of the wire, and the contacts were made and broken at the other; but although the battery and the magnet were close together, the same retardation was perceived. It was noticed also that the marks made on the Bain's printing paper by the current were not only slow in appearing, but often their appearance, instead of ending abruptly and instantaneously as they do in the overground lines, the mark tailed off gradually to a point. The cause of the phenomenon was at once perceived to be induction, and to verify it the 100 miles in water were detached, and the 175 miles on dry land used alone, and, as was expected, no retardation whatever was perceptible.

66. Before entering on the consideration of the laws which govern induction in submarine wires, it will be useful to describe the phenomena as they present themselves in practice. The following experiments down to section 91 were performed by myself in the autumn of 1853. They were so interesting and novel that Professor Faraday and Professor Airy were invited to witness their repetition, and they attended with other gentlemen in October for that purpose. In January 1854 the former gentleman made them the subject of a lecture at the Royal Institution, and from this circumstance they are frequently but erroneously quoted in electrical works as Faraday's researches on submarine cables; that eminent philosopher was, however, careful to state the source from whence he derived them. (See Exp. Researches, vol. iii., p. 508.)

67. One hundred miles of No. 3 gutta-percha wire were immersed in a canal, the distant end of the wire was permanently disconnected from the earth. The near end was also disconnected, but had a galvanometer attached, which indicated any current which passed into or out of the wire. The battery was arranged with one pole permanently connected with the earth. The other pole of the battery was now brought into contact with the line wire. The galvanometer was immediately and very violently deflected to the right by a current passing into the wire, but this current ceasing almost immediately, the needle resumed its nearly vertical position and remained quiescent. The connexion with the battery was now broken, and the galvanometer having no current passing became perfectly vertical. Things being in this position, that is to say, both ends being disconnected from the battery, the near end was connected with the earth; the galvanometer was now deflected in the opposite direction by a returning charge quite as violently as in the first instance, proving that a charge had been retained in the wire.

68. Another experiment was made similar to the preceding, except that the discharge was taken from the distant end of the wire instead of from the end at which it had been introduced. The galvanometer was deflected in the same manner and direction as before, nor was any difference observed in its amount of deflection.

69. All the arrangements were the same as before in every respect, except that the 100 miles of wire, instead of being in water, were lying in coils in a dry store-room.

When the pole of the battery was brought into contact with the wire in the manner described in experiment 88, no deflection whatever could be perceived, nor could any discharge be obtained on making connexion with the earth, thus proving that the presence of a conducting medium was necessary on the outside of the wire, and that the phenomenon was caused by induction.

70. One pole of the battery being put to earth, the other was placed in contact with one end of the 100 miles wire, in water, and also with one end of the 100 miles of dry wire, the distant ends of both wires being connected with earth, with a galvanometer on each wire to indicate the quantity of current passing; the battery current dividing passed half through the dry wire and half through that in water, and no difference could be perceived in the strength of the current passing through each.

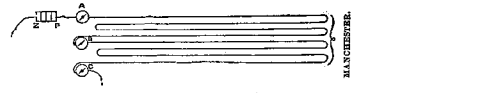
71. In another experiment 18 plates of copper, each about 4 feet × 3 feet, were laid one upon the other, being separated from each other by thin sheets of gutta percha (of the thickness of writing paper); they were then connected into two alternate groups of nine plates, half of them being connected with the copper, and the other half with the zinc pole of a battery. (The surface of each group of nine plates was about equal to three miles of gutta-percha wire.) On making connexion a distinct charge was seen to enter the plates, and a discharge was received from them after the battery had been disconnected, but the indications were feeble; one of the groups was connected to earth instead of the battery pole, the opposite battery pole being also to earth. The results were the same.

72. At another experiment, made at Lothbury, October 15, 1853, the length of wire in circuit was 1,490 miles, of which 70 miles were in dry boards, the remainder being in iron or earthenware pipes buried underground along the railway between London and Manchester.

The gutta percha was a full quarter of an inch in diameter, and the interior copper wire $\frac{1}{16}$th of an inch; each wire was lapped round with cotton tape dipped in coal tar and then dusted with fine sand. One hundred miles of the copper wire have 8,250 feet surface.

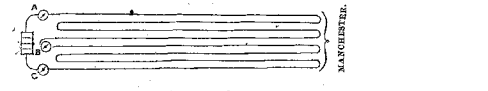
The wires were loosely bound into a bundle of eight wires by twine, and drawn into the pipes, thus forming eight independent circuits from London to Manchester, which were all joined up into one circuit of 1,490 miles.

The battery power used was 508 cells of Daniell's battery, each plate 4 inches square.



All the eight wires were joined up into one continuous length, viz., the zinc pole of the battery to earth, the copper pole through a galvanometer to the line wires, thence to Manchester and back twice, and through a second galvanometer, again to Manchester and back twice, and through a third galvanometer, and then to earth. On making contact with the battery, the needle of the first galvanometer was deflected with great violence for an instant, and immediately afterwards settled at an angle of nearly 90°. After a lapse of a full second or more the needle of the second galvanometer was deflected in the same direction, not with violence, as in the case of the first, but rather slowly, and tardily settling to an angle of about 40° or 50°. Again, after a still longer interval the current appeared to reach the third galvanometer, the needle deflecting feebly with a slow movement, but increasing gradually, apparently by jerks or pulsations, till it remained at an angle of 15° or 20°. On disconnecting, the first galvanometer fell back first, the second and third last.

73. In a further experiment the same arrangement was much as above, except that the third galvanometer was disconnected from the earth, so that the current had no outlet at the distant end of the wire. On making a contact with the battery as before, the needles of the first and second galvanometers were successively deflected to the same extent and in the same manner as before; but the third galvanometer, having no outlet beyond it, of course suffered no deflection. On now making contact with the earth, at the third galvanometer, a considerable discharge of the pent-up electricity took place, deflecting the needle powerfully (in the same direction as the others), but the first discharge subsiding immediately, the needle remained, as before, quiescent at an angle of about 15° or 20°.



74. This experiment is similar to the former one, except that there is no earth connexion used, the poles of the battery being connected directly to the exterior galvanometer, A and C forming a closed circuit. On making connexion with the battery, both the exterior galvanometers A and C were violently deflected in a parallel direction by a charge of positive and negative electricity evidently passing into the respective wires, the needles afterwards continuing deflected strongly in the same direction. The intermediate needle B was not deflected until after an interval of a full second or more, when it settled quietly in the same direction without any jerk. In repeating the preceding experiments the poles of the battery were reversed, and the battery power diminished, but without any marked difference in the results.

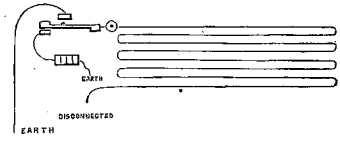
When the battery connexion was broken, and the galvanometers A and C connected together, a powerful discharge returned through each of them, deflecting them in a reverse direction to that caused by its entrance.

75. The experiment was repeated with only 800 miles wire and 144 cells battery. A and C were revulsed by the

returning discharge, as before, and after a short interval B also fell back to its position with some abruptness.

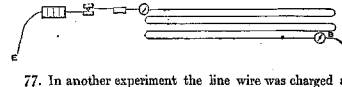
The experiment was then repeated, but when the battery was disconnected the ends A and C were *not* joined together. In this case the needles A and C of course fell vertically by their own gravity. After a considerable interval B fall back *slowly* to its vertical position, and without the abruptness mentioned in the last paragraph.

In all these experiments the exterior galvanometers A and C were powerfully and instantly deflected, but it was not till after a lapse of a considerable interval (more than a second) that the intermediate galvanometer B was deflected (it being separated from each of the other galvanometers by 750 miles of wire); its deflection was not sudden, but rather tardy. In repeating the preceding experiments, the poles were changed, and the power diminished, without any decided difference of results.



76. In another experiment one pole of the battery was put to earth permanently; the other pole was put into contact with one end of the wires (all joined on to each other = 1,490 miles) by a finger key, which when pressed down put the line wires into contact with the battery, the current passing through the galvanometer into the line wire. When the spring returned up again the battery connexion was broken, but at the same time a contact was made with the earth, and the induced current returned out of the line wire through the galvanometer and the key to earth.

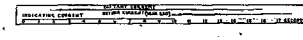
On depressing the finger key a powerful charge passed through the galvanometer (this was partly due to the imperfect insulation of the wires, which admitted a considerable escape to earth, but principally to a current of induced electricity flowing into and charging the wire, the latter effect ceasing immediately, but the former remaining permanent), and the needle deflected violently in one direction. On raising the key the return current deflected the needle still more powerfully in the opposite direction, at the same time completely reversing its poles. This was repeated many times. The return charge or inductive discharge was also taken after intervals of four or five seconds. In the Gutta Percha Company's works this return current was in one case very evident after a lapse of five minutes, during which time the wire (100 miles) had been totally disconnected from the battery.



77. In another experiment the line wire was charged as before, and the battery connexion at A having been broken, the distant end was suddenly put to earth through a galvanometer B. The discharge was not so powerful as when taken at the near end, but was of the same character, so that if a positive current were sent into the wire, the return current was positive from whichever end of the wire it were taken. The return current did not leave the wires instantly, but coming with great violence at first, diminished gradually but rapidly to nothing.

78. A current was sent through both galvanometers A and B, with the line wires intervening; both needles were ultimately deflected to the right, B moving much slower and more feebly than A, as in experiment 72. When the battery was disconnected at A, the return current had two opportunities of escape, viz., to the earth at A, and to the earth at B. The greater portion returned through A, throwing the needle violently back to the left; but a considerable current continued to pay out at B, holding that needle over for a second or more in the same direction that the original current had deflected it, the needle A settling to its vertical position a little sooner than B.

79. In the following experiment no galvanometers were used, but the currents were indicated on a strip of paper, moistened with a solution of prussiate of potash and nitrate of ammonia, kept moving by clock-work at the rate of about 2 feet per minute. Three iron needles rested on the paper as it travelled, and whenever a positive current passed from the needles into the paper a blue mark was formed. The apparatus used for sending currents was the same as that described in experiment 76 (with a slight addition for sending an indicating current), and had the following properties. When the finger key F was depressed, the battery was thrown on to the line wires, at what we shall term the "near" end. The distant end had at all times connexion with the earth through the needle B, so that a current passed from the near end through the line, and became apparent on the needle at the distant end. When the finger key was allowed to fly up again the connexion with the battery ceased, but at the same time the near end of the line came into contact with the stud G, and thus had access to earth through the near needle A. In this, the normal state of things, both the near and distant ends of the line were in contact with earth through the respective needles A and B. The third needle, C, was only used by means of a small local battery to indicate the exact moment of the depression or raising of the finger key. All the needles made marks in parallel lines along the paper. (In a subsequent experiment there was also a fourth needle in connexion with a pendulum, which made a dot on the paper every second to indicate time.)



80. The finger key was depressed so as to charge the line with a positive current, and was held down 10 seconds. On first doing so no current whatever was apparent on the needle at the distant end, but after ½ second a faint indication became apparent, which increased to the end of the third and fourth second, when the blue line became of uniform intensity, being not greater than would be formed by two pairs of plates on short circuit. At the end of the 10th second the battery current ceased at the nearer end, but the current continued to flow uniformly out of the distant end until the 12th second, fining off gradually to invisibility between the 14th and 15th second. But at the end of the 10th second the near end of the line was put to earth as well as the distant end, and, accordingly, a current of great power returned out of the line, burning a hole completely through the paper, afterwards diminishing gradually till between the 18th, 19th, or 20th second, when all traces of it disappeared. This current continued, therefore, to flow four seconds after the distant current had ceased. (The distant current also flowed about the same time after the battery current ceased.)

81. In this experiment the return current possesses great power at its first contact with the earth, diminishing instantly to a more moderate character, but at the end of $\frac{5}{10}$ths of a second it appears to assume a second maximum of intensity, forming a well defined spot (see figure), which is perfectly uniform in the time of appearance in every experiment. It may have some connexion with the pulsations observed in experiment 72. After this node the current diminishes with uniformity to nothing. If the key be depressed but a short time so as not to charge the line wire fully, this node appears sooner after contact with earth, even down to $\frac{1}{10}$th of a second. When the current was held on three seconds instead of 10, the node appeared after $\frac{4}{10}$ths of a second, and so on.

82. When the current was sent into the wire for two seconds and allowed to return for two seconds, this being repeated several times, the current appeared at the distant end in a *constant stream*, varying slightly in its force, but never ceasing to flow. When the current was sent and allowed to escape at rapid intervals (by keying rapidly), no mark whatever appeared at the distant end.

83. Two naked iron wires suspended in the air between London and Liverpool were connected together at Liverpool and attached to the machine, thus forming a circuit of about 500 miles. Each wire communicated with the earth at London. On sending a current along the wire it appeared at the distant end almost instantaneously, and ceased to flow almost at the same instant that it was stopped at the near end.

Even this slight loss of time might be accounted for by the fact that about eight or ten miles of the wire were laid under ground in the streets of London, Manchester, and Liverpool, besides passing in wooden troughs through a great many wet tunnels.

There was a slight tailing off or tapering of the marks of the distant current both at its commencement and termination; and when the key was raised, a return current in the form of a well defined dot; both which phenomena may be attributed to the proportion of under-ground work. The evening was wet when this experiment was tried, and with the intensity of the battery power there would doubtless be some escape of the current from one wire to the other without passing through Liverpool. This circumstance would slightly affect the results of the experiment.

84. A current was sent on two of the wires, their ends at Manchester being disconnected from the earth. A very

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delicate horizontal galvanometer was interposed between two other neighbouring wires and the earth, to see if any inductive charge entered or left them when the current was made, or interrupted on the original wires; the current arising from partial contact between them, about 37°, was sufficient to prevent any decisive observation when the current was sent; but when it was interrupted, the needle fell quietly back to zero without any jerk or indication of an induced current.

85. A charge was now given to 800 miles of wire at the near end A, as in experiment 77, the distant end B not being connected to earth. On suddenly receiving the charge from the end A, it was found to be a great deal more powerful than that from B. If, however, an interval of two seconds were allowed to elapse after the disconnexion of the battery, the charge appeared to equalise itself perfectly, and was as strong at one end as at the other. The comparisons were made by inclining the needle of the galvanometer, while resting against a stop, to such an angle that the discharge had barely power to deflect it a little further in the same direction.

86. A charge was now given to 400 miles of wire through a series of very powerful resistance coils of very fine wire. The discharge was not so powerful as when the wires were charged direct from the battery (see section 128).

87. Four hundred miles were now connected with the earth at each end, resistance coils being interposed between the battery and the earth at the near end. On disconnecting the battery, as in experiment 76, no return discharge was evident on the galvanometer, the whole charge appearing to escape at the distant end. On lessening the resistance considerably, the discharge began to become evident. Signals could be made on a galvanometer at the distant end at the rate of two a second. The coils were afterwards interposed between the battery and the line, but the results were similar.

88. Four hundred miles of wire were joined up as before, without resistance coils, the battery and galvanometer being permanently joined to the line at the near end. On making and disconnecting contact with the earth at the distant end, signals could be given on the galvanometer at the other end after intervals of three-eighths of a second, the needle moving at first with a slight jerk. This is similar to the American method of printing, in which electro-magnets are used at the opposite ends of the line from the batteries, or batteries in constant action distributed throughout the line.

89. Four wires with an intermediate battery were charged in the same manner as in experiment 74, and when charged the battery was removed, leaving the two portions of the line at A and C separated. On now connecting the half A with the earth through a galvanometer, it was found to contain a charge of positive electricity, and in the same manner the other half C gave a discharge of negative electricity.

91. In May 1854, at the separate request of both Professor Faraday and Professor Airy, an experiment was made on 800 miles of subterranean wire, to ascertain what difference of velocity was occasioned by varying the number of cells of the battery. The arrangements described in section 79 were used, the fourth needle being used to mark a dot on the paper every second. The battery power was gradually varied from 31 cells to sixteen times 31 cells, or 516 cells; the time occupied before the first appearance of the current was about half a second, and it was sensibly the same for all the tensions tried, the only difference visible being in the strength of the mark on the paper. The quantity of the current was also varied by connecting the batteries laterally instead of in series, and the effect, contrary to expectation, was apparently a slight diminution of the velocity of the current.

On the Measurement of Induction.

On the measurement of induction.

92. The causes of the foregoing phenomenon were well understood. The inner conducting wire in each of the cases above stated answers to the inner coating of a Leyden jar, the water or moist earth on the exterior answers to the outer coating. When the battery is applied to the length of the wire, with its end insulated from the earth, a charge is given precisely similar in its nature to that in the Leyden jar. But whereas from the small size of the Leyden arrangement we require a tension of 200 or 300 cells to make effects apparent, the enormous area of the Leyden arrangement, as formed by a mile of wire, enables them to be very manifest. On an ordinary galvanometer, even with a tension of only one cell, if the wire be disconnected from the battery and allowed to remain insulated; this charge will remain in the wire for a length of time dependent on the degree of insulation of the wire. In some cases it has been known to remain sensible in the wire for even one or two days.

93. In determining the quantitative laws which govern the accumulation of electricity in cables of varying dimensions, the first necessity which presents itself is that of obtaining the means of measuring the quantity or *amount of induction* with accuracy. A great deal of attention was devoted to this subject before this difficulty was satisfactorily overcome. The amount of discharge from a cable as measured by the swing of the needle of a horizontal galvanometer was the method usually employed and ultimately adopted. As the question to be determined was, whether the quantities of electricity represented by the numbers 1, 2, 3, 4 would deflect the needle through arcs having the same ratio to each other, and if not, what that ratio might be. If the question be examined theoretically, we have to regard the needle as a pendulum, and the velocity with which it is deflected from its normal position is equal to that with which it returns back again past the centre, and this is known to be proportional to the chord of the arc of vibration. Upon endeavouring to apply this law in practice, it was at once observed that different galvanometers gave very different results, and therefore it became necessary to determine the matter experimentally. With this object a great many galvanometers were examined, and in most of those in which the needle was suspended by a fibre, it was found that when small arcs were used the number of degrees of swing or the arc of vibration was nearly proportional to the amount of induction. There were considerable variations in different instruments, but in one particular galvanometer the above law prevailed to such an extent that even in an arc of 60° or 70° its swing was almost exactly proportional to the force of the induced current. This instrument was therefore employed afterwards exclusively for these observations.

94. This instrument is of ordinary construction, but is of unusually large size. It was manufactured by Mr. W. T. Henley.*

The following experiment was taken in order to ascertain the effect of the increase of tension on the amount of induction in wires, but it will serve at the same time to illustrate the degree of reliance which may be placed on the galvanometer, and on the relative value of its indications at different arcs of vibration.

95. Four similar coils of gutta-percha wire were placed in water, each coil being one mile in length. The copper wire was about $\frac{1}{16}$th of an inch in diameter, and the gutta percha of sufficient thickness to make the whole diameter about a quarter of an inch. The battery power employed in the experiment varied from 4 cells to 256 cells, being increased successively from 4 to 8, 16, 32, 64, 128, and 256 cells, and in each case the swing of the galvanometer was taken with coils of wire 1, 2, 3, and 4 miles in length attached. The battery was suddenly thrown into connexion with the wire by means of a key similar to that described, and figured in experiment 76. As the charge entered the cable it caused the needle to swing to the left, and the number of degrees was carefully noted. The key was held down in each case 30 seconds, and during this interval the connexions of the galvanometer were reversed by means of a rheotrope, and the needle was rendered stationary, so that when the key was afterwards allowed to spring suddenly back, the return current or discharge passing out of the wire to earth caused the needle to swing a second time in the same direction, the object being to avoid any inequality which might arise from the adjustment of the instrument, the form of the coils, or the position of the needle. The results of some of these experiments are given below. It must be remembered that the charge on entering the wire is increased by the amount of leakage or conduction through the percha, and therefore the discharges which are free from this error are more to be relied on than the charges; this is especially the case when the battery power is considerable. A comparison of the results obtained sufficiently proves that this particular galvanometer, up to a limit of at least 70 degrees, gives oscillations which are strictly proportional to the amount of induction; the deviations from this law being so small as to be within the limits of errors of experiment, it was therefore exclusively employed throughout the whole of the subsequent investigations. The needle is not magnetised to saturation, and is so well tempered that its magnetism has never sensibly varied throughout the whole series of experiments, although it has repeatedly had currents from 512 cells accidentally transmitted through it. Its oscillations are 10 in 33 seconds. The whole of the observations in this report are therefore, unless otherwise stated, taken on this instrument, and are given without any correction.

* The index is formed of a thin strip of whalebone. The needle is of very thin steel, 3¾ in. long, and rather less than ¼ in. broad. It is suspended by a single fibre of silk, 10 inches in length. Each of the frames which support the coils of wire is 7 inches long and 2¼ inches deep, and the clear space for the wire is 1¾ inches. The instrument is wound with several thousand turns of fine silk-covered wire ·008 inches in diameter, and its electrical resistance is equal to 385 miles of ordinary No. 16 copper wire. It is covered with a glass shade, and furnished with the usual facilities for adjustment.

96. TABLE of the Induction of Four similar Miles of Gutta Percha covered Wire, with varying Battery Power. Diameter of Copper $\frac{4}{32}$; Thickness of Percha $\frac{3}{32}$; External Diameter $\frac{8}{32}$; Current on 30 Seconds before discharge; Temperature 55°. 30th Dec. 1859.

Battery Power. (Neg. to Cable.)	1 Mile.		2 Miles.		3 Miles.		4 Miles.	
	Charge.	Discharge.	Charge.	Discharge.	Charge.	Discharge.	Charge.	Discharge.
4 cells	1·0	1·0	1·9	2·1	3·0	2·9	4·0	3·8
	1·0	1·0	1·9	2·0	3·0	2·8	3·8	4·0
	0·9	1·0	1·9	2·0	3·0	2·8	4·0	3·8
	1·0	1·1	2·0	1·9	3·0	2·9	3·9	3·9
	0·9	1·0	1·9	2·0	3·0	2·8	3·9	4·0
Average	**·96**	**1·02**	**1·92**	**2·00**	**3·00**	**2·84**	**3·92**	**3·90**
8 cells	2·0	1·7	3·6	3·8	5·9	6·0	7·8	7·7
	2·0	1·8	3·8	3·8	5·9	6·0	7·7	7·9
	2·0	2·0	3·9	3·8	5·9	5·9	7·9	7·9
	1·9	2·0	3·9	3·7	5·8	6·0	7·8	7·8
	1·9	2·0	3·9	3·8	5·8	5·9	7·7	7·7
Average	**1·96**	**1·90**	**3·82**	**3·78**	**5·86**	**5·96**	**7·78**	**7·80**
16 cells	4·0	4·0	7·9	7·8	11·9	11·8	15·7	15·4
	3·9	3·9	8·0	7·8	11·9	11·8	15·6	15·6
	3·9	3·9	7·9	7·8	11·8	11·7	15·8	15·4
	4·0	4·0	7·9	7·8	11·9	11·8	15·6	15·4
	3·8	3·9	8·0	7·8	11·9	11·7	15·6	15·6
Average	**3·92**	**3·94**	**7·94**	**7·80**	**11·88**	**11·76**	**15·66**	**15·52**
32 cells	8·2	7·5	15·5	16·0	23·6	23·1	31·0	31·0
	8·1	7·8	15·8	15·8	24·0	24·0	31·5	31·2
	8·3	7·7	16·0	16·0	23·2	23·2	31·0	31·0
	8·0	8·0	15·9	15·8	23·5	23·5	31·6	31·6
	7·9	8·0	15·9	16·0	23·5	23·6	31·0	31·2
Average	**8·10**	**7·80**	**15·82**	**15·92**	**28·60**	**23·48**	**31·22**	**31·20**
64 cells	15·9	15·7	31·3	31·0	47·0	47·0	63·5	63·0
	15·5	15·5	31·6	31·0	47·1	46·8	63·5	62·8
	15·5	15·5	31·4	30·8	47·2	46·3	63·6	63·0
	15·5	15·5	31·4	31·0	47·2	46·8	63·0	63·0
	15·3	15·4	31·3	30·9	47·3	46·4	63·2	62·8
Average	**15·54**	**15·52**	**31·40**	**30·94**	**27·16**	**46·66**	**63·36**	**62·92**
128 cells	31·0	30·9	64·0	63·0	101·	=101·	153·0	166·0
	31·0 leakage ·6	31·0	63·7 leakage 1·0	63·0	101·	=100·	157·0	153·0
	31·0	31·0	64·1	63·3	100·	= 99·		
	31·0	30·6	64·0	63·1	100· leakage 2·	=100·		
	31·0	30·6	64·0	63·0	100·	=100·		
Average	**31·00**	**30·82**	**63·96**	**63·08**	**100·04**	**100·00**		
256 cells	61·8	61·8						
	62·1 leakage ·9	62·1						
	62·0	62·1						
	62·2	62·2						
Jan. 5, 1860	62·0	62·0						
Average	**62·02**	**62·04**						
Charge of each mile of wire taken singly, 128 cells.	31·2		31·5		31·5		31·0	

97. The use of a suspended needle would be insufferably tedious for these observations, if recourse were not had to some means of checking the vibrations and bringing the needle to rest; this is, however, readily effected by the use of a very feeble magnet, such as the point of a slightly magnetised penknife, directed, not towards either end of the needle, but towards its centre, so that the attraction of one end of the needle equals the repulsion of the other; and although the oscillations are rapidly checked, no pendulous vibration is communicated to the needle. The dexterity which is acquired by practice in the use of this contrivance is surprising; it is not an unusual thing to bring the needle, while swinging in an arc of 20°, to absolute rest by one single dexterous application of the magnet, and two or three alternate applications of the point and heel of the knife at gradually increased distances will at all times effect the same object.

98. It may be stated, once for all, that a great deal of personal skill and experience are required in the conduct of these experiments, as well as inexhaustible patience. The experiments were chiefly performed at the factory of the Electric Telegraph Company, in Gloucester Road, Camden Town, which closely adjoins the goods station of the London and North Western Railway Company. Unfortunately the disadvantages of this position were not sufficiently foreseen, for the continued passage of the engines engaged in shunting the trains had a most vexatious influence on the indications of the galvanometer, and, even at a distance of 20 or 30 yards, would cause a sensible deviation of the needle, sometimes amounting to as much as two degrees.

In the more delicate experiments, such as those on short lengths of wire under pressure, this evil was so great that it was frequently necessary to suspend the experiments. Indeed, the causes of error are so numerous and so easily overlooked, that very few persons are capable of performing such experiments satisfactorily.

On the Effect of Variation of Battery Power on Induction.

On the effect of variation of battery power on induction.

99. Having obtained the means of measuring with satisfactory accuracy the amount of induction, the first point investigated was that of the effect of varying the battery tension. The table just referred to (section 96) contains experiments on this subject, and it will be seen that *the amount of induction varies directly as the tension of the battery*, as shown by the observations taken with tensions varying from 4 cells up to 256 cells. These results were confirmed by numerous other experiments, the details of which it is unnecessary to give. The law was proved to hold good, even with the extremes of 1 cell and 508 cells.

App. No. 2. Report by Mr. L. Clark.

On the Effect of Variation in the Diameter of the Conducting Wire and the Thickness of the Insulator on Induction.

On the effect of variation in the diameter of the conducting wire and the thickness of the insulator.

100. The determination of the extent to which the amount of induction is dependent on the relative size and proportions of the wire and its covering was one of the most important in the whole investigation. Although the laws which govern the amount of induction in the two cases are entirely distinct, it is more convenient to treat of them together.

In order to arrive at this determination, five separate lengths of experimental wire were, in the first instance, manufactured by the Gutta Percha Company, each one mile in length. In three of them the thickness of the coating of gutta percha was varied in proportions of 1, 2, 4, the diameter of the copper wire remaining constant. The other two miles, taken together with one of the preceding miles, formed a series in which the diameter of the copper wire was varied in proportions of 1, 2, 4, while the thickness of gutta percha remained constant. Subsequently four more lengths were manufactured in order to make the series complete, so that there were, in all, nine sizes, in three series. Their dimensions, and the results obtained with them, are given in the following tables:—

101. TABLE of Induction of Wires of varying size covered with Gutta Percha of varying thickness. Battery Power 64 Cells, Daniell's, Positive to Cable. Dimensions given in 32nds of an Inch. Temperature 54°. Feb. 24, 1860.

Diameter of Copper Conductor.	Thickness of Dielectric. 3. Charge.	3. Discharge.	Leakage.	6. Charge.	6. Discharge.	Leakage.	12. Charge.	12. Discharge.	Leakage.
2.	15·6	15·4	·1	11·4	11·3	·0	8·7	8·7	·0
	15·6	15·5	·0	11·2	11·3	·0	8·8	8·6	·0
	15·5	15·5	·0	11·3	11·4	·0	8·7	8·7	·0
	15·7	15·5	·1	11·4	11·3	·0	8·6	8·7	·0
	15·5	15·4	·0	11·3	11·3	·0	8·6	8·6	·0
	15·5	15·5	·0	11·3	11·2	·0	8·7	8·7	·0
	15·56	15·46		11·31	11·30		8·68	8·66	
4.	25·4	25·0	·3	16·8	16·4	·0	11·8	11·7	·0
	25·4	25·0	·3	16·5	16·4	·0	12·0	11·8	·0
	25·4	25·0	·3	16·5	16·5	·0	12·0	11·8	·0
	25·3	25·0	·2	16·6	16·5	·0	11·9	11·8	·0
	25·3	25·0	·2	16·7	16·5	·0	11·9	11·8	·0
	25·5	25·0	·2	16·4	16·4	·0	11·9	11·8	·0
	25·38	25·00		16·59	16·46		11·91	11·78	
8.	33·1	32·7	·5	23·5	23·4	·2	16·6	16·7	·1
	33·1	32·6	·5	23·7	23·4	·2	17·0	16·7	·1
	33·0	32·6	·5	23·6	23·5	·2	17·0	16·4	·3
	32·9	32·4	·5	23·6	23·4	·2	16·7	16·7	·2
	33·0	32·6	·5	23·4	23·4	·2	16·7	16·4	·1
	32·9	32·6	·3	23·5	23·4	·2	16·7	16·6	·2
	32·98	32·58		23·55	23·40		16·78	16·61	

102. TABLE of Induction of Wires of varying size covered with Gutta Percha of varying thickness. Battery Power 64 Cells, Daniell's, Negative to Cable. Dimensions given in 32nds of an Inch. Jan. 6, 1860.

Diameter of Copper Wire.	Thickness of Dielectric. 3. Charge.	3. Discharge.	Leakage.	6. Charge.	6. Discharge.	Leakage.	12. Charge.	12. Discharge.	Leakage.
2.	16·8	15·4	·1	11·6	11·2	·4	10·4	9·0	1·1
	16·1	15·9		11·3	11·3		10·2	9·0	
	16·2	15·8		11·8	11·3		10·0	9·0	
	16·2	15·6		11·9	11·0		10·0	8·6	
	16·4	15·6		11·8	11·0		10·0	9·2	
	16·34	15·66		11·78	11·16		10·12	8·96	
4.				17·4	17·0	·6			
				17·0	17·0				
				17·1	16·8				
				17·4	16·8				
				17·3	16·7				
				17·24	16·80				
8.				26·8	25·3	1·0			
				26·6	25·6				
				26·7	26·4				
				26·6	25·8				
				26·8	26·0				
				26·68	25·60				

103. TABLE of Induction of Wires of varying size covered with Gutta Percha of varying thickness. Battery Power 128 Cells, Daniell's, Negative to Cable. Dimensions given in 32nds of an Inch. Jan. 6, 1860.

Diameter of Copper Wire.	Thickness of Dielectric. 3. Charge.	3. Discharge.	Leakage.	6. Charge.	6. Discharge.	Leakage.	12. Charge.	12. Discharge.	Leakage.
2.	32·7	31·2	·3	23·9	22·4	·8	20·2	17·4	1·9
	32·4	31·3		23·8	22·5		20·2	17·3	
	32·6	31·1		23·8	22·5		20·1	18·0	
	32·7	31·4		23·8	22·7		20·3	17·8	
	32·8	31·5		23·3	22·5		20·3	17·8	
	32·64	31·30		23·72	22·53		20·22	17·76	
4.				35·0	33·3	1··			
				35·0	33·4				
				34·8	33·5				
				34·8	33·0				
				35·0	33·3				
				34·93	33·30				
8.				52·8	51·0	·9			
				52·7	51·0				
				52·8	51·2				
				52·4	51·5				
				52·4	50·8				
				52·62	51·10				

104. The three foregoing tables are virtually repetitions of each other, but taken with varying battery power. The two latter tables were the first that were made, and are only given for comparison. Where the battery power is large and the cables do not insulate perfectly, it is obvious that the swing of needle produced by the charge will be increased by that due to the amount of leakage, for this reason the discharge only should be regarded. The first of the three tables, which is at once the most complete and most reliable, was taken with every precaution, and a rheotrope was used between the observation of charge and discharge, so that both were observed on the same side of zero. When due allowance is made for leakage, it will be seen by this table that the amount of charge and discharge are sensibly equal.

For the sake of reference and for the convenience of inspection, we take the average induction measured by the discharge in the subjoined condensed table:—

Diameter of Copper Wire.	Thickness of Dielectric. 1.	2.	4.
1	15·46	11·30	8·66
2	25·00	16·46	11·78
4	32·58	25·40	16·61

105. The examination of this table will show that with wire of ordinary dimensions the amount of charge or of induction may be taken as varying *inversely as the square root of the thickness* of the coating of gutta percha, and also *directly as the square root of the diameter* of the copper conducting wire. Thus, if we compare the induction of the series of wire having the thickness of dielectric No. 4 with that having thickness No. 1, we shall find the induction in the ratio of 1 to 2 approximately; and if, on the other hand, we compare the series having the respective diameters of the copper conductors 1, 4, we shall find the ratios to be as 1 to 2, similarly with those of the intermediate wire or series. It will follow from this law, that if we increase the diameters of the conductors and the thickness of dielectric in the same proportions, the induction would remain constant, and this is found to be the case when we compare the wire of diameter 1 and the thickness 1 with that of diameter 2 and thickness 2, or of diameter 4 and thickness 4, or the wire of diameter 2 and thickness 1 with that of diameter 4, thickness 2. In fact, the results are closely accordant with this law, that in gutta-percha wires *the amount of induction varies as the square root of the diameter of the conductor, and inversely as the square root of the thickness of the dielectric*; or $I = \sqrt{\frac{d}{t}}$

106. Now in air the quantity of electricity accumulated under induction between two opposing flat surfaces varies inversely as the thickness of the plate of air intervening, and not as the square root of the thickness;* it was an interesting point, therefore, to ascertain whether this singular departure from the usual law was due to the cylindrical form of the coating in percha wires, or whether the law of

* Sir W. S. Harris, Rudimentary Electricity, 3rd edition, p. 169.

variation with different thicknesses of percha was different to that in air. Some experiments made with frictional electricity, and described at section 292, had indicated that the latter supposition was correct, and it was confirmed by the following experiment:—I obtained several plates of percha three feet square, and flat plates of metal of the same size, and by means of the differential inductometer described at section 109, the induction was measured at different thicknesses, by ascertaining the lengths of percha wire which under the same circumstances gave the same inductive capacity. A plate of metal was placed on a table, then a plate of percha and a second metal plate, then another plate of percha and a third metal plate; the top and bottom metal plates were connected with the earth, and the induction of the centre plate was measured with a battery power of 256 cells, with the following results:—

Thickness of Plate of Percha above and below Metal Plate.	Equivalent Length of Percha Wire, No. 2 size, with No. 16 Copper Wire.
One-eighth of an inch.	165 feet
Two-eighths ,,	97 ,,
Four-eighths ,,	58 ,, 4 inches.

When plates of air were used instead of percha, the induction at the distance of one-eighth of an inch was equal to that of 69 feet 4 inches of wire, and at the distance of four-eighths of an inch it was equal to that of 16 feet 6 inches of wire. This last result is in tolerably close accordance with received views, but in the case of percha plates the results approximate more nearly to those obtained with cylindrical wire, as given in section 105. There is probably some thickness at which the induction of plates of air and percha would be the same. It is evident that the variations with different substances would be represented by curves, and their further study is a point of much interest.

107. The law alluded to in section 99, that the induction of a cable is directly proportional to the number of cells of the battery, affords a method of testing the induction of wires which may be convenient to the telegraphist who has not at hand a galvanometer whose indications are known. If we note the deflection given by a known mile of telegraph wire with the known number of cells, and if we take another mile of wire and vary the number of cells until it gives the same deflection on the same instrument, the relative induction of the two cables will be inversely as the number of cells. Very great reliance may be placed on this ready method of comparing the induction of cables. Another method of measuring induction is to give a known charge, say 10° to a given length *A*, and place it momentarily in contact with the unknown wire *B* of similar length, so as to divide the charge between them and then to measure the charge left in each of them. The charge will divide itself in proportion to their relative inductive capacities.

108. Messrs. Siemens have employed an instrument for measuring the induction of submarine cables, in which they use a reversing key driven by an excentric which gives a rapid succession of charges or discharges from a cable. These charges in passing through a galvanometer cause a permanent and nearly steady deflection of the needle, which may be read with great accuracy. The instrument is described in a paper read before the British Association in July 1860, and is alluded to in another part of this report. It is obvious that the amount of this deflection of the needle depends entirely on the number of charges transmitted through it in a given time, and the system necessitates the use of a mechanical arrangement, by which these charges may be transmitted with perfect synchronism and that at different periods. This is by no means an easy thing to attain with accuracy, and the necessary machinery is both inconvenient and costly; and when attained, a certain calculated correction has to be made in order to render the different readings of the galvanometer comparable. Professor Wheatstone has modified this machine by driving the excentric by a multiplying wheel, and has thereby made its indications surprisingly delicate; when driven rapidly, with a battery power of 500 cells, it will readily exhibit on a galvanometer the succession of discharges from 12 inches of wire $\frac{1}{16}$th inch in diameter, suspended in an apartment.

The writer has adapted the system of rapid motion to a differential instrument, with three reversing keys, and has thereby constructed a *differential inductometer*, which will probably, when better known, be generally adopted by telegraphists for the measurement of induction on short or experimental lengths of cable.

109. The ordinary induction or discharging key which forms the principle of this instrument is well known; it has long been the custom to increase the sensibility of the indications on a slowly oscillating galvanometer, by keying rapidly and sending a great number of charges through the instrument during each oscillation of the needle to one side, and waiting quiescent during its return. The instrument in question operates on this principle, but is provided with a battery reverser, and is thereby made differential in its character. The instrument transmits positive and negative currents alternately through the galvanometer, and these are always equal in number whatever the speed of the instrument; the positive currents all come from one piece of cable, and the negative currents from the other, with which it is to be compared; and if both pieces have the same dimensions their opposite effects on the galvanometer will neutralize each other, and the needle will remain stationary; but if the inductive capacity of the two wires is unequal the needle will become deflected to one side or the other.

Fig. 1.

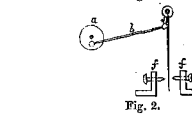


Fig. 2.

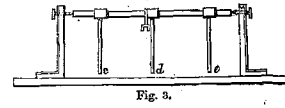
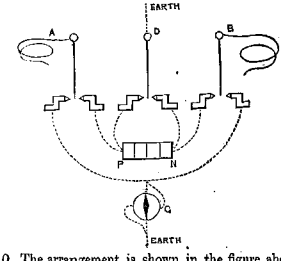


Fig. 3.



110. The arrangement is shown in the figure above, in which the pulley *a* carries an eccentric and rod, which at each revolution cause the arm *b* to vibrate to and fro, carrying with it the insulating rod of vulcanite which forms its axis, and three silver springs *c*, *d*, *e* (one of which only is shown), which are fixed at different points along the rod, as shown in figure 2, and are insulated from each other. These springs at each vibration alternately make contact with the other two platinum points *f*, *f*, which are about one-eighth of an inch apart, and are also insulated from each other. The points and springs are each connected with terminals, which are not shown. This comprises the whole machine. Its operation will be understood by referring to the diagram, figure 3, in which the three springs depending from the vibrating rod are again shown, but, for the purpose of clearness, in different planes. A and B are connected with the short lengths of wire under comparison; the centre spring D being connected with the *earth*; P, N, is a battery connected with four of the points in the manner shown, and most carefully insulated from the earth; G is a very sensitive galvanometer, having one of its terminals connected with the two remaining points, and the other with the earth.

It will be seen that when in the course of vibration all the springs move to the left, the cable B is in a position to receive a negative charge from the battery, its positive pole being to earth through D, and that the cable A, which had received a positive charge at the previous vibration, is now in a position to discharge itself through the galvanometer to earth; and similarly when the springs move to the right, the cable B sends its discharge through the galvanometer, while A obtains a fresh charge from the battery.

But since all the discharges of A are positive, and those of B are negative, their effects tend to neutralize each other. The needle of the galvanometer therefore remains stationary so long as the induction of the two lengths is equal, but inclines to the right hand or left if there is any inequality.

Art. No. 1. Report by Mr. L. Clark.

On the effect of variation in the diameter of the conducting wire and the thickness of the insulator.

111. The indications become still more delicate if a length of about half a mile of well insulated wire be substituted, for the galvanometer, and all the discharges from both the small lengths of the cable be thrown into this. If the cables are perfectly similar, the charges of positive and negative electricity being equal, will exactly neutralize each other, however long the machine be turned; but if either of them preponderates, the excess will gradually accumulate in the large wire, and will become evident when this latter is occasionally connected with the galvanometer, its character being at the same time indicated by the direction in which the needle swings. Its effects are still further increased by only making connexion with the galvanometer synchronously with its swings. By these means, with a battery of 500 cells, and a galvanometer with 30,500 convolutions of wire and rapid rotation, the extra immersion of half an inch of telegraph wire, or even the approach of the hand, were rendered readily perceptible. An eighth of a square inch of tin-foil on a coated pane will also affect the galvanometer, so that the instrument is well adapted for comparing the electrical capacities of globes, cylinders, plates, &c., of varying dimensions, and for determining the specific inductive capacities of different materials. As no standard unit has yet been generally adopted, it is convenient in practice to keep a standard of, say, 100 inches or more of wire of any convenient size, and to ascertain what length of any new wire is equal to this standard. The inverse proportion will give its relative inductive capacity. It is even more convenient to have a standard fixed in the base of the instrument, and a piece of tin-foil a few inches square between two planes of coated glass will be found large enough for this purpose.

112. The connexions of the instrument may be readily varied so as only to send positive currents into each cable, their direction on returning through the galvanometer being alternately reversed; this change of connexion is effected by substituting the galvanometer for the battery in the preceding figure, and putting one pole of the battery permanently to earth. It obviates the necessity for the perfect insulation of the batteries from earth. On the other hand, both the charges and discharges of a piece of wire may be made to pass in succession in the same direction, and thereby to conspire in their effect upon the needle. The machine may also be employed for sending rapidly reversed currents.*

On the Effect of Variation in Temperature on Induction.

On the effect of variation in temperature on induction.

113. As it was known that variations in temperature had great influence on the conduction of insulating materials, it was not unnatural to expect that it might have an equally powerful effect upon induction. This has not, however, proved to be the case. The inductive capacity of almost all the materials experimented upon appears to be increased by increase of temperature, but only to an inconsiderable extent. The experiments on this subject were made at the same time as those on the insulation of wire, and the arrangements are fully described in section 48. Wires covered with gutta percha and other insulating materials were immersed in a large tank of water, as has been already described, and the temperature artificially regulated, the induction being observed at each successive increase of 10° from 32° to 92°. The measures were taken in the manner described at section 95; and the following very necessary precaution was taken in order to eliminate errors which might arise from the variations in the magnetism of the needle and the tension of the battery. Two miles of gutta-percha wire, Nos. 1 and 2 in the table, were placed in a separate cistern, and kept throughout the whole experiments at a temperature of 32° by ice. These wires were used as a standard of comparison for each series, and the indications of the galvanometer were compared and adjusted by their means. The number of cells was varied, so as to make their indications the same in every experiment. The variations in the tension of the batteries were so considerable, that experiments taken at different dates without this precaution could not be relied on.

114. On examining the subjoined table, it will be found that all the wires give a slight increase at a higher temperature, except the "special" wires of the Gutta Percha Company and the india-rubber wires of Messrs. Silver and Co. The wire most affected by temperature appears to be Hughes' fluid cable, but even in this case the amount is small. The india-rubber wire of Messrs. Hall and Wells show, apparently, a much greater change; but this was probably occasioned by the mechanical construction of the wire, the water penetrating but slowly through the fibre of the exterior coating. Some of the other wires appeared to undergo a permanent increase, as, for example, that called Wray's compound. The cause of this is not understood.

115. TABLE of Inductive Capacities of various Telegraph Wires at different Temperatures. Length of each Wire one Statute Mile; Battery Power 128 Cells, Daniell; Positive to Cable; Measurements taken on a Galvanometer with suspended Needle Discharge only recorded.

The Dimensions are all given in 32nds of an Inch; the Length of each Wire one Statute Mile.	Diameter of Copper conducting Wire.	Thickness of Coating of Dielectric.	Total external Diameter.	Temp. 32°.	Temp. 42°.	Temp. 52°.	Temp. 62°.	Temp. 72°.	Temp. 92°.	Temp. 52°.*	General Average.
1. PLAIN GUTTA PERCHA (A.)—Kept always at 32° as a standard of comparison.	(Temperature always 32°.)			30·0 30·0 30·0 30·0 30·1	30·1 30·3 30·1 30·2 30·0	30·1 30·0 30·2 30·1 30·1	30·1 30·2 30·0 30·0 30·1	30·0 30·2 30·0 30·0 30·0	30·0 30·1 30·0 29·9 30·0	30·0 29·9 30·0 30·0 29·9	
	2	3	8	**30·02**	**30·14**	**30·10**	**30·08**	**30·04**	**30·00**	**29·93**	**30·04**
2. CHATTERTON'S COMPOUND (D.)—Kept always at 32° as a standard. Copper wire of this and the preceding No. 16 gauge. (*See* No. 6.)	(Temperature always 32°.)			30·1 30·2 30·3 30·0 30·0	30·3 30·4 30·4 30·4 30·4	30·4 30·6 30·5 30·7 30·6	30·3 30·4 30·3 30·3 30·3	30·2 30·1 30·3 30·0 30·2	30·4 30·1 30·0 30·2 30·2	30·1 30·1 30·1 30·2 30·2	
	2	3	8	**30·12**	**30·36**	**30·56**	**30·32**	**30·16**	**30·18**	**30·14**	**30·26**
3. PLAIN GUTTA PERCHA.—One of the wires used for observations on the effect of variation in size.				31·5 31·7 31·6 31·6 31·6	31·5 31·6 31·4 31·5	31·5 31·4 31·4 31·3	31·4 31·2 31·4 31·6 31·4	31·8 31·7 31·7 31·4 31·7	32·2 32·2 32·4 32·3 32·3	31·1 31·3 31·4 31·4 31·2	
	2	3	8	**31·60**	**31·50**	**31·40**	**31·40**	**31·66**	**32·28**	**31·28**	**31·59**
4. PLAIN GUTTA PERCHA (C.)—Ordinary wire belonging to the Electric Telegraph Company; the wire No. 16 gauge.				30·0 29·8 29·8 29·9 29·7	30·0 30·0 29·8 30·0	29·7 29·8 29·9 30·0	30·0 29·7 30·0 29·8 30·0	30·2 29·9 30·0 30·0 30·2	32·0 32·1 32·1 32·2 32·1		
	2	3	8	**29·84**	**29·95**	**29·85**	**29·90**	**30·06**	**32·10**		**30·26**

* The instrument is manufactured by Messrs. Elliott, Brothers, of Charing Cross.

115. Table of Induction of various Telegraph Wires at different Temperatures, &c.—*continued.*

App. No. 2. Report by Mr. L. Clark.

The Dimensions are all given in 32nds of an Inch; the Length of each Wire one Statute Mile.	Diameter of Copper conducting Wire.	Thickness of Coating of Dielectric.	Total external Diameter.	Temp. 32°.	Temp. 42°.	Temp. 52°.	Temp. 62°.	Temp. 72°.	Temp. 92°.	Temp. 62°.	General Average.
5. Plain Gutta Percha.—One of the wires used for observations on the effect of variation in size.				22·8 22·8 22·8 22·7 22·7	22·8 22·8 22·7 22·7	22·6 22·8 22·8 22·7 22·7	22·6 22·8 22·8 23·0 22·7	22·7 22·5 22·4 22·9 22·9	23·4 23·4 23·5 23·5 23·5	22·5 22·6 22·7 22·6 22·6	
	2	6	14	**22·76**	**22·75**	**22·72**	**22·26**	**22·78**	**23·46**	**22·60**	**22·75**
Chatterton's Compound (D.).—Wire of gutta percha in two coatings, with a thin layer of compound (gutta percha and Stockholm tar?) upon the wire and between the two coats of percha. Copper No. 16 gauge.				29·7 22·9 29·9 29·7 30·0	29·8 29·8 29·7 29·7	29·8 29·8 29·9 29·9	29·7 29·7 29·4 29·4 29·6	29·9 29·9 30·2 29·7 30·0	30·2 30·4 30·3 30·4 30·5		
	2	3	8	**29·84**	**29·75**	**29·85**	**29·56**	**29·94**	**30·36**		**29·88**
7. Gutta Percha Co.'s Special Material (A.).—Composition unknown. Resembles gutta percha.				23·7 23·7 23·8 23·8 23·7	23·7 24·0 23·8 23·8	23·9 24·0 24·0 24·0	23·4 23·4 23·4 23·5 23·5	23·6 23·4 23·6 23·7 23·4	22·7 22·8 22·9 23·0 23·0	23·7 23·7 23·9 24·0 23·8	
	2	3	8	**23·74**	**23·82**	**23·97**	**23·44**	**23·54**	**22·88**	**23·82**	**23·60**
8. Gutta Percha Co.'s Special Material (B.).—Composition unknown; laid on in coatings alternating with thin layers of Chatterton's compound.				23·0 22·8 22·8 22·9 22·8	23·0 23·0 23·0 23·0	23·0 23·2 23·0 23·2	23·0 22·8 23·0 22·8 23·0	23·4 23·4 23·2 23·3 23·4	23·0 23·0 23·0 23·1 23·2	23· 23·1 22·9 23·0 23·1	
	2	3	8	**22·86**	**23·00**	**23·10**	**22·92**	**23·32**	**23·06**	**23·02**	**23·04**
9. Gutta Percha Co.'s Special Material (C.).—Composition unknown; laid on in coatings alternating with thin layers of Chatterton's compound.				15·0 15·0 15·1 15·2 15·3	15·3 15·3 15·2 15·2	15·2 15·4 15·3 15·4	15·0 15·1 15·0 15·0 15·0	15·3 15·2 15·0 15·2 15·3	14·9 14·8 14·9 14·9 14·7	15·0 15·2 15·1 15·0 15·1	
	2	6	14	**15·14**	**15·25**	**15·32**	**15·25**	**15·20**	**14·84**	**15·08**	**15·15**
10. Radcliff's Special Material (A.).—Composition unknown; prepared by Messrs. Hancock & Co. Stated to be "pure gutta percha only, chemically prepared.				25·0 25·0 25·0 25·0 24·9	25·2 25·3 25·0 25·1	25·6 25·4 25·3 25·4	24·8 25·1 25·0 25·2 25·1	25·4 25·4 25·3 25·3 25·4	26·5 27·1 26·5 25·9 26·4	24·3 24·0 24·0 24·3 24·0	
	2	6	14	**24·98**	**25·15**	**25·35**	**25·04**	**25·36**	**26·48**	**24·12**	**25·21**
11. Radcliff's Special Material (B.).—Composition as above (No. 10.)				24·3 24·3 24·1 24·1 24·1	24·4 24·5 24·1 24·0	24·8 24·4 24·6 24·7	24·2 24·4 24·2 24·2 24·1	24·5 24·4 24·4 24·4 24·4	25·4 25·4 25·3 25·4 25·4	25·0 25·1 24·9 24·8 25·0	
	2	6	14	**24·18**	**24·25**	**24·62**	**24·22**	**24·42**	**25·38**	**24·96**	**25·56**
12. Godefroy's Special Material.—Called "Godefroy's Improved Gutta Percha," a mixture of gutta percha and ground cocoa-nut shell.				33·7 33·0 33·6 33·5 33·6	33·5 33·7 33·5 33·4	33·5 33·7 33·6 34·0	33·5 33·5 33·6 33·8 33·6	34·1 34·1 34·1 34·1 34·1	35·2 35·3 35·0 35·4 35·4	34·2 34·2 34·0 33·8 34·0	
	2	3	8	**33·48**	**33·52**	**33·76**	**33·60**	**34·10**	**35·26**	**34·04**	**33·96**
13. Hearder's Submarine Cable.—Covered with gutta percha in two coatings, with a layer of hemp fibre. Saturated with tar between the coatings. (*See* Phil. Mag., May 1859.)	(440 yards, or ¼ mile only.)			9·4 9·3 9·2 9·3 9·3	9·0 9·1 9·2 9·1				9·8 9·8 9·9 9·8 9·9	9·5 9·4 9·6 9·7 9·7	
	4	6	16	**9·30**	**9·10**				**9·84**	**9·58**	**9·43**

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§ 115. Table of Induction of various Telegraph Wires at different Temperatures, &c.—*continued.*

The Dimensions are all given in 32nds of an Inch; the Length of each Wire one Statute Mile.	Diameter of Copper conducting Wire.	Thickness of Coating of Dielectric.	Total external Diameter.	Temp. 32°.	Temp. 42°.	Temp. 52°.	Temp. 62°.	Temp. 72°.	Temp. 92°.	Temp. 52°.	General Average.
14. TWENTY ALTERNATE COATINGS.—Ten coatings of gutta percha alternating with ten coatings of Chatterton's compound. (*See* No. 6.)				20·6	20·6	20·6	21·0	21·0	22·5	22·3	
				20·4	20·7	20·7	20·9	21·3	22·5	22·2	
				20·4	20·7	20·8	21·0	21·4	22·6	22·3	
				20·4	20·8	20·7	20·8	21·1	22·7	22·2	
				20·5		20·9	21·1	21·3	22·7	22·3	
	2	6	14	**20·46**	**20·70**	**20·74**	**20·64**	**21·22**	**22·60**	**22·26**	**21·23**
15. HUGHES' FLUID CABLE.—Wire covered with two coats of gutta percha and enclosed loosely in an exterior percha tube, the interstice being filled with a fluid resembling tar.				21·4	21·6	22·3	22·0	22·6	25·0	21·5	
				21·5	21·5	22·2	22·0	23·0	25·0	21·5	
				21·4	21·7	22·3	22·1	22·6	25·0	21·5	
				21·3	21·7	22·4	22·1	22·7	25·0	21·5	
				21·3		22·4	22·1	22·8	25·0	21·5	
	2	6	14	**21·38**	**21·62**	**22·32**	**22·06**	**22·74**	**25·00**	**21·50**	**22·37**
16. SILVER & Co.'s INDIA RUBBER (B.)—The india rubber masticated, cut into strips, wound spirally on in many layers, and rendered homogeneous by heat.				15·9	15·8	15·8	15·7	15·6	15·7	16·0	
				15·8	15·9	16·1	25·7	15 7	15·9	15·9	
				15·8	15·8	16·1	15·6	15·6	15·8	16·0	
				15·9	15·8	16·1	15·7	15·6	15·8	15·9	
				15·8		16·1	15·5	15·7	15·8	16·0	
	2	5·24	12·48	**15·84**	**15·82**	**16·04**	**15·64**	**15·64**	**15·80**	**15·96**	**15·82**
17. SILVER & Co.'s INDIA RUBBER (C.)—(For description, *see* No. 16.) The exterior was covered with a thin adherent coating of cotton felt, not included in measurement.				14·4	14·3	14·6	14·1	13·9	14·1	14·3	
				14·4	14·3	14·6	14·2	13·8	13·9	14·3	
				14·4	14·3	14·6	14·1	13·9	14·2	14·3	
				14·5	14·2	14·5	14·3	14·0	14·1	14·4	
				14·3		14·7	14·2	14·0	14·0	14·2	
	2	6·2	14·4	**14·40**	**14·28**	**14·60**	**14·18**	**13·92**	**14·06**	**14·30**	**14·25**
18. HALL and WELLS' INDIA RUBBER (A.)—Wire covered with cotton thread, and lapped with spiral layers of Para india rubber masticated; then covered with elastic vulcanized india rubber thread wound very tightly.				29·6	31·8	34·0	37·0	39·5	45·3	44·0	
				29·6	31·7	34·2	37·0	39·7	45·2	44·1	
				29·6	32·0	34·0	36·8	40·3	45·3	44·2	
				29·7	32·0	33·9	37·0	40·4	45·6	44·3	
				29·5			36·9	39·9	45·2	44·0	
	2	3	8	**29·60**	**31·87**	**34·02**	**36·94**	**39·96**	**45·32**	**44·12**	**37·40**
19. HALL and WELLS' INDIA RUBBER (B.)—(For description, *see* No. 18.)—The first layer of pure cut Para bottle rubber, not masticated. The vulcanized thread in each case not included in measurement.				21·8	23·1	23·7	24·4	26·5	31·1	31·2	
				22·0	23·0	23·9	24·5	26·7	31·4	31·3	
				22·0	23·0	23·6	24·5	26·7	31·3	31·5	
				22·0	23·0	23·6	24·5	26·6	31·3	31·3	
				22·0			24·4	26·6	31·4	31·4	
	2	3	8	**21·96**	**23·02**	**23·70**	**24·46**	**26·62**	**31·30**	**31·34**	**26·06**
20. WRAY'S SPECIAL MATERIAL (A.)—A mixture of india rubber, shellac, powdered silica or alumina, and a little gutta percha, in two or three coatings.				15·5	15·8	15·8	16·0	16·4	19·8	18·7	
				15·6	15·7	16·2	15·7	16·4	19·8	18·5	
				15·6	15·8	16·0	16·1	16·4	19·8	18·6	
				15·7	15·9	16·2	16·0	16·5	19·8	18·5	
				15·7		16·3	16·0	16·5	19·8	18·4	
	2	6	14	**15·62**	**15·80**	**16·10**	**15·96**	**16·44**	**19·80**	**18·54**	**16·89**
21. WRAY'S SPECIAL MATERIAL (B.)—Same composition as No. 20, with the omission of the gutta percha 176 yards, or $\frac{1}{10}$ mile only.					1·6	1·8	1·7	1·7	2·1	1·9	
					1·6	1·9	1·6	1·6	2·1	2·0	
					1·6	1·7	1·6	1·7	2·0	1·9	
					1·6	1·6	1·6	1·8	2·0	2·0	
								1·6	2·1	1·9	
	2	6	14		**1·60**	**1·72**	**1·62**	**1·68**	**2·06**	**1·92**	**1·77**
Dates at which the observations were taken.		1860.		March 5	March 23	March 28	April 7	April 13	April 26	May 22	
Number of (Daniell's) elements employed.	—	—	—	128 cells	132 cells	132 cells	132 cells	121 cells	120 cells	130 cells	

On the Specific Inductive Capacity of different Materials.

On the specific inductive capacity of different materials.

116. The variation of the amount of induction through different material, to which Faraday has given the name of *specific inductive capacity*, was first noticed by Cavendish, it was re-discovered by Professor Faraday, and is described in a series of papers contributed to the Royal Society in 1842. At the date of Faraday's interesting researches it could little be foreseen that such an obscure phenomenon should be destined to become one day, as it has now, a consideration of high national importance, and one which has a direct and most important bearing on the commercial value of all submarine telegraphs.

In table 115 will be found instances in which wires of similar dimensions, but of different materials, give induction varying in proportion from 16 to 23; and it follows from this, that one of these wires in a given time would send nearly 50 per cent. more messages than another, a consideration of the first importance.

117. An examination of the table and a comparison of the sizes of the wires will show great variations in the specific inductive capacity of different materials; gutta percha, especially; the modification of it called Godefroy's compound, gives a higher induction than any other of the cables examined. Next in the series is Radcliff's special material; then come gutta percha, with coatings of Chatterton's compound, and Hughes' fluid cable; next the wire of gutta percha with 20 alternate coatings, and Hall and Wells' india-rubber wire. The lowest inductive capacity is shown by Wray's compound, the "special materials" of the Gutta Percha Company, and the india-rubber wire of Silver and Co. This table will be again referred to and the respective values of these wires considered when we come to treat of them individually.

On the Effect of Variation in Pressure on Induction.

On the effect of variation in pressure on induction.

118. The effect of variation of pressure upon the induction of submarine cables is an inquiry of great interest, for the pressure of the ocean at great depths is extreme, and its influence on the insulation of cables is very considerable. It appears, however, from the experiments made, that it exercises but a slight influence upon the inductive capacity of materials, not more in fact than might be fairly attributed to their compressibility and consequently diminished size. The apparatus employed in this investigation has been before alluded to under the head of insulation, and is figured in another part of the Appendix. It was chiefly designed by the writer. It consists of a small hydraulic press loaded with weights to maintain the requisite pressure, and of two parallel iron pipes, each one-sixteenth of a mile long, to contain the wire under experiment. The press is formed of an upright steel piston exactly one inch in diameter, working in a cylinder of wrought iron, and having a stroke of 18 inches. It is guided in its motion by a cast-iron frame. On the top of this piston is placed a saddle, from which depend four chains carrying a scale pan, in which were placed iron weights. The pipe leading to this apparatus is connected with two parallel hydraulic pipes of wrought iron in which the wires under examination were placed. These pipes were each 110 yards, or one-sixteenth of a mile in length, and when not in use were open at both ends. They were laid side by side on a platform, which was about three feet higher at the distant end than at the press, to allow the ready escape of the air. The wires under examination were drawn into these pipes, and the ends made secure by a stuffing box of gun metal, a figure of which is given in another part of the Appendix.

119. Under high pressure these joints gave a great deal of trouble, and the india rubber and gutta percha in warm weather would ooze bodily away before the pressure like a liquid. It became necessary to prepare ends of hard vulcanised india rubber to withstand the pressure, but these frequently gave way by cracking. At other times dry cotton was stuffed into the box round the wire before the introduction of the loose cylinder, and the joints made in this way were perfectly water-tight, but the pressure necessary would almost always cause the india rubber to ooze away from the copper wire, the cotton taking its place, and destroying the insulation.

120. In the following experiment 110 yards of gutta percha covered wire, the copper $\frac{5}{8}$ in. diameter, the gutta percha $\frac{3}{16}$ in. in thickness were secured in the hydraulic pipes in the manner described.

"The temperature was 54°, and the time midnight; the pressure employed was 3 tons on the square inch, which is equivalent to 2,555 fathoms of sea water. The battery power was 512 cells, and the instrument and arrangements were the same as those described at section 93. Before the application of pressure 12 observations were taken, and the amount of induction was as follows:—

On the effect of variation in pressure on induction.

AMOUNT OF INDUCTION.

7·0	7·3
7·2	7·4
7·2	7·4
7·2	7·3
7·2	7·4
7·4	7·3

Average - 7·27

Thirty minutes were occupied in making some experiments on insulation, after which the pressure was applied and 14 observations were taken as follows:—

AMOUNT OF INDUCTION.

7·5	7·6
7·5	7·5
7·5	7·5
7·5	7·4
7·5	7·5
7·6	7·4
7·6	7·5

Average - 7·50

The pressure was suddenly taken off, when the induction was as follows:—

AMOUNT OF INDUCTION.

7·5
7·4
7·5
7·4
7·4
7·4
7·4

Average - 7·43

After 20 minutes the pressure was again put on, and the induction given was—

7·5
7·6
7·5
7·6
7·5
7·6

Average - 7·55

The pressure was then suddenly taken off, and the deflections taken were—

7·3
7·4
7·4
7·4
7·4
7·5

Average - 7·40

121. May 3, 1860.—The following experiment was made with a wire 110 yards in length, covered with *Wray's compound*. Diameter of wire $\frac{2·7}{32}$. Battery power 508 cells. Pressure 8,556 lbs. to the square inch, being equivalent to a depth of 3,190 fathoms. The following was the induction before pressure was applied:—

4·6
4·7
4·6
4·7

Average 4·65

Upon pressure being applied, the induction was—

4·3
4·2
4·2
4·2
4·3

Average - 4·24

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The pressure was instantly taken off, when the induction fell, showing—

	3·9
	4·1
	3·8
	4·0
Average -	3·95

The pressure was put on again, when the induction was—

	4·0
	4·0
	4·1
	4·0
Average -	4·02

122. India rubber treated in the same way gave, before pressure was applied, an average of 4·5, and during pressure 4·3. Similar results were also obtained with a wire of the Gutta Percha Company's special material. These experiments show a sensible increase of induction during pressure, and appear to indicate a slight permanent increase after the pressure is removed. It is possible this is due to the condensation of the material and its smaller size. Some attempts were made to verify the conjecture by placing 4-inch cylinders of the material in a cast-iron hydraulic press with suitable means for measuring their compression, but they failed from the repeated bursting of the presses.

On the Comparative Induction of Iron and Copper Conductors.

On the comparative induction of iron and copper conductors.

123. From the well-known fact that electricity under induction dwells only on the surface of conducting bodies, and that its quantity is the same whatever may be the material of which the conductor is composed, it might have been assumed that the induction with iron or steel wires would have been the same as with copper. It was nevertheless thought necessary to make one experiment on this subject, and a mile of iron wire of the same dimensions as one of the experimental copper wires, was covered with gutta percha by the Gutta Percha Company for the purpose of comparison. Its diameter was $\frac{4}{32}$ in.; thickness of gutta percha $\frac{8}{32}$ in., and total diameter $\frac{16}{32}$ in. The induction of this wire was compared with that of the copper wire of similar dimensions with a battery of 128 cells, and the results are given below. The weight of the gutta percha on the copper, as furnished by the Gutta Percha Company, was 134 lbs.; that on the iron 150 lbs.; and this difference in thickness is perhaps sufficient to account for the discrepancy in the amount of induction which is observable in the table. The insulation of the iron wire was rather more perfect than that of the copper, the leakage of the one being 1·3, and that of the other 3·7. This, however, would not sensibly affect the induction. The battery current was positive.

Discharge from One Mile of Copper Wire.	Discharge from One Mile of Iron Wire.
45·6	43·7
45·7	43·8
45·7	43·8
45·8	43·7
45·7	43·7
45·8	43·8
45·71	43·76

124. The subject of the following experiment was to ascertain whether the amount of induction received from a cable would be altered by causing the discharge to pass through a second length of cable. A mile of ordinary gutta-percha wire, $\frac{4}{32}$ in diameter, was connected in the usual way, and the discharge with 128 cells was noted. The observations were—

	32·0
	31·6
	32·0
	31·7
Average -	31·52

The discharge of the same wire was now caused to pass through a second mile of similar wire before it reached the instrument, so that the tension had an opportunity of diffusing itself and becoming lower. The discharges were—

	32·0
	31·7
	32·0
	31·9
Average -	31·90

The connexions were now altered in such a manner that the discharges did not pass *through* the second mile, this last, however, being connected with the same terminal of the galvanometer as the first mile, so that the discharge had the opportunity of temporarily entering the second mile. The results were similar to the last.

	31·7
	32·0
Average -	31·85

125. The next experiment is somewhat similar in character. Two separate miles of gutta-percha wire, copper $\frac{4}{32}$, external diameter $\frac{8}{32}$, were experimented on. For convenience we will term these wires A and B. The battery power was 128 cells. Date 11th March 1860.

The induction from A alone was—

	39·4
	38·9
	39·0
	39·3
Average -	39·22

Induction from B alone:—

	38·7
	38·8
	38·7
	38·8
Average -	38·75

B was then charged and quickly connected with A, and the two together connected to the galvanometer, so that the charge, after dividing itself between the two wires, passed through the galvanometer. The discharge was, however, sensibly the same, viz.:—

	39·0
	39·0
	38·8
	38·9
Average -	38·93

B was next charged and joined for an instant to A, then separated, and the discharge of A taken. The induction was—

	20·3
	20·0
	19·7
	19·7
Average -	19·92

A was then charged and momentarily connected with B, and separated. The discharge of A was—

	20·0
	20·0
	20·1
	19·9
Average -	20·00

A was now charged and connected with 20 miles of cables of various descriptions, joined together in a bunch, and the discharge from the whole taken on the galvanometer. The induction was—

	39·5
	39·6
	39·0
	39·3
Average -	39·35

A was next charged, and its charge divided with 20 miles as above, and the discharge taken from 20 miles only. The induction was—

37·0
37·0

A was charged, and divided with 20 miles as above, and the discharge taken from A only. The discharge was—

2·0
2·0

A was again charged, and the discharge, before reaching the galvanometer, caused to pass through a mile of gutta-percha wire, as in section 145. The induction was—

39·0
39·0

A was charged, and the discharge caused to pass through 20 miles of wire joined up in a continuous circuit, and interposed between A and the galvanometer. The induction here was—

38·7
39·0
39·0
39·0

Average - 38·92

126. Another wire, C, was discharged in the same manner as the foregoing, through 22 miles of wire, giving the following results:—

39·7
39·6
39·8
39·7

Average - 39·7

C was now discharged *direct* through the galvanometer, but the 22 miles of wire were previously connected together in the form of a loop, with both ends at the same terminal of the galvanometer as C, *so that* the discharge had an opportunity of temporarily entering the 22 miles of wire before passing through the galvanometer. The discharge was—

39·5
40·0
39·7
40·0

Average - 39·53

127. The general deduction to be drawn from the foregoing experiments is, that the effect of the discharge on the galvanometer is not sensibly absorbed or altered by being allowed to pass through or enter into an additional length of wire, although we are sure, from the resistance of the galvanometer, which was equal to about 325 miles of ordinary No. 16 copper wire, that the discharge must at least have been for a time locked up inductively in the second wire. In fact, in the experiment in which one wire was charged and joined to the other, and then both put to the galvanometer, we are certain that this was the case. The effect on the galvanometer was somewhat increased by this division of the charge, but the increase was no greater when 22 miles were added than with one mile. Another thing we learn is such as might have been expected, namely, that when the given charge is divided among several lengths of wire it distributes itself among them all in the direct ratio of their inductive capacity. If all the wires are of similar dimensions, then the proportions of the charge in each will be equal. A similar result is shown in experiment 85, in which the discharge from 800 miles between London and Manchester, and which was of course at first much more powerful near the battery than at the distant end, equalised itself throughout the whole wire when the charge was allowed to remain insulated in the cable for a few moments, and the discharge became as powerful from the distant end as from the end near the battery.

128. The following experiment affords an illustration of a law very similar to the preceding, and which may be thus stated. If a perfectly insulated cable be connected with a battery the charge will ultimately attain the same maximum, whatever be the length of the cable or whatever be the resistance of the intervening conductor. This law is easily illustrated, even in extreme cases. Thus a considerable charge may be given to a length of cable through the intervention of two feet of ivory, or through a piece of deal 3 feet long, or through a piece of dry string. The following case, however, is on a more practical scale. One mile of plain gutta-percha wire was charged with a battery power of 128 cells, a resistance equal to about 700 miles of line being interposed between the battery and the cable, so that the charge and discharge had to pass through this length of wire before reaching the cable.

Charge.	Discharge.
30·4	30·3
30·5	30·3
30·4	30·5
30·5	30·4
30·5	30·3
30·46	30·36

The same wire was now charged with the same battery, and connected direct to the galvanometer without any resistance. It will be seen that the induction, as given below, is sensibly the same as it was when it passed through 700 miles of resistance.

Charge.	Discharge.
30·4	30·7
30·1	30·7
30·4	30·4
30·6	30·9
30·4	30·6
30·38	30·66

129. The wires employed in these experiments were all wound in coils of about 2 feet 6 inches in diameter. In the gutta-percha wires the coils were so loose that the water readily penetrated to every part of them, and it might be safely assumed that the static induction would be the same coiled up as when laid *out* in straight line. The india-rubber wire was, however, coiled so neatly and closely on the wooden drums, that it became doubtful whether the water on the exterior could enter between the coils and give free passage to the current of electricity leaving the exterior wire. To ascertain this a coil containing about half a mile of Silver and Co.'s india-rubber wire (No. 17 in the tables) was placed in a canal, and the induction measured in the usual way with a battery of 128 cells. The discharge given was—

6·2
6·3
6·4
6·5
6·4
6·3
6·4

Average - - 6·37

The cable was then unwound and laid straight in the canal, and the induction again measured; the results were—

6·4
6·4
6·5
6·5
6·5
6·4
6·4
6·3

Average - - 6·42

130. The wire used in the preceding experiment was covered with a thin coating of felt, which possibly facilitated the admission of the water to all parts of its exterior. Another cable of similar dimensions ($\frac{1}{4}$ external diameter) of naked india rubber, 916 yards in length, very closely wound on a wooden drum, was therefore placed in the canal; in this condition its induction was as follows:—

Discharge.

11·7
11·6
11·5
11·4
11·6
11·7
11·5

Average - - 11·54

The same cable was then unwound and laid at full length in the canal, under which circumstances the induction was as follows:—

Discharge.
11·4
11·4
11·6
11·7
11·8
11·6
11·8
Average - - 11·61

The increase, which was inconsiderable, might probably be ascribed to errors of observation.

131. In all the experiments hitherto described our attention has been directed to the induction existing on the interior copper conductor. It might have been inferred from analogous experiments with frictional electricity, that the amount of external induction was exactly equal to that taking place internally, but the following experiments are not without interest. A large tub of water was carefully insulated by suspending it on slings of gutta percha, and about half a mile of gutta-percha wire $\frac{1}{12}$nds in external diameter, with a copper conductor $\frac{3}{32}$, was placed in it. The battery power used was 256 cells. The negative pole was connected with the earth, and the induction was measured in the usual way with the apparatus described at section 93.

In the first experiment the wire was placed in the tub and the charge and discharge from the wire measured, the tub being at this time insulated from the earth. The measurements taken were—

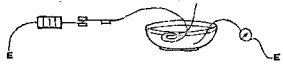
Charge.	Leakage.	Discharge.
·7	·2	·6
·7	·3	·5
·7	·25	·55

The charge in the above experiment was of course caused by the induction of the surrounding walls and ceiling of the room. The water was now connected with the earth, and the observations repeated. The results were—

Charge.	Leakage.	Discharge.
28·4	1·8	26·4
28·2	1·8	26·2
28·3	1·7	26·3
28·2	1·7	26·4
28·30	1·72	26·32

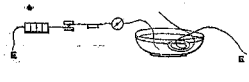
132. The connexions were now changed in the manner shown in the above figure, and the current from the battery, instead of being sent into the wire, was sent into the water in the tub, while the wire was connected through the galvanometer with earth. In this, as in the other experiments, the connexions of the galvanometer were reversed between the charge and discharge, so that both were taken in one direction, and it is scarcely necessary to remark that in comparing the charge and the discharge, we should add in each case the leakage to the latter. The observations taken under the new conditions just described were as follows:—

Charge.	Leakage.	Discharge.
27·7	1·7	*26·0
28·0	2·0	26·0
28·1	2·0	26·0
27·9	2·1	25·9
27·92	1·95	25·97



133. The galvanometer was now removed from the coil of wire and placed in circuit with the wire leading to earth, so as to measure the amount of electricity passing from the tub to earth. The battery was connected directly with the wire. The results were now—

Charge.	Leakage.	Discharge.
28·4	2·0	26·0
28·1	2·0	26·0
28·0	1.7	26·0
28·0	1·8	25·5
28.12	1·87	25·88



134. The galvanometer was now removed into the position shown in the above figure, so as to measure the amount of electricity passing from the battery into the water. The cable was connected to earth, and the measurements taken in each case show the amount of electricity entering the water of the tub.

——	Charge.	Leakage.	Discharge.
When cable not to earth.	·7	·3	·6
	·7	·3	·6
When cable to earth.	28·7	2·4	26·2
	28·7	2·4	26·0
	28·9	2·4	26·2
	28·6	2·5	26·0
Average -	28·72	2·42	26·1

135. The connexions were now replaced in the manner described at section 131, but the positive and negative poles were alternately put to cable. The following tables give the results:—

——	Charge.	Leakage.	Discharge.
Negative to cable.	27·7	2·4	26·0
	28·4	2·4	26·2
Average -	28·05	2·4	26·1
Positive to cable	28·6	1·7	26·3
	28·3	1·8	26·2
Average -	28·45	1·75	26·25
Negative to cable.	28·1	2·2	26·2
	28·3	2·1	26·3
Average -	28·2	2·15	26·25
Positive to cable	28·4	1·7	26·3
	28·2	1·6	26·3
Average -	28·30	1·65	26·30

136. The foregoing experiments sufficiently prove that the amount of the current which enters the cable is exactly equivalent to that which leaves its exterior, and is, of course, of the same denomination. In the first experiment the electricity which entered the cable was a little in excess of that which left the tub, but the difference is perhaps sufficiently accounted for by the possibility of error in the observation. In the third and fourth experiment we see that the electricity entering the tub is in excess of that which left the cable, but here we have no difficulty in ascribing the difference to the induction of the tub itself upon the surrounding objects, which, as we have seen, amounted nearly to a degree. The results appear, generally, to be the same with negative as with positive electricity. It is instructive too to consider, in these experiments, the changes of tension which take place. When the battery is connected with the tub it raises it, of course, to its full tension, and the insulated wire having no exit for its electricity, acquires exactly the same tension, but with electricity of the opposite denomination. When, on the other hand, the battery is connected with the wire (the tub being insulated), a somewhat different result takes place. The tension of the wire is, of course, that of the battery, but the quantity of electricity disengaged from its exterior having, in the tub, a large surface to diffuse itself upon, is necessarily at a tension somewhat lower than that of the battery, and the larger the tub the lower its tension; these changes of tension, however, have no effect on the indications of the galvanometer, this latter only indicating the quantity which passes through it, which quantity will be precisely equal to that entering the wire. In a cable submerged in the sea the quantity of electricity set free by a positive charge in the wire of course diffuses itself throughout the ocean generally, and in the same manner the electricity called to its surface by a negative charge is abstracted from the whole electricity of the ocean.

137. The following experiments, though they have not much interest, may be worth description. The tub was charged positively, and the consequent induced charge in the cable was allowed to pass off to earth. The cable was then disconnected from earth and the water discharged, leaving, of course, the cable negatively charged. On now applying the positive pole to the cable through the galvanometer, the charge was, from an obvious cause, double the amount of that given under other circumstances. In two experiments thus tried the deflections were 53° and 52°.

In similar experiments performed in November 1854, two tubs were employed, with a coil in each tub; the two coils were connected together, and the battery was applied to the water in one tub and the galvanometer to the water in the other, or the water in the two tubes was connected, and the battery applied to one coil and the galvanometer to the other; the results were precisely similar to the above.

The deflection on half a mile of wire hung up wet in a coil in a store room was quite perceptible on the galvanometer, amounting to ·6. A wet cloth thrown over the coil gave the same induction, viz., ·6. When the wire was connected with the earth the cloth gave an induction of 15·0, and when the cloth was connected to earth the wire gave 14·5.

A piece of canvas eight feet square was wetted and held out in the open air, on insulating bands of gutta percha. The induction, measured with 512 cells, was one degree. The canvas was then held in a store room as far removed as possible from the walls and ceiling; the induction was now more than two degrees; it was then folded into a small compass, and the induction became too small to be perceptible.

138. It became a point of some interest to determine whether there might be any essential difference in induction derived from different sources, as, for example, from a battery, from a magnet, and from a frictional machine. For this purpose one mile of india-rubber wire was taken and charged by a battery of 64 cells, which gave, on a Milner's electrometer, a tension of 9½ degrees. The induction of this wire was measured in the usual way, and the observations gave, on the galvanometer, a swing of—

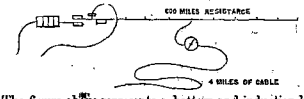
18°
17°
17°
18°

The battery was then removed, and a frictional machine, with a plate two feet by six inches diameter, was connected to the wire. The machine was in very bad order, but after about 50 turns the wire showed, on the Milner's electrometer, a tension of 9½ degrees. The discharge was now taken on the galvanometer in the usual manner, and the induction in four experiments was—

18°
19°
17°
18°

thus indicating that at the same tension the cable gives the same induction, whatever be the source from whence the electricity is derived.

139. The following experiments have a great similarity to those described under the heading "Insulation," but they are important as illustrating accurately the distribution of induced electricity in a long submarine cable.



The figure above represents a battery and induction key connected with a series of resistance coils, equal to 800 miles of line wire. The key is so arranged that when it is pressed down the battery current passes through the whole 800 miles of line to the end, but when raised by its spring no current passes. Four miles of gutta-percha wire were immersed in water, and connected successively at different points along the 800 miles of line, with the intervention of the galvanometer, the battery power employed being 64 cells. When the key was suddenly depressed the current traversed the resistance coils, and a certain portion passed through the galvanometer and charged the four miles of wire up to a certain tension, which tension was, of course, the same as that of the line at the spot at which the galvanometer was connected. The current passing through the galvanometer into the cable caused a deflection proportionate to the tension of the current. The deflections at the successive distances are recorded below. Date of experiment, January 7, 1860.

	Charge.	Discharge.	Leakage.
At 800 miles	57·0	55·9	·7
	57·2	55·6	
	57·0	55·8	
	56·5	55·7	
	56·8	55·6	
Average	56·90	55·72	
At 700 miles	49·4	48·6	·8
	49·6	48·6	
	49·4	49·0	
	49·6	48·6	
	49·4	48 6	
	49·48	48·68	
At 600 miles	42·5	41·2	·8
	42·4	41·2	
	42·2	41·4	
	42·3	41·3	
	42·4	41·2	
	42·36	41·26	
At 500 miles	35·0	34·5	·4
	35·0	34·2	
	34·9	34·2	
	34·8	34·0	
	34·8	34·0	
	34·90	34·18	
At 400 miles	28·0	26·8	·4
	27·6	26·6	
	27·8	27·0	
	27·9	27·0	
	27·5	27·0	
	27·76	26·80	
At 300 miles	21·4	20·5	·5
	21·4	20·6	
	21·40	20·55	

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	Charge.	Discharge.	Leakage.
At 200 miles	14·4	13·4	·4
	14·3	13·5	
	14·35	13·45	
At 100 miles	7·2	6·6	·3
	7·2	6·6	
	7·20	6·60	
At 50 miles	3·8	3·2	·1
	3·8	3·2	
	3·80	3·20	
At 25 miles	1·9	1·6	·1
	1·9	1·6	
	1·90	1·60	
At 0 miles	·0	·0	

If we regard the amount of induction at the respective points, 25, 50, 100, 200, 400, and 800 miles, we shall see at a glance that they are almost exactly proportional to the distance from the end of the line, each deflection being double the preceding one.

The following experiment is almost identical with the foregoing one, the only difference being that the current was allowed to flow steadily and constantly through the 800 miles of wire, and the connexion was made by suddenly bringing the galvanometer and cable into contact with different parts of the line wire. The results are given chiefly in confirmation of the preceding experiment. The battery power was as before, viz., 64 cells, and the positive to line. The key in this experiment was, of course, connected to the cable instead of to the end of the line wire.

	Charge.	Discharge.	Leakage.
At 800 miles	56·8	55·8	·7
	56·5	55·6	
	57·0	55·8	
	57·0	55·9	
	57·0	55 6	
	56·86	55·74	
At 700 miles	49·6	48·4	·9
	49·5	48·6	
	49·5	49·0	
	49·4	48·4	
	49·6	48·4	
	49·52	48·56	
At 600 miles	42	41	·7
	42	41	
	42	41	
	42·1	41	
	42·1	41	
	42·04	41·00	
At 500 miles	34·7	34·2	·5
	34·6	34·0	
	34·6	34·0	
	34·8	34·0	
	35·0	34·0	
	34·74	34·04	
At 400 miles	27·8	27·0	·4
	27·7	27·2	
	27·8	27·2	
	27·8	27·0	
	27·8	27·0	
	27·78	27·08	

	Charge.	Discharge.	Leakage.
At 300 miles	21·2	20·2	·3
	21·0	20·5	
	20·8	20·5	
	20·9	20·4	
	21·0	20·3	
	20·98	20·38	
At 200 miles	14·0	13·6	·2
	14·0	13·6	
	14·0	14·0	
	13·9	13·6	
	13·9	13·8	
	13·96	13·72	
At 100 miles	6·9	7·0	·2
	7·1	6·8	
	6·9	6·8	
	7·0	6·8	
	7·0	6·8	
	6·98	6·84	
At 50 miles	3·6	3·4	·1
	3·7	3·4	
	3·7	3·4	
	3·7	3·4	
	3·8	3·3	
	3·70	3·38	
At 25 miles	1·9	2·0	
	1·7	1·9	
	1·80	1·95	
At miles			

In practical cases and in very long lines the leakage would slightly modify these results.

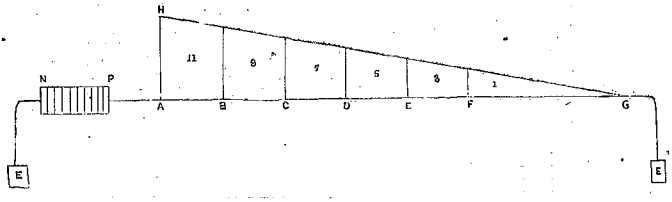
141. Having ascertained that the amount of induction in any given section of cable was directly proportionate to the distance from the battery, it was thought desirable to ascertain in what proportions the discharge of each given section would divide itself between the two ends of the cable after connexion of the battery had been broken, and both ends connected to earth. It might, of course, have been foreseen that the amount of discharge would be in the inverse ratio of the resistances, and the following experiment confirms this. Resistance coils equal to 1,600 miles of line were taken to represent a line, the resistance including that of the galvanometer, which formed part of the circuit, and which was placed at one end of the line. Both ends of the line were connected with the earth, and a discharge from four miles of cable, with a battery power of 64 cells, was made into the line at different points along its length, and allowed to escape to earth at each end. When one end of the line was disconnected from earth (so that the whole discharge had to pass through the galvanometer), the deflection was 56·8. When both ends were connected with the earth a portion only of the discharge passed through the galvanometer. The following are the results:—

	Discharge.		Discharge.
Cable discharged at 0 miles (that is to say, full discharge)	56·8	At 500 miles	40·0
			40·0
			40·2
			40·3
			40·0
			40·10
At 400 miles from galvanometer end of line.	43·8	At 600 miles	36·1
	43·7		36·4
	43·8		36·5
	43·7		36·2
	43·9		36·2
	43·78		36·28

	Discharge.		Discharge.
At 700 miles	32·6	At 1,000 miles	21·8
	32·6		21·9
	32·5		21·8
	32·5		21·8
	32·5		21·8
	32·54		21·84
		At 1,100 miles	18·2
At 800 miles	29·0		18·3
	29·0		18·4
	29·0		18·3
	28·9		18·3
	29·0		18·30
	28·98		
		At 1,200 miles	14·8
			14·8
At 900 miles	25·4		14·8
	25·3		14·8
	25·3		14·8
	25·4		
	25·5		14·8
	25·84	At 1,600 miles (that is, at earth.)	0·0

142. An examination of the above experiment shows, as might be expected, that the discharge from each section divides itself between the two ends in an inverse proportion to its distance from them. But this is true only in the case above cited, where the line wire is free from all electricity except that of the small section of cable under experiment. In practice we have not to consider the discharge from one section of the cable only, but every section is endeavouring to discharge itself at the same time, and as these sections are all charged at *different* degrees, a different law comes into action, which will be treated of further on.

143. We are now in a position to consider the effects of induction in connexion with long submarine cables having their distant end connected with earth in the manner in which they are employed in practice. We have seen that the amount of induction (that is to say the quantity of electricity stored up by induction) in any given portion of a cable varies directly as the tension of the battery current at that point. And we have seen moreover that in a long wire connected at one end to the earth with a current passing through it, the tension is at a maximum near the battery and falls regularly to zero at the distant end. It might naturally be inferred that the quantity of induction at successive points along such cables would also diminish directly as the distance from the battery increases and become 0 at the distant end. This law, which is confirmed by numerous experiments, may be rendered clearer by a diagram.

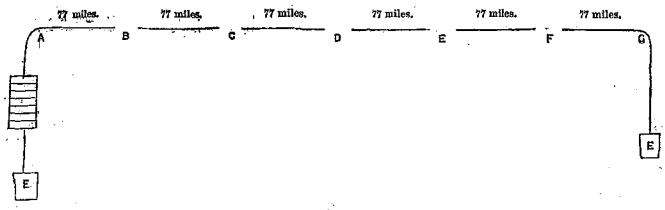


144. Let A, B, C, G represent a long submarine cable with a current flowing through it, and let A, H represent the tension near the battery. Now we know by many previous experiments that the tension at the other points B, C, D, E, F will vary as the distance from the battery, and that the line H, G will represent the tension of the electricity at all points; but we have seen also that the quantity of induced electricity varies directly as the tension. Therefore the perpendiculars B, C, D will correctly represent the quantity of induction in any small section of the cable at those points, and in fact the line H, G may be taken to represent the amount of induction at all points along the line in the same manner as it was formerly taken to represent the amount of tension. The whole amount of induction then in a cable is represented by the triangle H, A, G, and if the triangle be divided into sections by the perpendiculars A, B, C, D, E, F, the area included between any two of the perpendiculars will correctly represent the amount of induction upon that section of the cable. Now it is one of the properties of the triangle, that if the base A, G be divided by any number of equal lengths, and perpendiculars such as B, C, D, &c., be erected upon it, and if we call the area of the smallest section F, G = 1, the area of all the other sections will increase in the ratio of the odd numbers 1, 3, 5, 7, 9, 11, and so on; and this is true into whatever number of lengths it may be divided. Consequently if the whole length of any submarine cable be conceived to be divided into any number of equal parts, the quantity of electricity stored up inductively in each section will be in the ratio of 1, 3, 5, 7, 9, 11, &c. This is a very simple and important law and easily remembered in practice. If, for example, we suppose a cable divided into two halves, the section nearest the battery will have three times the electricity of the section nearest the earth. Or if a cable be divided into three parts, the nearest section will have five parts of electricity, the middle three parts, and the distant section one part, and this disposition of the electricity exists so long as the current is passing. It is also quite independent of the battery power, always bearing in mind that the standard tension of any section depends not on the number of cells, but on the actual tension measured close to the battery.

145. If the battery be now disconnected from the cable, the whole of the electricity will flow out at the distant end to earth; but if, on the other hand, the near end of the cable at the instant it is disconnected from the battery is connected with earth, a much larger quantity of electricity rushes out at the near end than at the distant end, for we have seen that three-fourths of the whole electricity is lodged in or near the half, and, in addition to this, its tension is enormously greater.

146. Before proceeding further with this point we will give some examples of the disposition of the electricity along the different sections of a submarine cable. The experiments about to be described were made on a submarine cable containing six conducting wires and 77 miles in length. The cable, which is the property of the Electric and International Telegraph Company, was lying in the East India Docks. It was not submerged under water, but it is well known that this circumstance makes little or no difference in the amount of induction observed.* It contained six conducting wires, and by joining these end to end, a length of 462 miles was obtained. The coating of gutta percha is $\frac{3}{32}$ inch in thickness, and the copper is No. 16, about $\frac{5}{256}$ in diameter. Fortunately for the experiment, the copper was of the kind in ordinary use before attention was paid to the conducting power of commercial copper, consequently its conductivity was extremely low, and this, together with its small dimensions, made the induction of the cable very considerable, and rendered it very suitable for experiments upon the retardation of currents. In fact the current of electricity sent into one end of the cable occupied ·16 of a second before it appeared at the distant end of the 462 miles. The six conducting wires, before being covered with hemp and iron, were twisted together into a long spiral, and experiments were made to ascertain that this condition and the magnetic influence of the iron did not affect the quantity and speed of the electricity flowing through them.

* It may be well in this place to explain why this should be so. The whole amount of electricity which a cable can hold inductively under any battery power is comparatively small, and we have seen that it is distributed proportionally along the whole length of the cable. Now, whatever amount of electricity enters the cable, precisely the same amount of electricity, of the same character, has to leave the exterior and escape to earth. Now, since the quantity which leaves the whole of the cable is but small, the quantity which remains stored up in any small section must be excessively small. This quantity, whatever it may be, has only to travel from the surface of the gutta percha to the iron conducting wires of the cable, an average distance of less than a quarter of an inch, for of course the iron covering of the cable has a perfect communication with the earth. Now, it is well known that such substances as tarred hemp are sufficiently good conductors to carry off this minute quantity of electricity to the iron wire with perfect facility. It is, in fact, as efficient for this purpose as water would be, for it is not necessary that the conduction should be absolutely instantaneous; the charge or discharge of a cable occupies an appreciable time, therefore the hemp may avail itself of the same period for conveying external electricity from the gutta percha to the iron cable and earth.



147. Let A G represent the whole 462 miles of cable, and the length A B, B C, C D, &c., the six conducting wires supposed to be joined end to end in one continuous length. The discharge from each of the six conducting wires was measured with a battery power of three cells and with the galvanometer formerly described; the current employed in all these experiments was positive.

Length.	Discharge.
A B. 77 miles -	40·1
B C. do. -	40·2
C D. do. -	40·1
D E. do. -	40·0
E F. do. -	40·0
F G. do. -	40·0
Average -	40·07

The whole length of cable when tested for leakage or earth gave the following results:—

Battery Power.	Deflection of Galvanometer.
1 cell - -	2·
2 cells - -	8·
3 cells - -	15·3
4 cells - -	20·2
5 cells - -	25·9
6 cells - -	30·

148. The following means were resorted to to determine the distribution of the electricity in the different sections of the cable while the current was flowing and after the tensions had assumed their permanent state:—A continuous current from three cells was sent through the cable, and a peculiar key was constructed by which any given section, such as A, B or C, D, could be struck out, as it were, from the cable; that is to say, the particular section could be instantaneously disconnected from the remainder of the cable and have both ends placed in connexion with the galvanometer, so that whatever charge happened to reside in it at the time was compelled to pass through the galvanometer to earth and register its quantity. It is unnecessary to describe the construction of this key, its nature being sufficiently obvious. The different sections of the cable were successively treated in this manner, and the subjoined table shows the several results:—

	Amount of Deflection.	Calculated Numbers in the Ratio 1, 3, 5, &c.
Length A B = 77 miles. (See fig. 147.)	36·4 36·5 36·6 36·5	
Average -	36·50	36·52
Length B C = 77 miles.	29·9 30·1 30·1 30·1	
Average -	30·05	29·88
Length C D = 77 miles.	23·0 22·9 23·0 22·8	
Average -	22·92	23·24
Length D E = 77 miles.	16·4 16·4 16·5 16·4	
Average -	16·42	16·60
Length E F = 77 miles.	9·9 9·9 9·0 10·0	
Average -	9·92	9·96
Length F G = 77 miles.	3·8 3·7 3·7 3·8	
Average -	3·75	3·75

149. If the foregoing numbers be plotted out as ordinates upon a horizontal scale, they will all be found to fall exactly in a straight line, and to form a triangle such as that represented at section 28. This regularity may be numerically traced by adding together the first and last sections, also the second and fifth, and the third and fourth. The sum in each case will be found the same. This experiment affords another confirmation of the theory that the amount of induction varies inversely in proportion to the distance from the battery, and that its amount in the several sections varies in the ratio of 1, 3, 5, 7, 9, 11, &c. For the purpose of comparison a column of calculated numbers in these ratios is appended. It is surprising how closely the electricity in each section corresponds with that given by calculation. Now, in this case the tension was reduced in the different sections by leakage as well as by the flowing out at the distant end. But since both causes follow the same law of variation (section 43), the ratio above mentioned equally prevails both in cables of perfectly and imperfectly insulating materials.

150. In the next experiment a battery power of only one cell was employed; a continuous current flowed through the whole length of cable, and the arrangements were the same as before. The sections of cable struck out were in this experiment gradually increased from 77 to six times 77, or 462 miles; and the amount of electricity in each section is shown in the following table. The letters refer to fig. 146.

	Current flowing Backwards and Forwards.	Current flowing Parallel in all Wires.
Length A B = 77 miles, or one wire.	6·7 6·8 6·9 6·9 6·9	6·7 6·8 6·9 6·9 6·9
	6·84	6·84
Length A C = 154 miles, or two wires.	12·9 12·6 12·8 12·8	12·8 12·8 12·8 12·9
	12·77	12·82

	Current flowing Backwards and Forwards.	Current flowing Parallel in all Wires.
Length A D = 231 miles, or three wires.	17·6 17·4 17·3 17·4	17·6 17·6 17·4 17·4
	17·42	17·50
Length A E = 308 miles, or four wires.	21·6 21·6 21·8 21·6	21·8 21·8 21·8 21·4
	21·65	21·70
Length A F = 385 miles, or five wires.	24·8 24·7 24·8 24·8	24·9 24·8 24·9 24·9
	24·77	24·87
Length A G = 462 miles, or six wires.	27·2 27·2 27·2 27·1	27·3 27·2 27·2 27·3
	27·17	27·25

151. It will be observed that in the above experiment means were taken to ascertain whether any change was produced in the quantity of induction by either making the current flow through all the six wires continuously in the same direction, so that all the currents were flowing parallel; or reversing the direction of the current in each alternate section, so that the current in three of the wires flowing in one direction, and that in the other three in the opposite direction. The result shows that the difference is extremely small and practically of no importance; but still a certain effect is decidedly manifest, and the existence of any difference at all is a matter of much interest, and deserving of the attention of electricians. It will be seen that in every case rather more electricity existed in the wire when all the currents traversed the cable in a parallel direction, than when the currents traversed backwards and forwards. Now, the existence of a difference in the time of the passage of the current, or in the amount of induction at the *first instant of contact* with the battery, would have been a matter occasioning no surprise, for the passage of the parallel currents might, and doubtless would have produced a temporary magnetic action on the iron wires, which would not appear when the currents neutralized each other by passing in opposite directions. Moreover, the well-known inductive effect produced by a current in one wire upon the neighbouring one would account for any initial or final effects of this character. But in the manner in which the experiment was devised these influences ought to have no effect, for at the time the lengths of cable were struck out the current was flowing uniformly, and the disconnections were so instantaneous, that there could not have been time for any such effects to come into play. The results doubtless represent with great accuracy the quantity of electricity in the cable at the instant of disconnexion, which took place after all effects of magnetisation had equalised themselves. It is just possible that the *time* occupied by the discharge was a little longer in one case than in the other, and that during that interval some minute portion of electricity escaped from the cable by leakage. (See 158.) At section 183 will be found some other experiments which bear on this point.

152. Passing on from this point, we remark that the induction from the whole length of cable A G is by no means six times as great as that from one section A B. It is true we know nothing of the actual tension of the current in the different cases, for although the battery power was one cell, it was small; and with a short length of wire its tension was probably less than with a long length. (See section 23.) But this explanation tells in the wrong direction, for on the supposition that the tension was more nearly equal to one element with the 462 miles than with the 77 miles, the induction from the greater length should have been more than six times as great as that from the lesser, which is not the case. The real explanation of this is probably found in the fact that the leakage of the cable was rather considerable (see section 158), and that, consequently, while the current was flowing out of the longer cable, there was time for a considerable portion to escape on the way; and this supposition will be confirmed if we refer to Table 148. In that experiment the sum of all the inductions of the six sections of cable was 119·66, but as this was taken with three cells, we know that with one cell the amount would have been one-third, or 39·88, which is much larger than we get from the whole length A G in the present experiment.

153. In the next experiment the same battery power and disposition of apparatus was employed, but the respective sections of the wire A, B, C, D, &c., instead of being connected in continuous lengths, were joined together side by side, so that the current flowing parallel through them divided itself equally among all the wires in circuit.

With one wire only = 77 miles.	7·1
With two parallel wires of 77 miles. (Resistance = 38·5 miles.)	13·1 13·1 13·1 13·0
	13·07
With three parallel wires of 77 miles. (Resistance = 25·6 miles.)	17·6 17·8 17·9 17·4
	17·70
With four parallel wires of 77 miles. (Resistance = 19·2 miles.)	21·9 21·9 21·7 21·9
	21·85
With five parallel wires of 77 miles. (Resistance = 15·4 mile.)	25·4 25·6 25·4 25·4
	25·45
With six parallel wires of 77 miles. (Resistance = 12·8 miles.)	28·0 28·0 28·0 28·0
	28·00

154. The first thing we observe in the above experiment is the unexpected accordance between its results and those in the preceding table. The resistance of the circuit in this last experiment was of course smaller and smaller in every successive case, and when the whole six wires were joined up parallel, the whole resistance was one-sixth of 77 miles, or 12·8 miles. In the preceding experiment, with the same length of cable, the resistance was six times 77, or 462 miles, and yet the amount of induction in the two cases is almost identical. The circumstance which occurs again, that the induction of the whole six wires is less than six times that of one, may in this case readily be explained, on the supposition that the tension of the battery was lowered by the free discharge through six cables at once. But the accordance of the results in the two experiments is not so easily understood.

155. The next table is intended to show the whole amount of induction in one, two, and three sections of the cables respectively with one cell, the cable being insulated at both ends and fully charged; it is given chiefly for the purposes of reference and comparison.

	Sections connected together in Series.	Sections connected together in a Bunch.
Full discharge of one section, or 77 miles.	13·4	13·4
	13·5	13·5
	13·6	13·6
	13·4	13·4
	13·47	13·47
Full discharge of two sections, or 154 miles.	24·9	25·2
	24·9	25·2
	24·7	25·3
	24·8	25·3
	24·82	25·25
Full discharge of three sections, or 231 miles.	33·0	35·0
	33·2	35·0
	33·0	35·0
	33·0	35·0
	33·05	35·00

156. In one column is given the amount when the cables are joined end to end in a series, in the other column the amount when joined in a bunch, that is to say, all parallel. It will be seen that the induction of one section alone was 13·47, and therefore the induction from the three ought to have been 40·41 if there had been no leakage; but owing to the imperfect insulation the discharge when the wires were joined continuously was reduced from 40° to 33°, and even where three were joined on in a bunch so as to allow free access to the galvanometer, the induction was still reduced from 40° to 35°, the galvanometer having a resistance of 325 miles.

It may be interesting to remark that the mere discharge of 77 miles disconnected from earth with one cell, namely, 13·47, is just about twice as great as the same section A B of the cable gave when it formed part of the whole length of cable in experiment 150, which indicates that the tension of the battery was reduced nearly one-half by being allowed to discharge itself through the wire.

157. The next experiment is almost a repetition of those at 153 and 155, but it presents some points of interest. The arrangements were the same as in those experiments, and the battery power one cell. The letters refer to the figure at section 147.

	Discharge.
Length A B, 77 miles. Discharge received from both ends at once.	12·0
	12·0
	11·9
	12·0
	11·97
Length F G, 77 miles.	1·1
	1·2
	1·1
	1·0
	1·10
Length A C, 154 miles.	21·2
	21·4
	21·3
	21·2
	21·27
Length C E, 154 miles.	12·4
	12·4
	12·4
	12·4
	12·40
Length E G, 154 miles.	4·7
	4·5
	4·6
	4·5
	4·57
Length A D, 231 miles.	26·4
	26·4
	26·2
	26·3
	26·32
Length D G, 231 miles.	9·2
	9·1
	9·0
	9·0
	9·07
Length D G, 231. Discharge received from end D only.	8·0
	7·9
	8·1
	8·0
	8·00
Length D G, 231 miles. Discharge received only from end G.	9·0
	9·0
	9·1
	8·9
	9·00
Length A G, 462 miles. Discharge received from end A.	22·9
	22·6
	22·6
	22·6
	22·67
Length A G, 462 miles. Discharge received from end G.	24·6
	24·6
	24·4
	24·5
	24·52
Length A G, 462 miles. Discharge received from both ends.	27·5
	27·4
	27·4
	27·5
	27·45

158. The ratios of the several sections 1, 3, 5, 7, &c. (section 144) may be abundantly traced throughout the experiment as before. In the later results it is well to note the influence of leakage as shown in the difference between the discharge obtained from A, D, G, and A, G, when these sections had their near ends and their distant ends respectively connected to the galvanometer. In the last experiments the measure is given when the charge was taken from the near end, the distant end, and it is well worthy of notice that it is greater from the latter than from the former. This interesting phenomenon will be understood by reference to Faraday's Experimental Researches, vol. i. p. 322, and vol. ii. p. 208.

159. A long series of experiments, extending over many days, was made on the effect of compelling the discharge from a given section of cable, to pass through other lengths of cable, or joining them up in the manner described in experiment 125. The results were anomalous, but were subsequently found to have been vitiated, owing to imperfect joints in the gutta percha, and were, therefore, rejected. The cause which led to their rejection was however, instructive. A series of joints had been made in the cable and the ends were led into the testing hut. Through the carelessness of the workmen these joints had been made by binding thin sheets of gutta percha on the joints, which were not united by heat. The consequence was that the rain penetrated them, and the copper on one side, acting through the wet joints and the earth to the iron exterior of the cable upon the other, formed a battery of one element, which gradually, though very slowly, caused the whole interior conducting wire of the cable to charge itself, so

that after remaining quiescent 15 or 20 minutes, the cable was found to have acquired a spontaneous charge equal to that due to the tension of a battery formed of iron and and copper. The charge, of course, accumulated very gradually; any number of small discharges could be obtained from the cable at intervals of a minute without the intervention of the battery. This result, which has rendered a long series of investigations useless, is here mentioned, because it may throw a useful light on the phenomena of spontaneous currents which are at times observed in submarine cables.

160. The following table gives the amount of discharge received in the ordinary manner from a certain section of the cable A B 77 miles in length direct, and also the same charge received successively through 1, 2, 3, 4, 5 sections of cable, inserted between the section and the galvanometer. The galvanometer was not the one usually employed, it had thick wire, the current positive, with 20 cells. The difference in these results is probably owing to leakage alone. The averages only are given.

	Average.
Discharge from 77 miles of cable direct through galvanometer.	33·5
Same discharge through 77 miles of cable	32·9
" " 154 " "	32·8
" " 231 " "	31·9
" " 308 " "	30·0
" " 385 " "	28·7

161. We now give the averages of experiments made (Feb. 9, 1860) to ascertain the amount of discharge from varying lengths of cable, the distant end being connected to earth:—

	Fine Wire Galvanometer, Resistance 125 Miles. Five Cells.	Thick Wire Galvanometer, Resistance $\frac{1}{10}$ Mile. Ten Cells.
Average of discharge from 77 miles or 1 section.	1·17	2·30
Average of discharge from 154 miles or 2 sections.	11·27	8·25
Average of discharge from 231 miles or 3 sections.	24·97	13·20
Average of discharge from 308 miles or 4 sections.	38·70	19·00
Average of discharge from 385 miles or 5 sections.	52·80	24·20
Average of discharge from 462 miles or 6 sections.	67·20	29·60

162. It should be remembered that the resistance of the galvanometer used in the first series, placed in circuit at the near end, was 125 miles, and this resistance compelled a much larger quantity of the charge to pass out at the distant end than would otherwise have been the case. To reduce this effect the experiment was repeated with a thick wire galvanometer, offering little resistance, the battery power being increased to 10 cells. The relative value of the indications of this galvanometer are not known, and the results given cannot, therefore, be numerically compared with those in the first column. Such as they are, however, they appear to increase less rapidly than the square of the length.

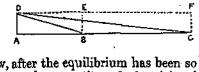
163. The idea has sometimes existed that by placing two wires A, B, in the same core, and charging one positively at the same time that the other is charged negatively, their mutual action will prevent the effects of induction; this is, however, not the case. The induction going on between A and the surrounding water would not be, in any way, affected by the presence of B, except in the particular direction occupied by B; and, on the side presented towards B, the induction, instead of being less, would be greater than anywhere else; first, because B is nearer to A than the exterior water; and, second, because the difference of tension between A and B is greater than between A and the water. The effect, therefore, as far as it goes, would be to increase instead of diminish the induction sustained by A. If two wires be employed at all under such circumstances, they should not be charged with opposite currents, but both with the same current. In this case the induction sustained between A and the earth would, as before stated, be unaffected, except on the side presented to B; but on this side, since the tension of A and B would be the same, there would be no induction, therefore, to a small extent, the induction would be less than if B were absent, but the advantage gained would be too small to have any practical value. If A could be entirely surrounded by a number of wires, B, B, B, so closely as to shield it altogether from the inductive action of the earth, and these wires were charged at every point along the line, and every successive instant to the same tension and with the same electricity as A, then we should do away altogether with induction in A. The charge of electricity would pass through A instantaneously. This may be done by surrounding A with an insulated tube of wires, and charging the tubes simultaneously with the same electricity as A. But this idea has no more practical value than the former, for we have even greater difficulty in charging the tube to the proper tension than in charging the wire, on account of its large inductive surface.

164. Neither is it of any use to charge the tube and leave it permanently charged, either with positive or negative electricity, for, whatever the charge of the tube, the changes of tension of A would be as great with respect to it as in respect to the earth; and wherever there are changes of tension, or wherever there is difference of tension, there will be equivalent changes and amount of induction. For example, if the tube be charged positively either from one end or through its whole length, the charge would cause an equivalent discharge to take place out of A exactly equal to that which A itself would have sustained with the same tension, and this charge has to be restored to A and taken out again at every signal to precisely the same extent as if B had been absent.

165. We now come to consider the laws which regulate the quantity of charge which enters a cable whose distant end is to earth when the battery is momentarily applied to to it, and the discharge from each end of such a cable when its near end is removed from the battery and connected with earth.

These are the ordinary conditions under which long submarine cables are worked. We have also to consider for the first time a new element in these investigations, viz., that of *time*. The entire commercial value of the telegraph depends on the time occupied in charging and discharging, and the rate at which signals can be distributed through the cable within a given period. The name of "retardation" was given to this phenomenon by the author when first seen, and is still the one commonly applied.

166. Before the contact of the battery with a cable the tension of the former is at its maximum, but upon contact the current enters freely into the empty cable, the tension, unless the battery be immensely large, at first falling, and again rising as the successive sections of the cable gradually become charged. The tension near the battery gradually becomes greater, until the flow of electricity out at the distant end equals the supply at the near end, and all the tensions have attained their final state. During this interval the tensions would be represented at each moment by a curved line if the battery be small, and by a straight line if it be infinitely large.



167. Now, after the equilibrium has been so obtained, we have seen that the quantity of electricity in any given section of cable as A, B, may be represented geometrically by the triangle A, B, D; also, that if the tension of the battery remain the same, the quantity in any longer cable A, C, may also be represented by the triangle A, C, D. Now, if we suppose the cable A, B, to be 100 miles long, and A, C, 200 miles, we shall see that the whole amount of electricity under induction in A, C, will be twice as great as in A, B; for the triangle A, B, D, is one half the parallelogram A, B, E, D, and the triangle A, C, D, is likewise half the parallelogram A, C, F, D; and since of these two parallelograms one is just twice as large as the other, the triangle A, C, D, will be twice as large as A, B, D. The whole electricity in the 200 miles will therefore be exactly twice as great as that in 100. It will be seen at the same time that the average distance through which the electricity has to flow before reaching its destination, and before returning to the earth, is exactly twice the average distance which the electricity in A, B, has to flow; and therefore, the longer cable A, C, not only has twice the electricity in it, but that electricity has twice as far to travel, and, conse-

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quently, the time occupied by the charge and discharge will be four times as long. Similarly, if the cable A, C, had been three times as long as A, B, the quantity of electricity under induction would have been three times as great, and the average distance it would have had to traverse would also have been three times as far, and consequently it would have taken nine times as long to become charged. In other words, the time occupied in charging and discharging a cable, with its distant end to earth, will, according to this theory, vary as the square of its length. This law was first enunciated by Professor Thomson in a paper read before the Royal Society in May 1855, and published in the Proceedings of that year, in which the question was submitted to the highest mathematical analysis.

168. It has been shown in experiment 91 that there was no increase in the speed of transmission of the electric current by increasing the tension of the battery, and we will see why this is the case. Let the induction in a cable with any given tension, say 100 elements, be represented by a triangle, as in figure 144, and let the time occupied by the passage of the electricity into the cable be represented by 1. Now, if we double the tension of the battery, the quantity of electricity contained in the cable will be twice as great as it was before; but while entering the cable each particle will be forced in under twice the tension that existed in the first case; and we have seen that the quantity of electricity flowing through a wire in a given time varies directly as the tension, consequently the double quantity contained in the cable under the double tension will occupy the same time in entering and leaving the cable as the original quantity did under the lower tension.

On Velocity of Transmission in Cables.

On velocity of transmission in cables.

169. The measurement of the time occupied in the transmission of electric currents through a submarine wire is one of the most delicate operations in electricity, for even in wires of considerable length the time occupied in the transmission of a current is extremely small. Professor Wheatstone was the first to attempt this delicate problem, and the highly scientific experiments which he made on this subject would alone have been sufficient to immortalize his name. At the time these experiments were made the effects of induction were overlooked as regarded the retardation of electric currents, and it was supposed for many years that his experiments had established the absolute velocity of electric propagation. The velocity, as observed by that philosopher, was 288,000 miles per second. This velocity is greater than any that has since been obtained by other observers. The experiments were performed on copper wires suspended in an apartment; and it is now pretty clearly ascertained that even this retardation, small as it is, is of the same nature as that experienced in submarine cables. In other words, the two ends of the wire which were joined to the positive and negative coatings of an electric battery, without any connexion with earth, took up positive and negative charges respectively, and the delay experienced in the appearance of the current at the distant end was in reality that occupied by the transmission of this charge through the length of wire under experiment. This quantity was by no means inconsiderable, for it is well known that such a length of wire as that employed, *viz.*, half a mile, would contain sufficient electricity to give a violent shock to the system. But if this wire, instead of being in a room, could have been stretched out in space away from the inductive influence of neighbouring bodies, the charge of the wire would have been proportionally less, and the velocity of propagation vastly greater, but to what extent it is impossible to say.

170. Mr. S. C. Walker made many experiments on telegraph wires in America, and he found the velocity to be about 18,000 miles per second. His method involved the use of electro-magnets, and cannot therefore be implicitly relied on, but it is very ingenious; a description of it will be found in *Nichols' Encyclopædia of Physical Science*, Art. ELECTRO-DYNAMICS. Passing over the results of other experimenters, which differ widely from each other, we come to the highly ingenious method of ascertaining the velocity of an electric current employed by Messrs. Fizeau and Gounelle, in a course of experiments which were made at Paris in November 1858, of which mention is made in the volume to which we have just referred, and which are more particularly described in the *Annales Télégraphiques* for 1858. They employed two revolving discs of ivory, fixed on the same axis, each carrying on the opposite points of the diameter metallic plates let into the ivory. Metallic springs pressed against the circumferences of these discs, and, making contact with the metal plates, formed part of a circuit. The current, in leaving the battery, passed through one of the ivory discs, and in returning passed through the other. One of the discs was slightly turned round on its axis into such a position that just as one of the springs was entering on the plate which conveyed the outgoing current, the spring of the return current was leaving its plate. It is evident that by this arrangement, as long as the wheel remained at rest in any position, the circuit was not complete, and no current could pass along the wire. The same was the case if the discs were made to revolve, so long as the current travelled instantaneously. If, however, the current occupied a sensible time in its passage, it would give time for the plate on the second disc to come round into such a position as to complete the circuit at the right moment and allow the returning current to pass through a galvanometer to earth. By adjusting the two discs into such a position as that they gave their maximum effect on the galvanometer, and observing the rate of rotation, the velocity of the electric wave was easily determined. We have merely indicated the outline of this system, but it is well worthy of attentive study, and, with certain modifications, is admirably suited for obtaining the velocity of electric currents.

171. The instrument employed in the following experiments on the velocity of electric currents was a modified form of the type-printing telegraph of Professor Hughes, and our acknowledgments are due to that gentleman both for the loan of the necessary instruments and his earnest co-operation. The whole of the following experiments were performed under his skilful superintendence, and many weeks of assiduous attention were voluntarily and gratuitously devoted by him to these and other investigations connected with the subject. A description of this apparatus will be found in the Journal of the Society of Arts, April 1859, and also in *Prescott's History of the Electric Telegraph*. The instrument contains a type wheel, upon which are engraved the letters of the alphabet. By means of a weight and chain and arrangement of wheels this type wheel is kept revolving at a uniform rate, and is not in any way checked even while printing letters. Its uniformity of rate is maintained by a governor, consisting of a ratchet wheel and escapement, which, instead of being controlled by a pendulum in the usual way, is governed by a stiff spring, which vibrates about 3,000 times in a second. It is scarcely necessary to say that the vibrations of the spring are as synchronous as those of a pendulum. On a shaft in connexion with the type wheel an arm is carried, which sweeps round on a horizontal table performing the same number of revolutions as the type wheel itself. This table carries around its circumference 28 small moveable pins corresponding to the 28 letters on the type wheel. Each of these pins is in connexion with a key and key-board like that of a piano. If any one of these keys be depressed, its corresponding pin is raised, and the arm in the course of its rotation comes into contact with it, and by a peculiar arrangement transmits the electric current along the line. The receiving part of the instrument is provided with an electro-magnet, which, as soon as it is acted upon by the current, disengages the apparatus connected with a band of paper, which it raises momentarily into contact with the type wheel, from which it receives the impression of whatever letter happens to be opposite to it at the time. The paper is not only raised into contact with the type wheel, but is for a moment borne forward at the same velocity as the wheel, and the impression, therefore, is very sharp and perfect. Now, if the type wheels of two distant instruments are adjusted, and are set revolving together at the same speed, the same letters will always be passing any given point together, and any instantaneous current acting on both magnets at once will print the same letters on both instruments. Although this at first sight seems a very difficult mechanical problem, it is accomplished with the most perfect facility and precision in the beautiful instrument of Professor Hughes.

Some minute portion of time is occupied in the movement of the electro-magnet and of the printing arrangement, but this time is perfectly constant, and is permanently compensated once for all by advancing slightly the arm of the type wheel which traverses the table. In this condition, whichever of the keys be depressed, the corresponding letter is by the action of the electro-magnet instantly imprinted upon the paper, which only moves as often as a letter is printed.

172. It is not the place here to describe the highly ingenious contrivances resorted to to obtain this precision, but an allusion to the peculiar and original form of the electro-magnet employed is necessary. It is a combination of the permanent magnet and electro-magnet. Two poles of a permanent horse-shoe magnet are prolonged by cylinders of soft iron surrounded by coils of wire, and provided with an armature of the usual kind. This armature, which lies in contact with the soft iron poles of the magnet, is held fast by the action of the permanent magnet. The attraction of the armature is counteracted by an opposing spring, which

tries to force it away from the electro-magnet, and is so adjusted that it almost overcomes the force of the magnet. In this state of things a most feeble current of electricity acting with the spring is sufficient to overcome the attraction of the magnet and the armature flies away, and by so doing brings the paper into contact with the type wheel and prints a letter. The next instant it is again mechanically forced down into contact with the electro-magnet, and is held there until released by the second current. Now in charging an ordinary electro-magnet some considerable time elapses before the iron becomes magnetised, and a still longer time before the armature acquires any sensible motion, and this length of time is variable and uncertain, depending on the quantity of electricity traversing the coil. But the action of Professor Hughes' form, which acts by demagnetising rather than by magnetising, appears to be to a great extent independent of the variation in the force of the current, and to be almost instantaneous, and its subsequent motion, when released, being governed solely by the spring, is of course uniform. It is moreover sensitive to the action of currents of the most feeble duration such as those from induced electricity.

173. The type wheel in these experiments revolved at the rate of 107½ revolutions per minute, and its circumference was divided into 28 equal divisions or letters. At this speed there would pass any given point 50 letters per second, so that each letter is equal to $\frac{1}{50}$ second of time. For the sake of convenience each letter is supposed to be divided into 20 parts, each of which would be equal to $\frac{1}{1000}$ of a second. These latter 20 divisions were estimated by the eye, according as the letter was printed full, or a little on one side. The following scale will show these divisions:—

A	= ·000	of a second.
(AB)	= ·010	,,
B	= ·020	,,
(BC)	= ·030	,,
C	= ·040	,,
(CD)	= ·050	,,
D	= ·060	,,
E	= ·080	,,
F	= ·100	,,
G	= ·120	,,
H	= ·140	,,
I	= ·160	,,
J	= ·180	,,

174. We have before said that if the line wire be extremely short when any key, as, for example, A is touched, the corresponding letter A is instantly printed on the paper. But if the line wire be a long one, and subject to induction, so that some sensible portion of time elapses before the current traverses the wire and reaches the magnet, the type wheel will have passed on through some small space. The letter printed will therefore no longer be A, but some other letter, such as B or C; and it is easy to calculate by the precedable the time which elapses during the passage of the current. In the following experiments the key depressed is invariably that which corresponds to the letter A.

175. In the first experiment a current was sent through a resistance coil equal to 460 miles of line, and the letter printed was invariably A, showing that the resistance of the coil had not any effect in diminishing the *velocity* of the passage of the current, but only that of lessening its amount.

176. The instrument was now connected through one of the wires of the submarine cable before described (section 146), having six conducting wires each 77 miles in length, the copper conductors being No. 16, or $\frac{5}{64}$ in. in diameter, and the gutta percha $\frac{3}{32}$ in. in thickness. As the six wires were twisted into a spiral rope before being enveloped in the iron, the actual length of each wire was probably about five per cent. greater than the length of the cable itself; that is to say, about 81 miles. We shall speak of it, however, as 77 miles. The near end of the cable was connected with the transmitting part of the apparatus, and the distant end was connected through the magnet to earth. The letter now printed at each operation was B, with a slight approach towards C, equal to 25 thousandths of a second. In every experiment 40 or 50 impressions were printed, and always with the most invariable uniformity.

177. The experiment was repeated, but sending the current through 154 miles of cable. The letter printed was C, with an indication of D, or 45 thousandths.

178. The instrument was connected with 231 miles of cable, or three times the original length; and the letter given was E, or 80 thousandths.

179. The instrument was connected with 308, and gave E, with an indication of F, or 115 thousandths.

180. With 385 miles, or five times the original length, the letter was H, or 140 thousandths, and the interval of time between the contact and the action of the printing apparatus became very perceptible to the ear.

181. With 462 miles of cable, or six times the original length, the letter was I, or 160 thousandths of a second.

182. These experiments were repeated many times, with slight variations of adjustment of the strength of the spring of the electro-magnet; but in no case did the variation of time differ more than 5 thousandths from the above. If the above numbers be plotted out as ordinates, they will be found to fall nearly in a straight line, and to favour the idea that the speed of propagation varies directly as the length of the cable, instead of as the square of the length. In this respect it must be acknowledged that they do not coincide with the received law of transmission; the cause of the discrepancy is not understood. The following is a table of the above observations:—

Length of Cable.	Number of Parallel Lengths.	Letter Printed.	Time in Thousandths of a Second.	Extreme of other Observations.
77 miles -	1	B	25	20
154 ,, - -	2	CD	45	
231 ,, - -	3	E	80	
308 ,, - -	4	FG	115	110
385 ,, - -	5	H	140	144
462 ,, - -	6	I	160	164

183. In the foregoing experiments the current flowed through all the six wires in the same parallel course, so that the current in all the coils of the cable flowed in a uniform direction. In order to ascertain if there were any magnetic influences in action, the connexions were reversed, so that the current in three of the wires flowed in the reverse direction to that in the remaining three. The instrument was adjusted so as to give I, with an indication of J, when the current flowed parallel, or 165 thousandths. On reversing the connexions, so as to make the current flow backwards and forwards, the letter was I J, or 170 thousandths.

184. The current was now sent through two wires simultaneously in a parallel direction, the wires being joined together at each end, so as to give a resistance of only 37½ miles. The letter printed was B C, or 30 thousandths, instead of 25.

185. With three parallel wires the letter was C, or 40 thousandths.

186. With four parallel wires the letter was CD, or 50 thousandths.

187. With five parallel wires the letter was D, or 60 thousandths.

188. With six parallel wires (giving a joint resistance of only 13 miles), the letter was D, with a trace of E, or 65 thousandths.

These last experiments, which are analogous to those which led the Atlantic Telegraph Company into the belief that small wires conducted more rapidly than large ones, show beautifully that the velocity of propagation does not depend simply on the length or resistance of a circuit, but on the relative amount of inductive surface and the supply of electricity. Had the battery been large enough to charge the whole six wires simultaneously, the velocity would undoubtedly have been the same with six wires as with one.

189. In this experiment the six parallel wires were joined up together in a common circuit, as in the preceding experiment; but an addition was made of a short circuit wire (one yard long only), connecting their ends together, and of course connecting the battery direct with the electro-magnet. The result was unexpected. The letter printed was CD, or 50 thousandths. It is evident that the electricity, instead of flowing direct through the short circuit wire and the electro-magnet coil to earth, found a readier entrance into the six conducting wires when it entered into the induced or latent state. The primary cause of this was the resistance of the coil of the electro-magnet, which occasioned a feeble tension to commence in the short circuit wire, followed instantly by an entrance into the induced state within the cable.

190. This experiment was almost identical with the preceding, but all the ends of the six wires were collected together in a bundle and jointly connected to a single thick wire. As often as this wire was placed in contact with the short circuit wire the letter printed was CD, as before, and as often as it was removed the letter was of course A. When the six wires were only connected together at one end instead of at both, the time was 45 thousandths instead of 50.

191. Two of the wires, each 77 miles in length, were joined up in a metallic circuit without an earth wire, the electro-magnet being connected between them at their distant ends, the letter was not quite B, or 15 thousandths. In this case the transmission was quicker than with a single wire 77 miles in length, but the notes taken at the time

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do not explain whether either of the wires was or was not previously in connexion with the battery.

192. Four hundred miles of resistance coil were now joined to 77 miles of cable and the electro-magnet, so as to form a circuit, the earth being used at both ends; the time was 25 thousandths; when 231 miles of cable were substituted for the 77 miles, the time was 80 thousandths. When their relative positions were changed, so that the current passed through the cable before entering the resistance coil, the time was 100 thousandths, and the current received was weaker, owing to the leakage.

193. The current was now sent through 400 miles of resistance coil alone, the letter being of course A. In this condition one end of a length of 231 miles of cable was put into connexion with the circuit at its near end, close to the battery; the time became A, with a trace of B, or 5 thousandths. When the same length of cable was connected at the distant end, near the electro-magnet, the time became 25 thousandths; when 462 miles were connected at the near end, instead of 231, the time was still 5 thousandths; and when connected at the distant end, the time was 30 thousandths nearly.

194. In this experiment the 462 miles, instead of being in a continuous length, had their ends joined all together to a common wire; and when this wire was applied to the near end, as in the preceding experiment, the time was 15 thousandths, and when to the distant end, the time was 60 thousandths. The causes of these variations, and of many others which it is not thought necessary to describe, are very obvious and instructive.

195. A circuit was made through 231 miles of the cable in a continuous circuit, as in previous experiments; the battery power was 20 cells, and the time 55 thousandths; the battery power was increased to 30 cells, and the time remained the same; with 40 cells the time was still 55 thousandths; with 50 cells the time became 60 thousandths; and with 60 cells the time became nearly 65; with 120 cells the time increased nearly to DE, or 68 thousandths. These changes are very much dependent on the quantity generated by the battery, and a single weak cell might greatly affect the results. No special examination of the battery was made in the present instance, but other experiments gave similar results. The plates were about three inches square.

196. Increasing the size of the plates slightly increased the velocity of propagation, as might have been expected. In three experiments made with one set of 40 cells, two sets of 40 cells, and three sets of 40 cells respectively, joined up quantitatively, (that is to say, all the positive poles to the wire, and all the negative poles to earth,) through 231 miles, the times were 80, 75, and 72 thousandths.

197. Although an increase in the number of cells gave no sensible increase in the velocity of propagation or charge, it gave a very greatly increased range of adjustment, which is a matter of considerable practical importance. The following table gives such results:—

Number of Cells.	Range of Adjustment of Electro-magnet given in Turns of the adjusting Screw.
10	$\frac{1}{2}$
20	1
30	2
40	$2\frac{1}{2}$
50	$2\frac{1}{2}$
60	$3\frac{1}{2}$
70	$3\frac{3}{4}$
80	4
90	$4\frac{1}{2}$
100	$5\frac{1}{2}$
110	6
120	6

198. In a similar way increasing the number of cells increases the number of *dots per minute which could be given* by a relay. Experiments were made *on this subject* with different relays, viz., Henley's, Whitehouse's Varley's, Siemens', Morse's, and the galvanometer relay. The average of all these gave through 462 miles 240 *dots per minute* with 100 cells, but only 180 *dots with 50 cells*. It would be invidious to give the precise results as the relays were not fresh from the hands of the makers.

199. An induction coil wound with No. 17 wire for the primary, and No. 30 for the secondary wire, printed IJ through the 462 miles, or 170 thousandths, nearly the same as the battery. A magneto-electric machine by Henley also gave IJ; the battery used for the induction was, however, only the ordinary Daniell's battery. The question in both cases is simply one of relative quantity and tension.

200. Observations were made upon the effect of leakage in cables. 231 miles were connected in a circuit in the usual manner; the letter given was E full, or 81 thousandths. An artificial defect of insulation was made at 77 miles from the battery, and 154 from the magnet, by connecting a resistance coil with the earth at that point equal to 231 miles of cable. The speed was increased by this to D, or 60 thousandths; when the same leakage was connected at 154 miles from the battery, and 77 from the magnet, the letter was D, with an indication of E, or 65 thousandths. An artificial defect of 200 miles at 77 miles from the battery, and another of 40 miles at 154 from the battery, gave D, or 60 thousandths.

201. When artificial leakages were introduced the range of adjustment of the magnet was greatly reduced, so that although currents and words could be sent with greater rapidity through a leaky cable than through a sound one, on the other hand a much nicer adjustment was required, and there was far more difficulty in working.

202. Experiments on the reversal of the currents were made (these must not be confounded with waves). A positive current was sent through 231 miles of cable, and the instrument caused to reverse this current. The speed appeared to be the same as when a current was sent into an empty cable, viz., E, or 80 thousandths. With reversed currents, although the speed of transmission was unaffected, the effective strength of the current and the consequent limit of range of adjustment of the magnet was doubled; thus, with single currents the extreme range of adjustment was 7 turns of the screw, with reversed currents it was 13; this is a great practical advantage.

203. A resistance coil of 231 miles was used instead of an earth connexion, but without any influence on the speed, which through 231 miles of cable was in each case 80 thousandths. A constant battery of 10 cells close to the magnet appeared to have no influence on the speed whether its current was in the same direction or in opposition to that of the transmitting battery.

204. Professor Hughes gives the following table of the approximate length of contact with the battery necessary to cause a signal to be recorded through the 462 miles of cable. The relays were of ordinary construction. The number of cells is not stated:—

——	Parts of a Minute.
Hughes' Electro-magnet finely adjusted	$\frac{1}{1000}$
Siemens' Relay	$\frac{2}{1000}$
Whitehouse's Relay	$\frac{3}{1000}$
Varley's Relay	$\frac{3}{1000}$
Henley's Relay	$\frac{4}{1000}$
Bains' Chemical Paper	$\frac{5}{1000}$
Galvanometer Relay	$\frac{5}{1000}$

205. One hundred reversals *per minute* were sent into the cable, and the strength of the current received at the distant end was estimated in turns of the adjusting screw; the battery power was 50 cells.

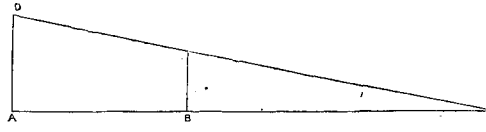
77 miles of cable gave 35 turns.
154 " " " $18\frac{1}{2}$ "
231 " " " $8\frac{1}{2}$ "
308 " " " $4\frac{1}{2}$ "
385 " " " $1\frac{3}{4}$ "
462 " " " $\frac{3}{4}$ "

206. When the number of reversals per minute was changed the strength of current increased in inverse proportion to the length of time of each reversal. Thus, with 462 miles of cable,—

2,800 reversals per minute gave ·3 turns of screw.
1,400 " " " ·55 "
900 " " " 1·00 "
700 " " " 1·50 "
600 " " " 2·00 "
500 " " " 2·50 "
400 " " " 3·00 "
300 " " " *3·50* "
200 " " " *4·00* "

On Electric Waves.

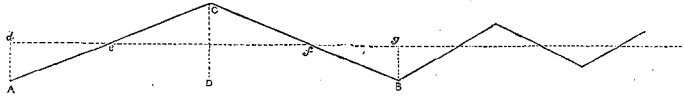
207. If two or three successive currents of short duration be sent into a long cable they will, if of sufficient magnitude, travel onwards separately, and emerge in succession from the other end; and this circumstance is usefully taken advantage of to increase the speed of working in long cables. It is at first thought difficult to understand how an imponderable fluid like electricity can exhibit the phenomena of momentum, and it is probable that they are not true waves. The following considerations will, perhaps, show how a wave of electricity may be propagated onwards without the existence of any momentum.



208. If A, C, D, represent the charge in a long cable, and the near end A be suddenly connected to earth, the quantity of electricity in section A, B, being so much greater than that in B, C, will pass out of the near end in great quantity, and after a short lapse of time the tension at A, instead of being equal to the full tension of the battery, will be 0, as it is also at the distant end C. But at some intermediate point B the tension will be still considerable, the current of course escaping at both ends of the wire. When this condition is attained this point will be the summit of the wave for the time being. Now if this wave had been originally the only charge in the cable, it would never have shifted its place, but would gradually have sunk to zero by discharging equally at both ends. Owing, however, to the great accumulation of dormant electricity in the section A, B, this wave is unable to discharge its electricity in that direction, for the wire is occupied in conducting away its own charge; but on the side of C there is no such accumulation, and the way is comparatively open, consequently the summit of the wave travels onwards towards C, falling all the while in tension. The wave cannot travel back towards A, for it meets there the nascent electricity which keeps springing into existence as fast as the tension falls on that side, and which keeps that section choked with electricity. The particles of electricity which form the wave are not, it must be remembered, in motion, but are locked up in the form of statical electricity, and are lodged exclusively on the exterior of the conducting wire; as often as the fall of *tension* at any one spot sets them free they travel onwards through the interior of the conductor, and in doing so raise the tension at some further point along the cable, and become locked up statically a second time, and so on until all tension ceases, and they emerge finally from the cable.

209. Electric waves may be of three kinds, namely, those consisting of positive electricity, of negative electricity, and of both together. In waves of positive electricity the tension of the wire rises alternately above that of the earth, and sinks to zero. In negative waves the tension falls below that of the earth and rises to zero. They are identical in their characteristics, and follow the same laws. In working with a double current, that is, with alternate negative and positive waves, there is a certain difference, which is best illustrated by a diagram.

210. Let the diagram represent waves of positive elec-

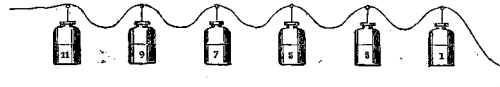


tricity in a long cable, and let the tension of the principal wave A B be equal to 10, and be represented by the perpendicular C D, A B representing the datum line or line of no tension, the quantity of electricity in the wave will be represented by A, C, B.

Let the same diagram now represent positive and negative waves, the line *d*, *e*, *f*, *g*, being the datum or neutral line; the quantity of electricity travelling forward through the same length of cable will now be *e*, C, *f*, added to *d*, A, *e*, and *f*, *g*, B, partly positive and partly negative, and this quantity may be shown to be only half as great as in the former case. But the height from the summit of the wave to its extreme depth, or, in other words, the difference of tension, is the same in both cases. We, therefore, get the same propulsive power acting on only half the quantity of electricity, and obtain a consequent gain of velocity.

211. The system of double waves or currents has also an important practical advantage over single currents. It is necessary with single currents to use a spring or other mechanical contrivance to bring back the armature or magnet to its position of rest. The force necessary to effect this has to be subtracted from the efficient force of the current; with double currents the armature or magnet moves passively under the influence of the alternate currents, the whole power of the current being effective to make and break the contact.

212. The phenomena of waves may be thus produced and imitated in the laboratory. Take a number of large Leyden jars, say a dozen, and connect them by strips of wood or lengths of fine gutta-percha tube, filled with distilled water, so as to form an equidistant series along the line; connect them with a battery of 500 cells at one end and with the



earth at the other; they will quickly assume tensions and charges represented by the number 1, 3, 5, 7, 9, &c., which may be measured by a series of small Milner's electrometers; if the battery be now suddenly reversed, the changes may be deliberately watched, the tension in all the jars falling, but the point of maximum for the time being continually travelling onwards towards the distant end. M. Guillemin, in some recent highly interesting experiments of this character, employs cotton cord for the conductor, which he finds to answer better than any other substance. See *Annales Télégraphiques*, November 1860.

On the Properties of different Insulating Materials.

Gutta Percha.

213. Gutta percha, when pure and well dried, is one of the most perfect insulators known, as was originally pointed out by Professor Faraday, and from its mechanical properties and strength it is especially adapted for telegraphic purposes. In practice it has, however, some most serious defects; it is softened by a very moderate increase of temperature, and in this state is readily injured; when exposed to the air, especially to the weather, it absorbs oxygen rapidly and becomes changed into a brittle resin, which cracks when bent, and eventually shrivels into a brittle and contracted mass. This change goes on rapidly even in dry earth, and to a slight extent it changes even in fresh water; but in salt water it seems, as far as experience has yet gone, to remain absolutely unchanged by time. Its insulation, though sufficiently good for all practical use, is yet in its ordinary condition so imperfect that minute faults are concealed by the leakage of the material itself, and are, therefore, liable to remain undiscovered. Its most serious practical defect is that after being submerged in an apparently perfect condition, and after working well for some time, minute faults become apparent, and gradually increase in magnitude more or less rapidly according to the strength of the battery power employed, and eventually render the whole cable useless. The discovery of the cause and remedy for these faults constitutes at the present time by far the most important study for the telegraphic engineer. They are probably original defects of manufacture. The mechanical operation of laying cables is a comparatively simple and easy one in any depth of water, but the preservation when laid has hitherto been surrounded with difficulty; in shallow water, where such defects can be removed, and in those few cables, when no such defects occur, the insulation remains quite unimpaired by time and use.

214. Lightning striking the line wires or the dry soil near a cable is one of the causes which has in many instances occasioned the destruction of cables. If a piece of ordinary gutta percha wire be applied to an electrical machine, every spark pierces the gutta percha with the greatest facility, leaving an invisible puncture which time and a battery current would soon enlarge into a fault. A battery of 500 cells does this in a few minutes; a battery power of 50 cells requires weeks or months to effect the same result. The best

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preventive is the use of several lightning protectors at the sea coast, and the burying of a considerable length of the telegraph cable *covered with an iron coating* inland; dirty hands and carelessly made joints constitute another source of mischief. On land the gutta-percha joint often spontaneously separates by time, especially if old wire is jointed with new gutta percha, or *vice versâ*. The best remedy is to surround the joint with sheet india rubber. At sea this evil does not occur, but a fault frequently occurs at the joint and forms a hole from the wire to the water usually large enough to insert a pin. Minute fibres or chains of vegetable matter or air holes may be conceived to originate these faults, which sometimes present themselves in the very best and soundest looking wire. The manufacture has, however, been singularly improved of late, and wire of the quality usually employed two years age would not now be admitted in a submarine cable of any length.* The use of numerous coatings of the material is now universally adopted. The excentricity of the wire is also an evil which should be avoided by every precaution.

215. Dr. Cattel purifies gutta percha by solution in benzole and subsequent precipitation; in this state it is perfectly pure and white, and its insulation is extremely perfect, although not more so than that of some of the special varieties prepared by the Gutta Percha Company, and the advantage gained is probably not sufficient to compensate for the increased cost.

216. The Gutta Percha Company submitted samples of wires covered with a special variety of gutta percha. These wires are numbered 7, 8, and 9, in the tables of insulation and induction; and it will be seen on reference that their insulation was most admirable, almost rivalling that of india-rubber; their specific inductive capacity was also fully 25 per cent. less than that of ordinary percha. Although in the tables above mentioned their composition is described as unknown, they have since been ascertained to be ordinary percha purified by mechanical treatment only, and in no way chemically prepared. Two of the wires have layers of Chatterton's compound. Twelve months since such high perfection of insulation and such low specific induction were deemed unattainable. The behaviour of gutta percha under pressure has already been described.

217. Mr. Radcliff prepares a variety of gutta percha by some chemical process not explained, which gives it great toughness and elasticity, to some extent approaching that of india rubber. The insulation is about twice as perfect as that of ordinary percha, but its specific inductive capacity and its other properties are apparently not at all changed; the wires are numbered 10 and 11 in the tables.

218. Mr. Godefroy prepares a variety of gutta percha by mixing with it about 20 per cent. of ground cocoa-nut shell and 10 per cent. of india rubber; its insulation is similar to that of ordinary percha, but its specific inductivity is somewhat greater.

219. The Gutta Percha Company use alternate layers of percha and of "Chatterton's compound," which is stated to be a mixture of percha with Stockholm tar and resin, and is laid on in very thin coatings between the layers of percha. The compound itself is an extremely perfect insulator, but is not well adapted for use in hot climates on account of its ready fusibility. To test the value of this invention, 20 miles of wire were prepared in four varieties, five separate miles of each variety. The results were very uniform for each kind, and the averages are given below. The copper wire was No. 16, and the external diameter of the percha a quarter of an inch, the battery power 502 cells, and the temperature 64°.

	Induction. (Discharge) Average of Positive and Negative.	Leakage. Average of Positive and Negative.
Plain gutta percha only, in two layers	47·0	4·73
Gutta percha, with a single coating of compound next to the copper wire	48·2	3·82
Gutta percha, with a coating of compound on the outside of all (see remark below)	43·4	3·23
Gutta percha, with a coating of compound on the wire and another coating between the two layers of percha	45·5	2·43

* The Gutta Percha Company favour me with the following instance of this:—20 miles of insulated wire of best quality manufactured in 1857, and tested with 504 cells and a horizontal galvanometer, gave in the aggregate 315 degrees. 20 miles of wire of the same size manufactured in 1861, and tested with the same apparatus and under similar conditions, gave 40 degrees. This last wire, however, had layers of compound. (See 219.)

The third wire with the compound outside was thickly coated, and its size greatly enlarged, to an extent sufficient perhaps to account for its diminished induction. The fourth wire with two coatings was so sensibly improved, both as regards insulation and induction, that it was supposed that the mere alternation of material might perhaps reduce the inductive effect. A wire was therefore prepared by the Gutta Percha Company having 10 coats of percha and 10 coats of the compound in alternate layers, and is numbered 14 in the tables; its insulation was of course most excellent, but its induction, though reduced, was not diminished to such an extent as the above results had given reason to expect; its induction was 10 per cent. less than that of plain percha. Apart from any electrical advantages, the material is probably serviceable in filling up air bubbles and crevices, and in breaking the continuity of conducting fibres, and thereby preventing incipient faults.

220. Five hundred grains of gutta percha in the form of very thin transparent sheet were exposed to the weather in a sheltered north aspect in the shade for eight months (from November till the end of June). The sheet became excessively brittle and resinous, especially at the bottom, where most freely exposed to light, and gained 19 grains in weight by the absorption of oxygen. Another sheet exposed daily to a few hours' morning sun and to rain became entirely broken into fragments and partly lost.

221. A similar sheet of gutta percha (where the quantity of material in these experiments is not stated, it is always understood to be 500 grains, and the time of exposure eight months) was fully exposed to sun and light in an inverted open bottle in the form of a loose roll. The exterior was perfectly brittle and resinous; the interior was not much affected; the increase of weight was 24½ grains.

222. A similar sheet in an inverted open bottle was placed in a dark box in the open air, a small hole at bottom admitting the air freely into the bottle, but nearly excluding light. It remained at the end of eight months perfectly tough and fresh; its increase of weight was only 2½ grains.

223. A sheet was fully exposed to the sun in a stoppered bottle. When opened it was much altered; the outside was bleached and brittle in parts; the inside of the roll not much affected; its increase of weight was 21 grains, showing the access of air to the bottle. The bottle had a pungent smell like formic acid, and the percha adhered in places to the bottle in the form of a sticky varnish.

224. A sheet in a stoppered bottle enclosed in a perforated box, as in 222, remained almost unchanged in colour, smell, or texture; it increased in weight 9½ grains; the bottle had a little moisture inside.

225. A sheet was placed in fresh water in an open bottle and exposed to diffused light in a room with a stove in it; usual temperature 60°. It had a slightly opalescent appearance when first opened, and had gained 13½ grains after drying with blotting paper; its strength was very slightly, if at all, impaired. After half an hour's exposure to air its weight was 507 grains, and its opalescence was not so apparent. After two hours, further exposure in a dry room, its weight was 500½ grains, showing a gain of half a grain only. It was returned to the water for two months longer, and then tested electrically; its insulation was about the same as that of a new sheet; its strength had, however, become very sensibly impaired, in which respect it presented a very marked difference from a sheet similarly exposed in sea-water, as described further on.

226. A sheet was placed in an open bottle of fresh water and enclosed in a perforated dark box indoors. It was scarcely changed in any of its physical characters, but had a slight tendency to opalescence. When first dried and weighed it had gained 11 grains; after half an hour only 5 grains; and after two hours more, it had apparently lost 3 grains of its original weight.

227. A sheet in a stoppered bottle of fresh water exposed to diffused daylight in a room had gained at first 13 grains, but after two hours and a half exposure to the dry air had lost half a grain; it was perfectly tough, fresh, and unchanged. A sheet similarly treated in a dark box was equally unchanged; its gain in weight was at first 8 grains.

228. A sheet was placed in sea-water in an open bottle and exposed to diffused light as above; its gain in weight, after washing and drying, was at first only 1 grain, and after two hours' exposure it weighed only 497 grains; its surface was slimy, its texture was beautifully fresh and strong. After two months' longer immersion it was tested electrically by comparison with new sheets, and its insulation was found not in the slightest degree impaired. Another sheet similarly treated in a dark box was equally unchanged in texture and quality; its weight at first was 500 grains, but after two hours' drying in the air it had lost 6 grains.

229. A sheet similarly treated to the former in a *closed* bottle had communicated to the water, and had itself acquired, a very powerful and offensive smell of sulphuretted hydrogen, but remained perfectly tough and fresh in texture; its gain at first was half a grain, and after two hours' expo-

sure its loss was 6 grains. It will be remarked that in every instance sea-water proved a perfect preservative of the material.

230. A sheet exposed to diffused light in a bottle of boiled linseed oil was found tough and strong, except where portions of the sheet projected above the surface of the oil; these had become perfectly brittle and resinous.

231. A sheet placed in plain linseed oil was perfectly tough beneath the surface of the oil, but brittle when exposed to the air. Turpentine destroys gutta percha entirely.

232. A sheet immersed in Stockholm tar was found extremely tough and fresh in texture and very strong.

233. A sheet exposed in coal tar was also found tough and strong, except the portions which projected above the surface.

234. A sheet immersed in Hughes' fluid (a variety of tar with a balsamic smell obtained from the distillation of Scotch bituminous shale or anthracite) was perfectly tough and fresh except above the surface.

235. A variety of experiments similar to the foregoing were performed on thicker sheets of percha, and similar results were obtained. One or two cases only present new features; the thickness was $\frac{1}{10}$ inch. A sheet $16\frac{1}{2}$ inches by 7, exposed to sun and wind, had lengthened to $17\frac{1}{2}$ by 7, and become very weak and rotten, especially in the longitudinal direction of the grain; it had lost 6 grains in weight, all the loss occurring in the first month. Another sheet exposed out of doors in an inverted open bottle had become rotten and brittle and gained 4 grains. A sheet under similar circumstances, protected from light by a perforated box, was nearly unchanged in texture, but had lost 12 grains in weight.

236. Sheets of similar thickness, kept in fresh water, were not altered physically, but had acquired a thick slimy coating on their surface, and after removal had still increased in weight about 12 or 14 grains, and in sea-water not more than seven grains. Of two specimens of thin sheets which were immersed, the one in boiled water and the other in ordinary water, the former was unaffected in texture, but that in ordinary water was somewhat perished and weakened; that would be probably due to the presence of oxygen or air in the unboiled water.

237. Sheets were exposed in a bottle containing potassium, and also phosphorus, to deprive the air of oxygen; in the latter case the sheet became dry and papery to the feel, and acquired a greatly increased power of stretching. Sheets exposed to coal gas and to carbonic acid were unaffected; in sulphuretted hydrogen they were most beautifully preserved, and apparently even improved in tenacity and softness of feel. The combination of percha and caoutchouc with phosphorus and selenium would probably repay investigation.

238. A rod of gutta percha, barely $1\frac{3}{8}$ inch in diameter, bore a weight of 560 lbs.; before any permanent stretching took place it elongated about 4 per cent., but regained its length as often as the weight was removed. At 700 lbs. it suddenly stretched greatly, and, at the same time, contracted in diameter to about three-fourths of an inch. In this new stretched condition it bore 912 lbs. before breaking. In other words, the ultimate breaking weight of unstretched gutta percha is, according to this experiment, 1,024 lbs. per square inch, or of stretched gutta percha 1,520 lbs. per square inch.

India Rubber Unmasticated.

239. India rubber has been applied to telegraphic use in several different forms, and from its cleanness and purity, its high non-conducting powers, and its low specific inductivity, it is excellently adapted for the purpose; it bears a temperature of 212° with impunity. At least two varieties are commonly met with in commerce; the East India gum is the cheaper variety, and is liable to become soft and sticky by exposure to air, even in a dwelling room, but is, nevertheless, stated to be well suited for making vulcanized india rubber; it is not fitted for telegraphic use. The para gum is the well-known bottle india rubber; it is much dearer in price than the other, and is much more permanent and durable in character. Both varieties insulate well; when masticated they are readily mixed together in any proportions, but they both lose much of their durability by the process, and evidently undergo a chemical change.

240. Messrs. Hall and Wells are the only parties who have supplied samples of wire covered with india rubber in its natural or unmasticated state; their wire is first covered with cotton, then with a layer of pure bottle rubber, cut into thin strips and wound on spirally, a little naphtha being used to ensure adhesion; other layers of masticated para rubber are then wound on in the same manner, and, finally, the whole is closely wound with stretched vulcanized india rubber thread, which compresses the whole tightly together. Three miles of such wire were submitted for examination; the first mile became faulty from some unexplained cause and was removed; the other two miles are numbered 18 and 19 in the tables of insulation and induction. A reference to the former table will show that both insulated extremely well at first, but one fell off greatly in this respect during the experiments, and, in fact, became faulty; the other also fell off considerably, but remained a good wire to the last. The table of induction shows a still more unaccountable change. The amount of induction in each wire increased gradually during the experiments to the extent of about 50 per cent. Their induction was, at first, much less than that of similar percha wires, but it ended by being greater. There was no other instance of a wire changing to this extent, and, unless it can be remedied, it constitutes a fatal barrier to the use of this wire in submarine cables. It is possible that the cotton wire surrounding the copper was, at first, very dry, and acted as an insulator, but gradually became moist by absorption through the pores of the india rubber, and virtually enlarged the diameter of the conductor. The defects in insulation are probably caused by the impossibility of preserving absolute cleanliness of the surface of the strip and perfect cohesion.

India rubber unmasticated.

241. Five hundred grains of the cut para bottle rubber in thin strips were freely exposed to sun and weather for 10 months; their weight had increased by oxydation to $534\frac{1}{2}$ grains. The rubber was very black in colour and smoky and dusty on the exterior, the latter probably increasing its weight; it was shrivelled and rotten, and had nearly lost its elasticity; when stretched it showed numbers of wide transverse cracks on the surface; it was not at all sticky; the interior portions retained some of their elasticity, and were not changed in colour.

242. Five hundred grains were placed in an inverted open bottle, fully exposed to the weather; its weight had increased to 514 grains, and the greater portion of it had sunk into an acid-smelling, amorphous, resinous-looking mass, which nearly closed the mouth of the bottle; it was clean and transparent and sticky when squeezed. Other portions were clean and unchanged in appearance but rotten, retaining, however, their contractile power when stretched.

243. A similar portion exposed in an open bottle of fresh water in a light room weighed, when first roughly dried, 586 grains, and even after an hour's exposure in a dry and warm room weighed 584 grains, probably indicating a considerable absorption of water; further experiments on this point would be valuable. It was perfectly strong and tough, and, instead of its natural light brown tint, it was as white as snow.

244. A portion similarly treated in sea water weighed 516 grains after washing and drying in the air; it was slightly paler than at first, but in toughness and strength was quite unchanged.

245. A portion immersed in boiled linseed oil was perfectly tough and strong, but portions which projected above the oil were rotten and partially contracted. The same results were obtained with plain linseed oil.

246. A portion immersed in Stockholm tar had shrunk spontaneously, but still retained considerable tenacity; it had become very clear and transparent in appearance. A portion in "Hughes' fluid" was similarly affected, but to a rather greater extent. Native caoutchouc does not seem to undergo any change when in contact with metallic copper.

247. India rubber is usually cut and wound on reels *in* a warm and stretched condition; when cooled it has no tendency to contract until heat be applied, when it instantly regains its original dimensions. The shrinking above-mentioned is of this character.

Native rubber in its green state, when cut fresh from a new bottle, is so good a conductor of electricity, that it cannot be termed an insulator, but after thorough drying and exposure to air it becomes an excellent non-conductor.

Masticated India Rubber.

India rubber masticated.

248. Messrs. Silver and Co. sent a great many miles of masticated india-rubber wire, prepared by their process, for examination, and a reference to the tables of insulation and induction, where two of them are numbered 16 and 17, will show the high perfection of their insulation, and at the same time the low specific inductivity of the material. Caoutchouc is very apt to attract moisture to its surface, which then conducts electricity, and it is probable that the insulation of the material is very much better than even the table of insulation would lead one to suppose. All the specimens tried were uniformly excellent. The induction of india rubber was to that of percha of similar size, as 14·7 to 22·7. It is singular that the amount of this induction did not increase at the higher temperatures.

249. Messrs. Silver and Co.'s wire is prepared by cutting the large square blocks of masticated india rubber into

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sheets about $\frac{1}{12}$ inch in thickness, and these are again cut into long narrow strips, while fresh, and wound upon the wire in a stretched state, by hand, in a lathe, in many layers. No solvent is employed, but great cleanliness is used, and the whole is consolidated by immersing the completed wire for a few minutes in water, at a temperature of about 140° or 150°. The joints in the wire are made by a similar process. The consolidation is so perfect that it is not possible to discover the point of union or to tear it again asunder at the same spot. In spite of these high qualities masticated india-rubber wire has some defects which render some caution necessary in its use.

250. One of these defects is the spontaneous decomposition of the caoutchouc. In certain specimens of the wire the rubber, where it is in contact with the interior copper wire, spontaneously changes into a black, viscid, treacly-looking fluid, which oozes out at the ends and leaves the wire to some extent loose in the centre. This fluid, when exposed to the air, dries into a dark, resinous, brittle varnish. It readily fuses again into a liquid state by warmth. The insulation of such specimens is very slightly impaired. The change goes on quickly, even while lying in a drawer, and always commences in the interior. It sometimes changes one-fourth of the whole thickness of covering. It has been stated, and it appears to be the fact, that the change never takes place in wire while it is kept under water, although other portions of the same wire above water may suffer from it greatly. The change has never been seen to occur in wire which has a coating of cotton or felt next the copper, and yet it does not appear to be a chemical effect of the copper, for the fluid, in small quantity, has been examined by Professor Miller, who does not find any copper present. The manufacturers of the wire, Messrs. Silver and Co., ascribe it to the use of inferior (probably East Indian) caoutchouc; but after this opinion was given, a sample which was supplied by the same manufacturers underwent the same change. Of the wire which was submitted for the examination of the Committee, comprising seven or eight miles, not a single specimen has presented any appearance of the kind, although in every case except one the copper was naked and without cotton. If any varnish is used on the copper it is too thin to be visible, and there is no apparent cause for the permanency of these wires above others. The use of cotton is apparently a complete preservative, but it has the disadvantage of increasing the size and consequent induction of the wire without increasing its size. It is not known whether with tinned wire it would enter into decomposition. A thin layer of native caoutchouc would probably ensure durability without causing the evil above alluded to.

251. The durability of masticated india-rubber wire is a question of so much interest that the description of a few cases in which it has been practically employed under the author's care may not be uninteresting. In 1850 or 1851 a quantity of masticated rubber wire was employed on the Lancashire and Yorkshire Railway in the Summit Tunnel, and also on the London and North-Western Railway in the Primrose Hill Tunnel. The copper wire was covered with cotton thread, varnished with shellac, and the whole wire was protected by wooden boarding. Specimens of wire examined in 1860 showed that near the ends of the tunnel it had perished and cracked in all directions, the cracks extending to a great depth, and in many cases leaving the cotton thread quite visible. The india rubber, although so rough and so deeply cracked, was perfectly firm, strong, and elastic, and not in the least sticky. Its colour was natural and its strength very little impaired, so that it could not be easily torn by the nail. It retained its natural smell when cut or handled. Specimens taken from near the centre of the tunnel, where they had been better protected from the air and light, were in a far better state of preservation, and were in fact scarcely distinguishable from new wire. The surface was still glossy, and the strength and electrical insulation unimpaired, nor was there any appearance of even a commencement of decay.

252. Specimens of similar wire were laid in grooved boarding at the Royal Victoria Bridge near Gateshead, in the commencement of 1853, and were, when removed in 1860, exactly in the same state as those above described as taken from the ends of the tunnel. Gutta-percha wires of similar size which were laid at the same time were also perished to about the same extent. Innumerable cracks penetrated transversely down to the copper wire, and they were perfectly brittle.

253. In 1852 a length of about three miles of cable was buried in the wet mud along the sea-shore between Lymington and Hurst Castle on the banks of the Solent. The cable contained four small sized india-rubber covered wires, twisted loosely together and served with tarred yarn, the whole being covered with a plaited braiding of small iron wires. Small lengths of the same cable were at the same time submerged under some of the creeks between Yarmouth and Cowes in the Isle of Wight. The insulation was very perfect. In the intervening parts of the line percha wire, buried in earthenware pipes, was employed. After a few years the maintenance of the india-rubber wire began to give trouble by repeated failures of the insulation, many of which were traced to mechanical causes. By 1860 the greater part of the cable had been removed from these causes. The percha wires buried at the same period still remain in good condition. The india rubber was generally sound and strong, and the defects were local rather than general. After it was taken up and exposed to the air the decay of the rubber went on rapidly, and the wires began to exhibit evidences of treacly decomposition. In some which was exposed to the air two years the decomposition was so complete that nothing was left visible in some places but the copper wires in a sticky condition.

254. Mr. Siemens has exhibited specimens of india-rubber wire laid by Professor Jacobi in Russia, which had been under water 13 years without undergoing any very great change. The caoutchouc was in direct contact with the copper. Wire similar to that first described (251) has been in use indoors for 10 years, and is still in good condition.

255. Some india-rubber wire was used in the Box Tunnel at an earlier period than either of those before described, and removed in 1856, owing to its defective insulation. The copper wire was first covered with cotton and shellac varnish, and then with thick strips of india rubber. They appear to have been less perfectly made than the later samples. Their condition when removed was much the same as the worst portions of that described in section 251, and five years' keeping in an office has caused no further alteration.

256. The following experience is of more recent date. A sheet of masticated india rubber, about $\frac{1}{12}$ inch thick, weighing 500 grains, was exposed to the full sun and rain in October 1859. After eight months' exposure it had sunk into a dark rotten crumpled mass; it was not sticky, but rotten; its weight could not be ascertained, owing to the pressure of dirt and dust.

257. A similar sheet was at the same time placed in an inverted open bottle and exposed to the weather. After the same lapse of time it was found to be soft, sticky, and rotten; it had lost its form, and some portions adhered strongly to the bottle; it had sunk to the bottom, and nearly closed the orifice, and had a similar acid chemical odour to the sample of percha which was described as being in a similar state; its weight was 508 grains.

258. A sheet exposed in an inverted open bottle, placed in a dark box in the open air, with small apertures for the admission of air to the bottle, had undergone no apparent change whatever; it had gained 3 grains in weight.

259. A sheet of the same weight (500 grains) was put into an open bottle of fresh water, and kept in a laboratory. After three days it was carefully dried with blotting paper, and had gained 20 grains in weight; an hour's exposure to dry air reduced the gain of weight to only $2\frac{1}{2}$ grains. After seven days' further exposure it was again dried in blotting paper, and its increase of weight was $20\frac{1}{2}$ grains; two hours' exposure to air reduced this gain to 11 grains; its electrical insulation was perfect. At the end of nine months it was again dried as before, and its weight was found to be 535 grains; it was very pale coloured, and slimy to the touch; when cut the paleness of colour was found to extend through its whole substance; it was very adhesive when pressed together even in water, as much so as warm freshly cut rubber. When squeezed or wrung violently, considerable quantities of water oozed out of its substance by the pressure; in other similar pieces it exuded spontaneously in drops while standing; it was as elastic and nearly as strong as fresh rubber. Its electrical insulating properties were quite destroyed. A piece weighing 471 grains was placed in folds of blotting paper in a press for two hours, its loss of weight was 29 grains; it was now much more adhesive than before, and difficult to handle if two pieces came into contact. Even under water it was impossible to separate them without tearing them to pieces, although their strength was nearly as great as ever; the caoutchouc had apparently got into that hydrated adhesive condition which school boys produce in india rubber by continued mastication with the teeth.

260. A piece in this condition was allowed to hang up in a dry warm room for 25 days; it soon regained entirely its original colour, strength, and elasticity, but when tested electrically its insulation was still very imperfect. Circular discs, 1 inch in diameter, placed on opposite sides of the sheet, conveyed a current with 128 cells, which was readily visible on a galvanometer; other pieces similarly treated gave the same results. The hydration of thin sheet caoutchouc goes on in a very regular manner, and is easily traced by the change in colour; a thin pale film or hydrated

portion is visible in 24 hours with distilled water; after a week the pale colour enters farther into the substance; and after a few months only a thin central layer of dark unchanged india rubber is visible. Up to this time the insulation remains perfect, but as soon as the central film becomes hydrated the insulation is destroyed.

261. A thick sheet of india rubber, half an inch thick and four inches square, was immersed in water for 10 months; the exterior was pale coloured and sticky, but the change had penetrated only to a depth which was estimated at not more than $\frac{1}{100}$ of an inch; the interior was dark and fresh, and insulated perfectly. The exterior pale surface conducted electricity freely.

262. A sheet exposed for nine months in a laboratory in an open bottle of sea-water gained 24½ grains when first weighed; it was slightly opaque and whitish in colour, but not nearly so much so as that in fresh water, very uniform in colour throughout. It was very strong and perfectly elastic, and its self-adhesion was about the same as that of new sheet; it gave no exudation of water when squeezed. It was replaced in water for 25 days, and again examined; its colour was paler than before, and liver-coloured throughout the whole substance; its strength and elasticity were remarkably perfect; it was more inclined to be flabby and adhesive. Its insulation was so extremely bad that even four cells gave a sensible indication on the galvanometer with discs of the size above described.

263. A sheet in a stoppered bottle of sea-water had gained 28 grains, and acquired a strong smell of sulphuretted hydrogen; it was in other respects similar to that last described.

264. Twelve bags of thin masticated sheet were prepared by Messrs. Silver and Co., and 400 grains of crystallised acetate of potass was placed in each, and the mouth carefully sealed up. The bags were immersed in water for nine months. Owing to a careless accident only three of these could be identified. Their original weights were 1,001, 961, and 1,000 grains respectively, and their gain in weight was 23 grains, 27 grains, and 9 grains, including, of course, any absorption of water by the india rubber. In every case the salt had entirely deliquesced, and it is certain from the above weights that either through exosmose or mechanical imperfection a portion of the solution had passed out of the bags; an accidental injury to one of the bags prevented the direct proof of this by examination of the water. The caoutchouc was stiff and hard, but readily softened by warmth. They contained a strong solution of the acetate; the thin sides of the bag were dark in colour, strong, and quite unaltered, with no adhesiveness or paleness of colour. The thick-jointed edges of the bags which were out of the influence of the salt were on the contrary pale whitish and adhesive for a considerable distance from the surface, perhaps $\frac{1}{10}$ inch, and had evidently absorbed much water. It was manifest that the saline solution had prevented any hydration, although the sheet was in contact with fresh water on one side.

265. Portions of several of these bags were tested electrically. In most of them the surface was sufficiently hydrated to conduct freely, especially on the fresh water side, and pieces which really gave perfect insulation appeared from this cause to conduct freely. It was necessary to surround the testing disk at a little distance by a ring of metal wetted and pressed tightly on the sheet. The ring was in direct connexion with the battery, and the inner disc was also in connexion with the same pole through the intervention of the galvanometer; the other pole being connected to the metal plate beneath; the intention of this arrangement is obvious. One piece only was found which conducted through its interior to an extent sufficient to be visible on a galvanometer with 500 cells; the nine other pieces appeared to insulate perfectly under every test applied; or where they did not so, it was proved to arise from surface conduction through the hydrated portion.

266. The india-rubber wires of Messrs. Silver and Co., marked No. 16 and 17 in the tables, known as B and C, were carefully examined after about nine months' immersion in water at various temperatures, from 32° to 92°. Also a wire known as Silver's A. The wire A was smaller than the others, being only $\frac{5}{32}$ inch exterior diameter. The copper wire was first covered with felt. Not the slightest spontaneous decomposition was visible. The exterior was pale coloured, but not at all sticky, even when dry and warm. When cut in section the pale colour only extended an infinitesimally small way inwards, gradually shading to the natural dark colour; the full change had not extended to more than $\frac{1}{10}$th part of the thickness of the rubber, and even the incipient change not more than $\frac{1}{50}$th, and this so slightly as to be doubtfully due to hydration. The electrical condition of the wire was perfect in all respects.

267. The wire B, or No. 17, had the copper naked and the rubber in direct contact with it. Not the slightest tendency to decomposition was apparent. The exterior was pale to nearly the same extent as in the wire A, but there was no appearance of a pale shaded semi-hydrated interior portion, as in the former case. The centering and insulation were perfect. The wire C, or No. 18, also had naked copper, and presented no appearance of any change in the rubber. The amount of hydration was exactly the same as in the wire B. Specimens which had been submitted to long continued hydraulic pressure, equal to that of nearly two miles of water, were examined, but the hydration had not extended to a sensibly greater depth than in the cases above described.

268. It had been supposed that grease might be the cause of decomposition of the india rubber when next to the copper, and to test this Messrs. Silver and Co. supplied specimens of wire purposely greased. After remaining in an office three months the presence of grease could not be detected either by smell or feel, but the wire was quite unchanged.

269. A sheet of masticated rubber placed in linseed oil was dissolved into a kind of jelly. In boiled linseed oil it was much softened and rotten, and greatly swollen in bulk, but still elastic. In Stockholm tar it was also much softened, weak, and rotten, but still elastic. In coal tar it was in a very similar condition. In Hughes' fluid it was also in a similar state. In a variety of the Hughes' fluid, stated to be especially adapted for india rubber, it remained perfectly tough and elastic, but perhaps just a little impaired in strength; the parts above the fluid were affected by the light, and had lost their strength. Para rubber in the same fluid was quite unchanged.

270. A consideration of the above experience leads to the conclusion that india rubber is in some circumstances a very durable and efficient insulator. Spontaneous decomposition is only an occasional phenomenon, and does not necessarily imply destruction; although its cause is not understood, it may be obviated with certainty. The decay or oxydation which takes place when exposed to light and air does not occur under water. The hydration is the most serious change, but it is an extremely slow one, and it is not known to what limits it might reach. It certainly goes on more slowly in salt water than in fresh. As long as it proceeds the inductivity of a cable will of course increase. The effect may probably be perfectly obviated by covering the india rubber wire with a coating of gutta percha or of vulcanized india rubber, or possibly by a vulcanization of the exterior by a momentary immersion in sulphuret of carbon and chloride of sulphur.

Vulcanized India Rubber.

Vulcanized india rubber.

271. Vulcanized or sulphurized india rubber exists in two very distinct forms, viz., the highly elastic form so well known, and in the form of a hard, black, strong, but brittle substance, capable of taking a high polish, and known under various names, such as vulcanite, ebonite, and carbonized caoutchouc. Both forms are prepared by kneading india rubber with sulphur or metallic sulphurets, in which state they form a plastic, highly insulating, and somewhat adhesive mass. In this state other substances may be mixed with them, and adulteration may be practised to almost any extent. It is afterwards exposed for several hours to a high temperature in an oven, or more usually within a high-pressure steam boiler, and it gradually assumes, first, the elastic state, and subsequently the hard or vulcanite form. During the operation it passes through every intermediate stage of elasticity and hardness, and retains it if the process be arrested. In one form it resembles soft elastic horn. If not adulterated with conducting substances, it is in all its forms a highly perfect insulator, and in the form of vulcanite it is not inferior to the pure caoutchouc. In the latter form it is fast superseding ivory and glass for electrical purposes, and even for electrical machines. It does not readily attract moisture to its surface like glass, and is hence well suited for temporary or indoor insulation. When exposed to the weather, it constantly gives off sulphur, and the surface in course of time assumes a porous or bibulous character, so that a drop of water placed on it spreads rapidly over it for half an inch or more, like it does with blotting paper. Were it not for this defect, it would be of great value for telegraphic insulation, for, unlike the other form of the material, it is highly durable and stands exposure well, and does not show any tendency to contract or lose its form or strength.

272. In the elastic form it is not at all durable when exposed to weather, although there is great difference in quality in this respect, but it appears nevertheless to be very permanent under water, as is evidenced by the long-continued use of pump valves, &c., which retain their strength and elasticity admirably.

273. A sheet of vulcanized india rubber about $\frac{1}{10}$ inch thick, weighing 500 grains, was freely exposed to the weather for nine months; its weight was reduced to 49

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grains, and it had begun to decay; it showed cracks when stretched, and smelt strongly of sulphur.

Vulcanized India-rubber.

274. A sheet of 500 grains was placed in fresh water indoors for nine months; its weight, when roughly dried, was 596 grains, and after an hour's exposure to air 582 grains. The interior was paler in colour, but it was still strong, and in other respects unchanged. Its electrical insulation remained quite as perfect as when first immersed.

275. Five hundred grains were similarly treated in an open bottle of sea-water; its weight, when first mechanically dried, was 515 grains, and after two hours' exposure to the sun 509 grains; it was perfectly strong and elastic, and the interior was dark and not pale coloured like the preceding specimen; it was slimy on the exterior.

276. A sheet placed in boiled linseed oil was soft and glutinous, and in linseed oil it was in the same state, but maintained considerable transparency and elasticity. In Hughes' fluid it was in the same condition.

277. A specimen of wire 176 yards long, covered with vulcanized india rubber in the elastic form, was submitted for examination by Mr. G. B. Daft. The conducting wire is of brass, which, by his process of manufacture, is caused to adhere perfectly to the vulcanized rubber. In practice, copper wire brazed on the surface would be preferable; its manufacture is perfect and its insulation is unexceptionably good, and, from the nature of the material, it is of course extremely difficult to injure it mechanically. Such wire seems highly suited for telegraphic use in every way; the only doubt which hangs over it seems to be whether it is practicable to make perfect joints in the material. It is confidently asserted that by joining it with new material and vulcanizing the whole in a portable bath, perfect adhesion is obtained, but the absolute proof of this and of its adaptation to practical use is yet wanting.

278. Mr. Hooper has submitted numerous short specimens of wire first covered with india rubber, then with a layer of some separating, material such as hemp or tinfoil, &c., then with the vulcanite compound, the whole being then exposed to high temperature to vulcanize the exterior coating either to the elastic state or to the hard horny condition. He has bestowed great pains in endeavouring to find a good separating material, but it is believed without entire success, the sulphur being apt to penetrate to the interior rubber and vulcanize it, thereby preventing the security of the joint and ultimately attacking the copper wire and converting its surface into sulphuret. Such cables have a very high perfection of insulation when the materials used are pure; and if the joints can be readily made and entirely relied on, seem every way suited for telegraphic purposes. Mr. Hooper further covers these cables with longitudinal iron or steel wires, and again coats them with an inferior kind of vulcanite, partly formed of old vulcanite articles re-masticated. This outer coating is very hard and strong, and protects the iron wires from rust and mechanical injury, but it is to be feared that the action of the sulphur on the iron will be quite as serious as that on the copper. It can scarcely be expected that the iron should remain insulated, and therefore the outer coatings are to be regarded as a mechanical protection only.

Wray's Compound.

Wray's compound.

279. The composition which bears this name is the invention of Mr. Leonard Wray, and is a mixture of shellac, india rubber, and powdered silica or alumina, with about one-ninth of gutta percha. The addition of the latter material improves its mechanical qualities, but impairs its insulation, as will be seen by reference to the tables, where it is numbered 20 and 21. It is, when warm, quite plastic, and may be laid on by a die in layers like percha; it may also be "jointed" in the same manner. It requires a much higher temperature to melt it than percha, and has the advantage of not being softened by any climatic temperature; it is exceedingly tough and strong, and as far as experience goes highly durable and permanent. It will be noticed that its insulating properties are exceedingly great, and when made without percha it surpasses even caoutchouc in this respect; the specimen tried retaining a charge 4½ hours before one half of it had escaped, while similar wires of gutta percha did not retain the same charge one hundredth part of the time. When to these qualities it is added that its inductivity is very little greater than that of india rubber, it will be seen that it is perhaps the best material at present known for the insulation of long submarine cables, nor has any serious drawback to its use been at present discovered. Its insulation fell off at the higher temperature, but not to an injurious extent; and its inductivity underwent a permanent increase during the continuance of the experiments, but only to a small degree. The material is sometimes laid on with a die, and at others laid on by the compression of two flat strips between grooved rollers, and it may readily be used in conjunction with either percha or caoutchouc; it undergoes no change when in contact with copper.

280. Five hundred grains of rather thin sheet were immersed in water in an open bottle for seven months; the weight had increased to 507 grains; in every other respect it appeared perfectly unchanged. It was again returned to the water, and after another month was tested electrically. Its insulation was found quite as perfect as when first immersed.

281. Five hundred grains were similarly exposed in seawater; its weight increased to 508 grains; it had undergone no other change, either physically or electrically.

282. Sheets were exposed to the weather, but were accidentally destroyed; after three months' exposure they, however, gave distinct evidences of the commencement of an oxydating change similar to that which occurs in percha, and the surface presented small cracks when it was bent.

283. Specimens of Radcliff's special material and Godefroy's material were also submitted for examination. Sufficient information concerning them is given in the tables. They are both varieties of percha, and comported themselves like it both electrically and when immersed in fresh and salt water and in oils, and when exposed to the weather. The wires are numbered 10, 11, and 12 in the tables.

284. A specimen of cable was prepared after the plan advocated by Mr. J. N. Hearder in the Phil. Mag., May 1859. A layer of fibrous material saturated with a tarry compound was interposed between the layers of percha. Its electrical insulation was of course about the same as that of gutta percha (see table). Its length was one quarter mile, and its induction was frequently compared with a length of one mile of percha cable of precisely similar dimensions at a temperature of 33° with 128 cells. The following is a specimen of the results:

Hearder's Cable.	Gutta-percha Cable.
8·8	33·2
9·0	33·1
9·0	33·2
9·0	33·1
9·0	33·4
9·0	33·4
9·0	33·2
8·9	33·2
8·9	33·0
8·9	33·2
Average 8·95	Average 33·20

By multiplying the average by 4, it will be seen that no advantage is obtained by the use of this form of cable, but rather the reverse.

285. Professor D. K. Hughes submitted a mile of cable of a very novel and peculiar construction, and one which well merits attention. The wire was first coated with a thin covering of percha in the usual way, and this was enclosed within an outer tube of percha, in a detached condition, the interstice between the two being filled with a balsamic smelling variety of tar called Hughes' fluid, stated to be obtained by the distillation of bituminous shale. The tables show that its insulation and inductivity were about the same as those of gutta percha. Its peculiar characteristics were of a mechanical nature; it might be pierced through and through with an awl and hacked and hewed with a knife without any apparent injury to its insulation. The tar would quickly exude out and cover the wounds and speedily restore the insulation; a discharge from a Leyden jar will readily destroy any other kind of wire, but powerful batteries may be repeatedly discharged through the wire with impunity, and without any apparent injury. The fluid exerts no injurious action on the percha, but rather tends to improve it: it appears to be absorbed by it to a considerable extent. There is no evidence to demonstrate the permanence of these effects. One of the most serious difficulties in the present state of submarine telegraphy is the spontaneous appearance and gradual increase of faults in the insulation after cables are submerged; whether originated by lightning or other causes is not known. This form of cable, however, has such a singular self-healing property, that it scarcely seems liable to suffer from the inexplicable defects above mentioned. The wire is very skilfully manufactured and filled at one operation by the Gutta Percha Company. Its electrical properties are not sensibly improved by increasing the quantity of fluid. In submarine cables its joints would perhaps be better made solid, so as to divide the length into separate compartments.

286. As a preliminary inquiry to the foregoing investiga-

tions, an interesting series of experiments with frictional electricity were undertaken which deserve mention. They were performed in a laboratory with the doors and windows carefully closed, heated by a gas stove which had no communication with the air of the room, and kept dried for several months by fresh supplies of chloride of calcium in shallow dishes. The quantity required was at first large, but after the walls and floor were dried the consumption was quite inconsiderable. The luxury of working in such a room is very great, as electrical measurements can be made in any state of the weather with leisure and accuracy. Wood dries and splits and becomes so non-conducting, that when standing on the floor charged jars may be touched with impunity. A kind of round towel dipped in a weak solution of chloride of calcium, and kept revolving by machinery over a copper roller heated by gas, so arranged as to dry the cloth and carry off the moisture of evaporation, would maintain any apartment in a state of perfect dryness either for electrical or other purposes.

287. One portion of the investigation was devoted to an inquiry into the degree of absorption or penetration of electricity into the substance of different insulating substances. This phenomenon, which was first observed by Faraday, is identical in its character with the well-known residual discharge in Leyden jars, and is a joint effect of conduction and induction; it is, in fact, induction within the substance instead of on the surface. It doubtless has a considerable influence on the speed of transmission in telegraphy. The following table extracted from a much larger one gives the proportion of electricity absorbed by different materials. The plates were half an inch thick, and were charged for 30 seconds with a tension of the electrometer of 1,000; they were then thoroughly discharged and allowed to remain free 15 minutes:—

——	Degrees on Torsion Electrometer.	Quantities of Electricity.
Gutta percha	10	3·2
India rubber, masticated	55	7·4
India rubber, vulcanized	50	7·1
Chatterton's compound	27	5·2
Marine glue	450	21·4
Shellac	270	16·0
Sulphur	5	2·2
Bees-wax	415	20·4
Spermaceti	400	20·0
Porcelain, glazed	78	8·8
Do. unglazed	375	19·4
Do. do., very vitreous	265	16·3

288. The electrometers employed in these experiments were a modified form of Coulomb's torsion electrometer with grass fibres, and were unexceptionally delicate and uniform in their action. The quantities of electricity are of course as the square roots of the degrees of torsion; 50 degrees of torsion corresponded to a tension of 500 cells Daniell.

289. Similar experiments were made with mile lengths of cable, with a battery of 512 cells, the discharges being taken at the end of every minute with a galvanometer. The wire was charged for one minute, then discharged and left in connexion with earth for one minute, then insulated for one minute and the first discharge taken.

——	Thickness of Insulator.	Diameter of Copper.	End of 1st Minute.	End of 2nd Minute	End of 3rd Minute	End of 4th Minute	End of 5th Minute	End of 6th Minute	End of 7th Minute	End of 8th Minute	End of 9th Minute	End of 10th Minute
Gutta percha	4/32	2/32	8	3	3	1	—	—	—	—	—	—
Do.	3/32	2/32	9	3	1·5	1	—	—	—	—	—	—
Do.	3/32	1/32	18	8	2·1	·5	—	—	—	—	—	—
Do.	2/32	1/32	22	10	2·5	2	1·5	1·5	1	1	·5	—
Silver and Co's. india rubber	4/32	1/32	4	2·5	1	·5	·5	—	—	—	—	—
Hughes' fluid	3/32	1/32	6	4	2	1	·5	—	—	—	—	—
Twenty alternate coatings	3/32	1/32	5	5	4	3	3	2·5	1·5	1·5	·1	1

The 20 alternate coating wire continued to give discharges every minute of about one degree for 30 minutes after its separation from the battery.

290. A six-wire submarine cable containing 462 miles of wire, charged for five minutes with 100 cells, and then discharged to earth, gave the following discharges at the end of each successive minute:—50 degrees, 45, 30, 25, 21, 20, 15, 4, 4. Professor Hughes found that after rest, if the cable were charged with either electricity, a current sent with the same electricity would print sooner by one letter on Hughes' type-printing instrument than in the ordinary case, or with the reverse electricity it would print one letter later; thus, G for H or F for H; but after a few reversals this effect ceased, and the charging of the cable before hand had no influence.

291. A very extensive series of measurements was made of the insulation of various resins, gums, liquids, and vitreous substances alone, and in combination, but want of space precludes me from giving the results.

292. Professor Hughes, whose assistance in these experiments I desire particularly to acknowledge, and who gave up his time to them gratuitously from a pure love of electrical science, made with me an extensive set of observations on the specific inductivity of various substances. It is not necessary to give these, but we arrived at the singular conclusion, that each dielectric substance has a different law of induction with relation to distance, forming another law of specific inductive capacity distinct from that of Faraday's. According to this view the amount of induction through plates of different thickness will in one material vary inversely as the square root of the thickness, but in another substance it will vary according to some totally different power. If this should be established, it would be of great and obvious importance in telegraphy. (See section 106.)

293. The manner in which these experiments were generally performed was essentially as follows:—A circular brass plate A, 8 inches in diameter, was fixed vertically and connected with the earth; plates of the dielectric, 16 inches in diameter, and of various thickness, were supported on edge against this plate; a second similar brass plate B, insulated on a sliding stand, was brought almost into contact with the dielectric, and while there touched by a wire leading from a Leyden jar of large capacity charged to a given tension, usually of 5 or 10 degrees. The wire being removed, the sliding plate B was removed out of the influence of the dielectric, and connected with the torsion electrometer, and its now free charge measured, any excess above its original 5 degrees being due to induction while under the influence of A. Precautions were taken to prevent the dielectric acquiring any charge of its own. The electrometer, having a certain electro-static capacity of its own, would of course reduce the measurement of B; but this objection was obviated by a continued and rapid repetition of the process, by which the electrometer soon acquired exactly the same tension as B itself, after which no repetition of the process could cause any further change. The quantities are of course as the square roots of the number of degrees of torsion.

294. The plates of dielectric varied from $\frac{1}{8}$ to $\frac{5}{8}$ of an inch in thickness, and the following is a specimen of some of the results obtained; in the case of white wax they are very anomalous. Of course the lower the numbers in the second column, the better suited is the material for diminishing induction.

Dielectric.	Standard Thickness of Dielectric	Thickness of Dielectric required to reduce the Quantity given with the Standard Thickness, to one half its Amount.
	Inch.	
Air	1/8	4/8
Gutta percha	1/8	10/8
Do. do.	2/8	8/8
India rubber (masticated)	2/8	8/8
Vulcanite (hard)	2/8	8/8
White wax	2/8	5/8
Flint glass	2/8	5/8

295. Professor Hughes remarks that, if this view be correct, experiments made by electricians on the relative inductive capacity of materials would give very different results if each used a different thickness as a standard, as observations made on half-inch plates would give very different ratios to those made on inch plates. If a dielectric possesses specific inductive capacity, as ordinarily understood, it should preserve the same ratio through all thicknesses, else the difference is owing to a specific transmittive law. The care with which the experiments were performed, and the uni-

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formity of the results obtained in numberless careful repetitions, was so unexceptionable, that it is difficult to reject the evidence of this novel law. Its importance at least demands for it a notice in this report.

General Observations.

296. A few general remarks and suggestions of the best mode of constructing submarine cables may not be out of place.

The conducting wire should obviously be of the purest copper, and should be freed from oxide of copper by the injection of hydrogen while in a state of fusion, as suggested by Dr. Matthiessen. Twisted strand is objectionable; first, because, in spite of every care, it leaves air cavities in the interior of a cable, which are very dangerous, while so intense a pressure is suddenly thrown on the exterior during immersion; and secondly, because the effective length and consequent electrical resistance are so considerably increased, and the sectional area of copper decreased, the current undoubtedly following almost entirely the course of the spiral strands. Mr. R. S. Newall, the author, and Messrs. Wilkes and Son, of Birmingham, have all made improvements on the stranded wire, which deserve consideration.

297. If any single material is to be employed alone as an insulator, Wray's compound would, perhaps, as far as experience has yet gone, appear to be the most suitable. Gutta percha alone, of the kind and quality hitherto employed, must be acknowledged to have failed so often as to destroy confidence in it, independent of its electrical inferiority and its susceptibility to injury by heat, and the latter very serious objection is applicable even to the improved varieties. India rubber will probably be eventually found trustworthy, but its liability to occasional decomposition and to absorption of water cast a slight shade of doubt over its character, which further experience will probably remove. The vulcanized india rubber cables and Hughes' fluid cable both give promise of much utility, but our practical acquaintance with them is as yet too limited for the formation of any decisive opinion. In the present state of our experience it however appears that a combination of two or even three materials offers advantages which no one material can ensure alone. A first coating of india rubber surrounded by percha forms an excellent combination. The first layer next the copper should perhaps be a thin strip of pure cut para rubber, to prevent the possibility of decomposition, and then layers of masticated rubber applied by Messrs. Silver's process. To prevent the action of the water or hydration, this should be covered with gutta percha, or one of its "special" varieties, or perhaps with Wray's compound, if that material should prove more suitable. Or the first layers might be of Wray's compound, and the outer coatings of gutta percha. By either of the foregoing plans we obtain the benefit of the high insulating properties and the low specific inductivity of caoutchouc and its compounds, and from the perfection of the insulation the most minute defect of manufacture would be readily detected. At the same time the great and insidious dangers which arise from the excentricity of the conducting wire and the injury of the core by heat would be entirely avoided. It is scarcely necessary to say that the way in which cables were formerly tested during manufacture would now be scarcely considered worthy of the name.

298. Due advantage would of course be taken of our improved knowledge of the laws of induction in relation to the thickness of material. By doubling the diameter of the copper wire, we only increase the induction by about one-half ($1 \times \sqrt{2}$), while we increase its power of conducting electricity four times. On the other hand, if we double the thickness of the insulator, we only diminish the induction about one third ($1 \div \sqrt{2}$), but do not increase its conducting power at all. Provided then sufficient thickness of the insulating material is employed to ensure its electrical security, every advantage is to be obtained by increasing the weight of the conducting wire, rather than that of the outer coating.

299. In a pecuniary point of view the gain is very great. Let it be supposed, for the sake of example, that it were desirable to double the rate of working of the Gibraltar or Rangoon cable. This consists of 400 lbs. of copper and 400 lbs. of gutta percha per nautical mile; its diameter is half an inch, and its value may be estimated as 400 lbs. of copper at 1*s.* 6*d.* per lb., and 400 lbs. of percha at 3*s.* 6*d.*, or 100*l.* per mile; its *actual* cost was 98*l.* If we confine our operation to the gutta percha alone, increasing its thickness to such an extent as to diminish the induction one-half, we should have to make its thickness about 1½ inch, and to add about 3,500 lbs. of additional percha, at a cost of about 612*l.* per mile. If, on the other hand, we enlarge the copper conductor, merely increasing the percha to a sufficient extent to maintain the same thickness of covering, we should require the addition of about 670 lbs. of copper, at 1*s.* 6*d.*, and 455 lbs. of percha, at 3*s.* 6*d.*, making the total additional cost only 130*l.* per mile. But we gain a collateral advantage, in this latter case, of the highest importance. Not only would the current or wave make its appearance at the distant end in the same time as in the former, but when it did arrive it would have more than double its volume or quantity (section 16), and consequently more than twice its strength; or it would permit us to reduce the battery power more than half, and still obtain a current of the same strength and in the same time (section 168); and it is of course a point of the highest importance to the permanence of a cable to work with as low a battery power as possible.

300. The nature of the best external protection of the cable is a mechanical question upon which much has been written, and every engineer will draw his own conclusions. Experience abundantly teaches that even iron cables may be safely laid in any depths, provided the operation goes on continuously, and that there is no stoppage or check in the operation; on the other hand, both theory and practice show that at a depth of 2,000 fathoms no iron cable can be safely trusted even to bear its own weight. At the other extreme we have the hempen cables, which may undoubtedly be laid with safety in the deepest soundings, as is proved by the facility with which sounding lines are raised and lowered; but from their levity there must be a certain danger of their lying across ridges of rock and slowly chafing. The best of these are the longitudinal ones covered with an outer lapping, such as Rogers' and De Bergue's, but their contraction, when wetted, necessitates precaution in their use. Between these extremes we have a useful form of cable consisting of iron or steel surrounded by hemp, forming strands of great strength, in which the iron is to some extent preserved from oxydation, an evil which quite precludes the use of iron cables alone, unless of very great weight. Steel is, of course, an admirable material for deep-sea cables, and when protected from oxydation leaves nothing to be desired. The recent Algerian cable has proved to be of great strength, but liable to kink. This evil might be greatly reduced by lapping it tightly and closely with twine or tape, according to the plan suggested by Captain Galton, R.E.; there are also other means by which it may be *entirely* obviated.

301. A perhaps unnecessary prejudice exists against the use of longitudinal steel or iron wires. It is true that when short samples are violently bent, mechanical displacement of the wires takes place, and this is an undoubted evil; but this does not occur to an objectionable extent in large curves, such as prevail in practice, and further experience on this form of cable is very desirable. It has, among others the obvious merits of having no tendency to kink, and of possessing the minimum degree of extensibility under tension. The author has designed a deep-sea cable of very numerous small steel wires laid on longitudinally (or in a very long spiral), and tightly wrapped by a coarse hempen tape saturated with marine glue, or with gutta percha, to prevent oxydation of the steel. This makes a very strong, non-elastic, and durable form of cable, and one well adapted for submergence in deep water.

302. For shallow waters, thick iron cables are everywhere used, but they suffer greatly from oxydation. When they get buried in mud or sand (especially if galvanized), this action is found to be entirely arrested. The author has endeavoured to imitate this condition by encasing iron cables in a thick coating of some cheap fibrous material, saturated with a solid mass of pitch or asphalt, and laid on hot under proper precautions. This form of protection merits attention, but no very lengthened experience of its use has yet been obtained. There is not that risk or difficulty in its practical application and use which might have been supposed. A cable so covered was submerged between the Isle of Man and the main land in 1859, and from the known durability of asphalt there is great reason for hoping that it will remain unaffected by its submersion for perhaps 30 or 40 years.

303. The operation of laying submarine cables is too long a subject to be allnded to here, further than to remark that there are many suggestions which have been made which might be adopted with great advantage.

304. Among all the improvements in the speed of working through long submarine cables, there is none greater perhaps than that to be derived from the use of Hughes' type-printing telegraph. By the ordinary Morse system of printing, each letter is composed of three or four distinct lines or dots, requiring as many complete electrical waves or reversals, with a sufficient duration of time on some of them to distinguish between dots and dashes, and between the spaces which divide portions of letters and those which separate whole letters or words. By Professor Hughes' system each complete letter is formed by a single wave, or even by a single reversal or half wave, and the spaces are formed by the mechanical action of the machine, and occupy no time. It might be expected from the nature of the machine that its operation would be far more rapid than that of instruments working on the Morse system, and the

expectation is fully confirmed in practice. The instrument has been seen to work with ease and accuracy at the rate of 16 words per minute through a cable which would only permit of six words per minute being sent by the ordinary apparatus, and through the same cable it has been seen to work whole days without *forming a single error or false letter*. The instrument probably requires the service of a skilful operator, but the commercial benefit of such an increase of speed outvalues all such considerations.

305. There are thus a great many ways in which the speed of transmission through long cables may be increased, and as these may all be used in combination, it may be safely asserted that a cable of the same length as the Atlantic cable designed at the present day would convey messages at five or six times the speed of that cable, and at a very moderately increased cost, while its electrical perfection would be incomparably greater. The value of this gain is, both in its social and commercial aspects, immense, and the acquisition of this power would alone amply repay the members of this Committee for their labours.

LATIMER CLARK.

35, Adelaide Road, N.W., January 1861.

App. No. 2. Report by Mr. L. Clark.

App. No. 3.

APPENDIX No. 3.

Investigation relating to the causes of the different electric conducting powers of commercial copper, by A. Matthiessen, Ph. D.

REPORT of an INVESTIGATION relating to the CAUSES of the different ELECTRIC CONDUCTING POWERS of COMMERCIAL COPPER, by A. MATTHIESSEN, Ph. D.

1, Torrington Street, Russell Square, W.C.,
April 18, 1860.

SIR,

In the following communication I shall record the results which I have obtained in carrying out the above inquiry, which I undertook at the request of your committee.

As the impurities contained in commercial copper are generally so small that their quantitative analysis (especially of the suboxyde of copper) would give no reliable results I have thought it better

I. To study the effect of metals or metalloids on the electric conducting power of pure copper, and then

II. To determine the electric conducting powers of the different commercial coppers and analyse them qualitatively so as to be able to assign the causes of the difference in their resistances.

Effect of metals or metalloids on the electric conducting power of pure copper.

I. On the effect of metals or metalloids on the electric conducting power of pure copper.

This part of the inquiry has been carried out in conjunction with Dr. M. Holzmann, and we prepared the pure copper:

1. By precipitating with sulphuretted hydrogen the purest commercial sulphate of copper dissolved in water acidulated with sulphuric acid—dissolving the washed sulphide in nitric acid—precipitating at a boiling temperature by carbonate of soda in excess, and finally reducing the oxide of copper by pure hydrogen.

2. By precipitating galvanoplastically sulphate of copper by a very weak current.

The commercial galvanoplastic copper was also tested, and found to have the same conducting power as that which we prepared.

The following are the results obtained with pure copper compared with an annealed copper wire drawn from galvanoplastic copper (not fused)=100 at 15°, 5C.=60° F. of, which is the standard taken for all the values given in this communication.

	Mean of Values found.	Temp. Cent.
	Mean.	°
I. Copper purified by the above method.	96·67	18·6
II. Copper galvanoplastic not fused	97·17	20·2
III. Copper galvanoplastic not fused (commercial).	96·69	18·4
IV. Copper No. III. fused in a porcelain tube in a current of hydrogen.	96·42	19·3
V. Copper No. III. fused as will presently be described.	96·66	17·5

The mean of the above determinations gives 96·72 at 19° C. for the conducting power of pure copper.

All the above wires were hard drawn, and the values given are means of two or three determinations.

The difference between hard drawn and annealed copper wire is shown by the following experiments:

Copper No. 2 was drawn hard, determined, and then annealed in a current of hydrogen.

	Conducting Power found.	Temp. Cent.
		°
1. Hard drawn	99·08	11·0
2. Annealed	101·69	11·0
Another wire gave for—		
1. Hard drawn	99·50	11·0
2. Annealed	101·89	11·0

Showing a difference in favour of the annealed wires of about 2½ per cent.

A.—*Effect of Oxygen on the conducting power of Copper.*

Copper readily absorbs oxygen from the air when in a melted state, and it is supposed to be present as suboxide, which it retains very obstinately, and in fact, hydrogen may be led over fused copper for hours in a porcelain tube without completely reducing the whole of it; it is also very difficult to prevent the absorption of oxygen during casting, &c.

Effect of oxygen on the conducting power of copper.

In order to prevent all these sources of error in making the alloys of copper we thought we might obviate them by the following simple arrangement:

In the furnace door communicating with a closed muffle are two holes; through the upper one passes a glass tube connected with a carbonic acid gas apparatus; through the lower one a clay tobacco pipe, to the stem of which a bottle evolving hydrogen is joined. The hydrogen is washed by potash, nitrate of silver, and strong sulphuric acid, the carbonic acid gas by carbonate of potash and concentrated sulphuric acid. The metal is placed in the bowl of the pipe (this being then pushed to the back of the muffle) and so fused in a current of hydrogen; when melted the hydrogen bubbles up through the liquid metal, thereby offering a new surface to the reducing effect of the hydrogen, as well as when making the alloys, causing a complete mixture. After the hydrogen had passed through a certain time, the india-rubber tube was disconnected from the sulphuric acid bottle, and the melted metal carefully sucked into the pipe stem, forming a wire which can easily be drawn finer. The carbonic acid was used to help drive out the air of the muffle, and in some experiments which will be described lower down.

In order to test this method Copper No. 3 was kept fused in the pipe for half-an-hour, when the conducting power was found

$$\overset{\text{mean}}{96{\cdot}66} \text{ at } 17°{\cdot}5\,\text{T.};$$

the wires were hard drawn.

By this method we have been able to reduce all the suboxyde in the copper, and have made all the alloys.

The amount of suboxyde present was not determined quantitatively, as no method known will give accurate results.*

The following values are those found for the conducting powers of chemically prepared copper (method I. p. 1,) fused with borax and chloride of sodium (the flux not covering the surface of melted copper). This specimen conducted

$$\overset{\text{mean}}{72{\cdot}11} \text{ at } 23°{\cdot}9\,\text{T.}$$

It was then fused for several hours in a current of hydrogen in a porcelain tube and conducted

$$\overset{\text{mean}}{89{\cdot}76} \text{ at } 18°{\cdot}9\,\text{T.}$$

This was now fused in the tobacco pipe in a current of hydrogen first half an hour, and then for three hours, which process caused the conducting power to increase to the following values:—

	Conducting Power found.	Temp. Cent.
	Mean.	°
After half an hour	93·14	17·4
After 3 hours	96·67	18·6

* Dick.—Phil. Mag., June 1856.

App. No. 3. Investigation into the conducting power of commercial copper, by A. Matthiessen, Ph. D.

Similar results were obtained with galvanoplastic copper fused in contact with air under a small quantity of borax and chloride of sodium.

	Conducting Power found.	Temp. Cent.
	Mean.	°
1. Fused in contact with air -	76·24	19·5
2. Fused in tobacco pipe half an hour.*	78·72	17·7
3. No. I. fused 1 hour in pipe -	85·97	15·9
4. No. I. fused 1½ hours in pipe -	94·26	19·7
5. No. I. fused 3 hours in pipe -	96·00	18·3

All wires were hard-drawn.

B.—*Effect of Carbon.*

Effect of carbon.

Galvanoplastic copper was fused with lamp-black. The analysis of the specimen gave 0·05 of carbon, and for the conducting power—

Mean.
77·87 at 18·3 was found.

The wire used was hard drawn.

C.—*Effect of Phosphorus.*

Effect of phosphorus.

Phosphorus alters the properties of copper to a great extent; it becomes very much harder, and its tenacity is greatly impaired.

Phosphorus was thrown on melted copper in a tobacco pipe and re-fused. The amount of phosphorus was determined as phosphate of magnesia.

	Conducting Power found.	Temp. Cent.
	Mean.	°
1. Copper with 2·5 per cent. phosphorus.	7·52	17·5
2. Copper with 0·95 per cent. phosphorus.	24·16	22·1
3. Copper with 0·13 per cent. phosphorus.	70·34	20·0

All hard-drawn wires.

D.—*Effect of Sulphur.*

Effect of sulphur.

Sulphide of copper does appear to dissolve in copper, but only to mix with it mechanically. It makes the copper very brittle, and although a vein which contained, according to the analysis, 0·18 per cent. sulphur, the values obtained for the conducting power did not agree at all with each other.

The mean of four determinations gave—

92·08 at 19°·4.

E.—*Effect of Arsenic.*

Effect of arsenic.

When arsenic is thrown on melted copper, part of it is absorbed whilst a part volatilizes, and if a larger quantity of arsenic has been used the alloy has a dingy grey colour, and is very hard and brittle. The arsenic was determined as arseniate of magnesia.

The following determinations were made with hard drawn wires, and they show that the effect of arsenic on copper is very great, although it does not reduce the conducting power so much as phosphorus.

	Conducting Power found.	Temp. Cent.
	Mean.	°
1. Copper with 5·40 per cent. arsenic.	6·42	16·8
2. Copper with 2·80 per cent. arsenic.	13·66	19·3
3. Copper with traces of arsenic -	60·08	19·7

Effect of the Metals.

Effect of metals.

The electric conducting power of copper is not so much impaired by the presence of small quantities of foreign metals, as by that of the metalloids; it is, however, particularly by iron and tin, very considerably diminished. The union of the copper with the other metals was effected in the manner above described, which offers here the additional advantage, that by the constant movement caused by the hydrogen in the melted metals the most intimate combination is effected. The amount of the metals thus alloyed, with the copper was determined by analysis. All the wires experimented with were hard-drawn.

	Conducting Power found.	Temp. Cent.
	Mean.	°
1. Copper alloyed with 3·20 per cent. zinc.	59·23	16·3
2. Copper with 1·60 per cent. zinc	79·37	15·8
3. Copper with traces of zinc. -	88·41	19·0
4. Copper with 1·06 per cent. iron	28·01	13·1
5. Copper with 0·43 per cent. iron	35·92	11·2
6. Copper with 4·90 per cent. tin	20·24	14·4
7. Copper with 2·52 per cent. tin	33·93	17·1
8. Copper with 1·33 per cent. tin	50·44	16·8
9. Copper with 2·45 per cent. silver.	82·52	19·7
10. Copper with 1·22 per cent. silver.	90·34	20·7
11. Copper with 3·50 per cent. gold	67·94	18·1
12. Copper with about 10 per cent. aluminium.†	12·68	14·0

Copper, even when alloyed with small quantities of lead (only 0·25 per cent.), is so rotten that it was impossible to draw it. In Gmelin's Chemistry it is stated that copper alloyed with only 0·1 per cent. of lead cannot be drawn into fine wire. Now the copper smelters add a small quantity of lead to their copper to soften and render it more tough. The addition of lead is supposed to reduce the sub-oxide of copper. A few experiments were made in this direction; to copper fused in contact with air, 0·1 per cent. of lead or tin was added and fused in a tobacco pipe, in a current of carbonic acid gas.

	Conducting Power found.	Temp. Cent.
	Mean.	°
1. The copper employed conducted	87·25	13·3
2. With addition of 0·1 per cent. tin.	93·45	14·0
3. The same repeated - -	94·55	13·9
4. With addition of 0·1 per cent. lead.	93·02	12·9

The quantity of lead and tin remaining with the copper was so small that it could not be determined quantitatively. The experiments, however, tend to prove that by the addition of a small amount of lead, &c., to copper containing sub-oxide a relative purer metal is obtained.

From the foregoing experiments it appears that there is no alloy of copper which conducts electricity better than the pure metal; and I will now proceed to the second part, viz., to the determination of the conducting powers of commercial copper and the analysis of the same.

I am indebted to the kindness of Latimer Clark, Esq., for the following specimens of commercial copper:—

	Conducting Power found.	Temp. Cent.
	Mean.	°
1. Spanish (Rio Tinto) - -	14·24	14·8
2. Russian (Demidorff) - -	50·34	12·7
3. Tough copper - -	71·03	17·3
4. Bright copper wire - -	72·22	15·7
5. Best selected - - -	81·35	14·2
6. Australian (Burra Burra) -	88·86	14·0
7. American (Lake Superior) -	92·57	15·0
Pure galvanoplastic copper not fused	100·00	15·5

All the above wires were annealed.

C. W. Siemen's, Esq., has kindly sent me several specimens of the "Gibraltar core," and from the results obtained it appears that the core of some of the specimens is made out of different sorts of copper.

	Conducting Power found.	Temp. Cent.
		°
Taking pure copper - -	100·00	15·5
	Mean.	
No. 112 was found to conduct -	90·7	15·5
No. 91 - - - -	89·5	15·5
Another wire of the same coil -	79·0	15·5
No. 292 - - - -	78·2	15·5
Another wire of the same coil -	67·4	15·5
N. 240 - - - -	74·4	15·5

All the wires were annealed.

* Of course hydrogen passing through the whole time.

† This alloy, in form of wire, was kindly lent by Professor Percy.

Now, as to the causes of the differences in the conducting powers.

In the Spanish copper 2 per cent. arsenic, besides traces of lead, iron, nitkel, sub-oxide of copper, &c., were found. The low conducting power is to be attributed to the arsenic present.

2. Russian (Demidorff) copper contains traces of arsenic, iron, nickel, suboxide of copper, &c., and here again the arsenic present may be considered the chief reason of its low conducting power.

3. Tough copper. In this specimen traces of lead, iron, nickel, antimony, suboxide of copper, &c. were found.

4. "Bright copper wire" contains traces of lead, iron, nickel, suboxide of copper, &c.

5. Best selected, according to analysis traces of iron, nickel, antimony, suboxide of copper, &c. were present in this copper.

6. Australian (Burra Burra). In this specimen traces of iron and suboxide of copper were only found.

7. American (Lake Superior). Traces of iron, silver (0·03 per cent.) and suboxide of copper were present in this specimen.

In Nos. 112 and 91 of the "Gibraltar core" were found traces of lead, suboxide of copper, iron and antimony; in Nos. 292 and 240 traces of lead, arsenic (very small), iron, nickel, antimony and suboxide of copper.

The causes of the differences in the conducting powers of the above coppers must be looked for in the different amounts of impurities present, but as we know of no accurate method for the determination of the suboxide of copper quantitatively, and as it was found in all the coppers tested, it was thought completely useless to determine quantitatively the other impurities, for as proved (page 2) the decrement in the conducting caused by suboxide of copper is in some cases equal to 28 per cent.

Some experiments were made with a view to find a method of improving commercial copper, but as my means and appliances only allowed me to try them on a very small scale, they have led to no practical results.

In conclusion, I would repeat the remark made at the end of the first part of this inquiry, viz., that there is no substance which can be added to pure copper to increase its conducting power, and that it is therefore of the utmost importance that the submarine cable manufacturers should use the purest copper and the best and surest method in order to bring this about would be in future only to contract for such cables at so much per knot of a certain resistance; it would then be to the manufacturer's own interest to use the best copper; for should he employ an inferior sort he would have to make the wire so much thicker, and therefore a much greater weight of copper would have to be employed.

App. No. 3. Investigation into the conducting power of commercial copper, by A. Matthiessen, Ph.D.

I have the honor to be, &c.

A. MATTHIESSEN.

To Captain Douglas Galton,
Chairman of the Government Submarine
Cable Committee, Whitehall.

1, Torrington Street, Russell Square, W.C.,
July 16, 1860.

SIR,

I HAVE *determined the conducting power of the* specimen of copper, cut from 1½ tons, sent me by Mr. Tennant, and found it annealed = 98·78 at 15°5, pure annealed copper=100 at 15°·5. It contains traces of silver (0·03 per cent.) and iron, *but no suboxyde of copper*. With regard to the standard of resistance I might be allowed to remark, that copper is not a good metal to use as such; for, first, it oxydises easily; and second, the conducting power varies so much with the temperature. Now, I have no doubt that there are alloys which do not suffer any change by exposure to air, and of which the conducting power *will not vary* at all *with* the temperature.

If during my researches on the subject I should find such, I will immediately inform you of the fact, and remain

Your obedient servant,

A. MATTHIESSEN.

Capt. D. Galton.

APPENDIX No. 4.

App. No. 4.

REPORT upon the RESULTS of CHEMICAL INVESTIGATION into the CAUSES of the DECAY of GUTTA-PERCHA used for the INSULATION of WIRES for conveying ELECTRIC CURRENTS, by WILLIAM ALLEN MILLER, M.D., F.R.S.

Professor Miller on decay of gutta-percha and india rubber.

King's College, London, Aug. 28, 1860.

THE inquiries to which this investigation has given rise have extended over many months, and have included a large number of analyses, but the results obtained may be stated in a small compass, as they are very definite. I have examined numerous samples of gutta-percha cables both injured and sound, which have been in use for several years, and I find in all cases that the deteriorated portions have undergone chemical change, and that change consists in a process of *oxidation*.

Whatever retards or prevents this oxidation, retards or prevents the decay of the gutta-percha; some of the specimens which I examined being as good as new, though they had been manufactured and used electrically for years; whilst others in a few months had become brittle, rotten, and unserviceable. As the general result of these inquiries, I find that whenever the gutta-percha has been completely submerged in water no injurious change has occurred, sea water appearing to be eminently adapted to the preservation of the gutta-percha. On the other hand alternate exposure to moisture and dryness, particularly if at the same time the sun's light has access, is rapidly destructive of the gutta-percha, rendering it brittle, friable, and resinous in aspect, and in chemical properties. A gradual absorption of oxygen takes place, and the gutta-percha slowly increases in weight, becoming at the same time proportionately soluble in alcohol, and in dilute solutions of the alkalies. In every instance, however, some portion of the gutta remained unchanged in composition.

My experiments have also been extended to the prolonged action of air, moisture, and light, upon india-rubber, and here also I find that these agents effect analogous changes, though somewhat less rapidly.

The caoutchouc, however, instead of becoming brittle, is converted into a glutinous mass, losing its elasticity, increasing in weight to a certain extent, and becoming partially soluble in alcohol and diluted alkaline liquids.

These deductions are made from the examination of a number of samples supplied to me partly by Captain Galton and Mr. L. Clark, including specimens of coated telegraphic wires suspended in air, specimens of submarine cables, specimens of wires sunk in the soil under various conditions, besides experiments instituted by myself upon the action of various agents upon gutta-percha, and they include the results of an extended and well-contrived series of experiments made at the works of the Electric Telegraph Company under the direction of Mr. L. Clark.

I will here subjoin an abstract of the principal experimental details, and for the convenience of reference will arrange them under the following distinct heads:—

1st. Experiments upon pure gutta-percha.
2nd. Experiments upon commercial gutta-percha.
3rd. Experiments upon submerged cables.
4th. Experiments upon decayed and damaged cables exposed in air and underground under various circumstances.
5th. Experiments upon caoutchouc.
6th. Experiments upon other compounds.

1. *Experiments on Pure Gutta.*

Experiments on pure gutta-percha.

Pure gutta differs in some of its properties from the commercial gutta. I found on examining the whitest samples purified by Dr. Cattell, that it formed a porous, milk-white mass, wholly soluble in benzol, in ether, in bisulphide of carbon, and in the ordinary solvents of gutta-percha. It is a perfectly pure hydrocarbon probably containing C_{40} H_{80}. I found it to consist of—

	found		C_{40} H_{80}
Carbon	88·96	or	88·88
Hydrogen	11·04		11·12
	100·00		100·00

1st. No. 2. Professor Miller on assay of gutta-percha and india rubber.

When exposed to a temperature of 212° it softens, but does not liquefy; it loses a trace of moisture, and then gradually absorbs oxygen, becoming brown, brittle, and resinous in appearance. In one specimen the increase in weight amounted to 4·45 per cent. The oxidized portion is insoluble in benzol, which when digested on the brown mass dissolves out a quantity of unaltered gutta, which had been protected from oxidation by the coating of resin.

This resinous mass when thus purified was found to have been produced from the gutta-percha by simple absorption of oxygen, the gutta having in one experiment absorbed more than a fourth of its weight of oxygen from the atmosphere.

2. Chemical Experiments on Commercial Gutta-percha.

Chemical experiments on gutta-percha.

The gutta-percha of commerce is not a pure proximate vegetable principle, but it consists chiefly of a hydrocarbon, which may for brevity be termed *pure gutta*, or simply gutta, mixed with a product of its oxidation which is in the form of a soft resin, amounting to about 15 per cent. in the best commercial samples.

The following is the composition of a piece of ordinary good commercial gutta-percha, taken from a piece of new cable supplied to me by Mr. L. Clark:—

Pure gutta	79·70
Soft resin	15·10
Vegetable fibre	2·18
Moisture	2·50
Ash	0·52
	100·00

The moisture reported in this analysis is mechanically diffused through the mass of the gutta percha, and seems to have some influence upon its pliability and toughness. 100 parts of the commercial sample when dried at 212° till it ceased to lose weight, deducting the ashes, contained—

Carbon	84·66
Hydrogen	11·15
Oxygen	4·19
	100·00

This gutta-percha softens and liquifies by a heat of 212°. It is soluble, with the exception of a few flocculi of fibrous matter, in benzol, in bisulphide of carbon, and in ether. Alcohol dissolves none of the pure gutta, but extracts a portion of the soft resin. This resin is an oxidized compound, probably in a transitional condition to a higher stage of oxidation. I found it to consist of—

Carbon	76·15
Hydrogen	11·16
Oxygen	12·09
	100·00

The true gutta was extracted nearly pure from this sample by dissolving it in benzol, filtering and adding alcohol, when a coagulum of pure gutta was obtained, which was found to consist of—

Carbon	87·22
Hydrogen	12·04
Oxygen	0·74
	100·00

The presence of the small quantity of oxygen in this case was due to a little of the resin which still adhered to the precipitated mass of gutta.

Commercial gutta-percha may be preserved for months, and even years, with little change, either in water or in air, provided light be excluded. This I have found from my own experiments, and the results which I have myself obtained are confirmed by experiments made by Mr. Clark. The following are some of the most important of these experiments:—500 grs. of thin sheet gutta were exposed under various conditions at the end of last October at the Electric Cable Works. The various samples were examined on the 2nd of July of this present year.

1. In netting exposed to sun and rain in open air.
2. In a bottle open to the air and light, but excluded from rain.
3. In a bottle open to the air, but excluded from light.
4. In fresh water, open to air and light.
5. In fresh water, open to air excluded from light.
6. In fresh water, excluded from air and light.
7. In sea water, exposed to air and light.
8. In sea water, excluded from light but exposed to air.
9. In sea water, excluding both light and air.

The specimens 4, 5, 6, 7, 8, and 9 were wholly unaltered, with the exception of a slight increase in weight, due to the absorption of water, which they lost again after exposure to the air for an hour or two. The tenacity and structure of the material did not appear to have undergone the slightest change.

No. 2, which had been folded up and introduced into a bottle, the mouth of which was open and inverted, had increased in weight from 500 grs. to 524·5 grs., or about 5 per cent., owing to absorption of oxygen from the air. The outer layers of the sheet, where exposed to light, were brittle and resinous in appearance; but the inner portion, which had been screened from light by the outer folds, was but little altered in texture or appearance.

On examining chemically a portion of the most brittle part, I found a large portion of it to have lost the composition of gutta and to have become converted into a matter soluble in alcohol, 55 per cent. of the mass being in fact transformed into the resin already spoken of.

The sample No. 3, which had been kept in the dark, had experienced little or no change. It had only increased 2·5 grains in weight, or 0·5 per cent.; and when treated with alcohol gave up 7·4 per cent. of resinous matter to it.

These results agree with those which I made upon gutta-percha which had been exposed to the light of day for the shorter period of two months. This specimen had become quite brittle, had increased in weight 3·6 per cent., and yielded 21·5 per cent. of resinous matter soluble in alcohol; whilst a piece of the same sheet kept in the dark had undergone no sensible change.

Samples of sheet gutta-percha were also subjected in November last to the action of the following liquids and exposed to diffused daylight:—

A. Boiled linseed oil.
B. Linseed oil not boiled.
C. Stockholm tar.
D. Coal tar.

When examined on the 4th of August 1860, or at the end of nine months, these liquids were found not to have exerted any perceptible solvent action upon the gutta, which retained its texture and tenacity in all those portions which had been fairly submerged in the liquid, and protected from the light and atmospheric air; but in those portions which had projected into the atmospheric air contained in the jar, where it was also exposed to the effects of diffused daylight, the texture had become rotten and the material more or less brittle and resinous.

All the liquids above mentioned are calculated to exclude oxygen from the gutta-percha, and thus they are enabled to exert a preservative influence upon it, without, however, in any degree softening or dissolving its texture. Hence they are likely to be highly valuable agents in coating the insulating material.

3. Experiments on Submarine Cables.

I have examined several specimens of cable from different lines which have been submerged for periods of time varying from a few weeks to seven years. In no case where the cable has been completely and continuously submerged have I found any sensible deterioration in the quality of the gutta-percha.

No. 1. Holyhead Irish cable (from Captain Galton), taken up in Februrry 1859, after seven years' submergence.

No. 2. *a. b. c.* Three specimens from the Dutch line, from Orfordness to Schevening (Mr. L. Clark), submerged five years, raised in August 1859.

This cable was enclosed in a coating of galvanized iron wire, and contained a single wire of copper in gutta, bound round with hemp and tape soaked in boiled linseed oil, tallow, and Stockholm tar.

a. External coating of galvanized wire, not damaged by corrosion. This sample had been buried on the Dutch coast to a small depth in sand.

b. and *c.* Outer galvanized wire much corroded, but the gutta-percha quite sound. The gutta-percha wire had in each of these samples been exposed to the air, out of the metallic casing, for some months, and consequently was drier than the sample *a.* taken from its metallic coating just before it was analysed.

3. *Cable off Portland* (Captain Galton); down for seven months; composed of seven thin copper wires twisted into a strand covered with tar, then coated with gutta-percha, without any metallic protecting envelope.

4. Cable from line *between Candia and Alexandria* (Captain Galton). Construction similar to the last; it showed superficial erosions of the gutta-percha after submergence for a few weeks; but the composition of the gutta was unchanged.

5. New gutta-percha covered wire (Mr. L. Clark); never used.

The only chemical difference perceptible in these different specimens was in the quantity of water mechanically retained in each.

100 parts of each contained:—

	1.	2.			3.	4.	5.
		a.	*b.*	*c.*			
Water -	0·84	8·36	1·75	1·49	4·8	0·95	2·50
Ash -	1·05	...	...	0·76	3·52	0·75	0·50

4. Experiments upon damaged Cables suspended in Air or placed underground.

Of damaged cables I have had various specimens for examination.

1 to 6. Six samples, described by Mr. Saward in his evidence before the Committee, January 12, 1860.

1. Buried in chalky or gravelly soil, coated with a white friable crust of altered gutta-percha. This was very brittle, and contained 35 per cent. of resin; this resin contained 17 per cent. of oxygen combined with a hydrocarbon of the same composition as pure as gutta.

2. Was in soil exposed to leakage of gas pipes, and also was resinous and brittle, but less so than No. 1.

3. Was described as pulpy when raised, as if fermenting; taken from ground into which drainage from oak trees or posts occurred. When forwarded to me it was hard and tough, the copper wire within was slightly corroded and adhered to the gutta-percha, the channel around the wire lined with a pale brown powder. This powder contained traces of copper. It appeared to consist of gutta-percha, as it was almost wholly soluble in benzol, and was insoluble in alcohol. The material in this case seemed to have undergone comparatively little permanent change, although so very different in appearance from ordinary gutta-percha, when it was taken up. It fused below 212°. It had been painted with some pigment containing lead, and on burning left an ash amounting to 1·87 per cent.

4. Not very brittle, taken from iron pipes.

5. Exposed to a dry heat near a baker's oven; coated with red lead.

6. Exposed to a dry heat (exact source not indicated). This was extremely brittle, could be powdered without difficulty, and was almost converted into a resinous substance. It fused below 212°, it left 1·03 per cent. of ash when burnt, and appeared to have been coated with some pigment.

In all these samples those which were most brittle contained the oxidized resinous body in largest proportion. This resinous substance varied somewhat in the proportion of oxygen which it contained in the different samples, but presented the same general properties. It was soluble in cold alcohol, and still more largely in boiling alcohol; was insoluble in ether, and but sparingly in benzol; diluted alkaline leys dissolved it with facility, and the solution coagulated on the addition of an acid in excess.

7 and 8. Samples of gutta-percha covered wire taken from a tunnel in the Stour valley, placed in the tunnel in 1850 (Mr. L. Clark). One portion of this, A, was comparatively little injured; the other, B, was brittle and rotten. This sample had been coated with some pigment containing lead.

It would be useless to cite in detail the various analyses which I have made of these several samples, or to give the numbers obtained for the proportion of moisture, ash, and resin which they contained, or to quote the proportions of carbon, hydrogen, and oxygen ascertained to exist in the altered portion, as contrasted with the unchanged portion, present in each sample.

It may be stated generally, that wherever the proportion of resin was greatest the sample of gutta exhibited the greatest degree of brittleness; and this brittleness was always found to be most marked in the specimens which had experienced the greatest degree of oxidation; and further, that these changes appeared to occur most rapidly and decidedly in those points where the gutta-percha was alternately wet and dry.

App. No. 4. Professor Miller on decay of gutta-percha and india rubber.

5. Experiments on Caoutchouc.

The caoutchouc of commerce is, like gutta, not a pure vegetable principle, and consists of a hydrocarbon of definite composition, mixed with a small quantity of resin, the amount of which varies in different specimens.

The following are the results of my analysis of a sample of pure unmanufactured Para rubber, compared with a sample of good sheet masticated or manufactured rubber.

	Virgin.	Masticated.
Pure caoutchouc - -	96·6	96·64
Moisture - - -	1·3	0·82
Resin - - -	1·8	2·06
Ash - - - -	0·3	0·48
	100·0	100·00

Or, deducting moisture and ash, its elementary composition gave—

	Virgin.	Masticated.
Carbon - - -	85·89	85·53
Hydrogen - - -	11·11	12·06
Oxygen - - -	3·07	2·41
	100·00	100·00

Caoutchouc, like gutta-percha, is, as already stated, liable to deterioration, by exposure to the action of oxygen in the presence of solar light, but the gum is less rapidly injured if exposed to their influence in the native state, than if it has been previously masticated. When subjected to the action of air excluded from light, it does not experience any marked change, even during very long periods. It is, however, important to observe that the masticated rubber is much more porous than the unmanufactured caoutchouc. When immersed in water, caoutchouc absorbs a much larger quantity of this liquid than gutta-percha, and the masticated much more than the unmanufactured or virgin rubber. I subjoin the results of my examination of some samples submitted to experiment by Mr. L. Clark.

A.—Virgin Para Rubber, Finest Quality.

Virgin Para rubber, finest quality.

500 grains of this was exposed in each experiment, in the form of a narrow tape-like strip of rubber, which had been stretched while hot and suddenly cooled. It was of a very pale brown colour. The various samples were submitted to experiment at the end of October 1859, and were examined nine months afterwards (August 4th, 1860).

No. 1, which had been exposed in netting, in the open air, to sun and rain, had become blackened and rotten, but was neither sticky nor crumbled, had increased in weight 34·5 grains or 7 per cent.

No. 2 was exposed in the air and light, but kept dry in a bottle placed mouth downwards; it had increased in weight 14 grains, or 2·8 per cent, by absorption of oxygen, and had become brown, soft, and sticky, especially in the parts most exposed to light. It gave up 11·81 per cent. of an oxidized, soft, and viscous resin to alcohol. The annexed analysis will give an idea of the composition of the resin thus formed:—

Carbon - - -	67·23
Hydrogen - - -	9·54
Oxygen - - -	23·23
	100·0

The proportion of oxygen differs a little in different samples.

No. 3, which was exposed to diffused light in fresh water in an open bottle, had become white and opaque from the absorption of moisture, and had increased 86 grains or 17 per cent., but it had experienced no other alteration in chemical properties, and when dried resumed its original characters.

No. 4, exposed in sea water in an open bottle to diffused light, had absorbed 3·6 per cent. of its weight of water, but was only a little altered in appearance, not in chemical composition.

App. No. 4.

Professor Miller on decay of gutta-percha and india rubber.

B.—*Masticated Rubber, Sheet, Best Quality.*

A similar series of experiments was made simultaneously upon masticated sheet caoutchouc.

No. 1, exposed to sun and rain, had collected into a sticky mass, which had lost its tenacity and elasticity.

Masticated rubber, sheet.

No. 2, in the inverted bottle exposed to diffused light and air, had increased in weight 8 grains or 1·6 per cent., and had collected into a lump, which was viscous, and had lost its elasticity, especially in the parts most exposed to the action of light. When treated with alcohol it was found to yield 12·64 per cent. of its weight of resinous matter to this solvent. These changes were in marked contrast to No. 3, which was kept in a glass bottle in the dark for the same period, but exposed to the air freely. It had increased in weight only 0·6 per cent., did not show any sign of alteration in tenacity or elasticity, and yielded to alcohol 2·0 per cent. of resin only.

No. 4, a sheet of the same rubber immersed in fresh water, open to the air and diffused light, had increased 87 per cent. by absorption of water, that is to say, it had nearly doubled its weight. It had become white, opaque, slimy, and sticky when pressed, and allowed water to be squeezed out by pressure. It lost weight rapidly by drying when exposed to the air.

No. 5, similar to the last, but exposed in sea water. It was slightly opaque and slimy, but had increased only 5 per cent. in weight by absorption. A second sample, in sea water in a closed bottle, emitted a smell of sulphuretted hydrogen, and had gained 5·6 per cent. in weight by absorption. Its elasticity and tenacity were not impaired. The gradual permeability of masticated caoutchouc to water was further strikingly shown by enclosing a quantity of acetate of potash in bags made of sheet rubber and accurately sealed. They were then immersed in water, and at the end of nine months the salt in each of the bags was found to have become liquified by the water which it had absorbed, and the bags had in each case gained in weight several grains.

C.—*A similar Series of Experiments was made with Sheet Rubber, vulcanized.*

Sheet rubber, vulcanized.

1. The sheet exposed in the netting to the sun and rain had lost 2 per cent. in weight; it was scarcely less tenacious than at first.

2. A similar sheet in fresh water absorbed 19 per cent., but was not otherwise altered.

3. A similar sheet in sea water was rather more slimy and had only gained 1·6 per cent. in weight.

Each of the three substances, viz., natural, masticated, and vulcanized rubber were submitted to the action of the following solvents for nine months:—

A. Boiled linseed oil.
B. Unboiled linseed oil.
C. Stockholm tar.

The *virgin rubber* had resisted the action of the solvents almost perfectly, retaining its toughness, excepting in those parts which were above the surface of the liquid and exposed to light. In the tar this rubber had contracted spontaneously, but was still strong and elastic.

The *masticated rubber* was in each instance greatly swollen and gelatinized, and indeed in the case of the unboiled oil was completely dissolved.

The *vulcanized rubber* had also lost its tenacity, and had become swollen and gelatinous, but retained its form and a certain degree of elasticity.

A sample of india-rubber cable (from Captain Galton), which contained six strands of copper wire, each coated with rubber, then bound round with tape and again with rubber, had experienced a singular change, having become, *where in contact with the wire*, quite glutinous and sticky. This change, however, did not progress in the specimen which I kept for some months in my room, but the viscosity on the contrary gradually diminished.*

6. *Experiments on other Substances.*

Experiments on other substances.

An insulating mixture, composed of gutta-percha, shellac, and powdered glass or clay, known as "Wray's Compound" (from Captain Galton), was also submitted to experiment.

Heated to 212°, it softened, but retained its shape. It lost by drying 0·5 per cent. of moisture, and when burnt left 22 per cent. of a white ash, chiefly silicate of alumina. This compound absorbs water but sparingly. 500 grains left in fresh water for six months increased 7·5 grains in weight, or 1·5 per cent., and a similar increase in weight occurred in another experiment where *sea* water was used.

Sample of Gutta-Percha Cable (from Captain Galton) vulcanized by Mackintosh's Patent.

The wire was found to be blackened on its surface from the action of the sulphur, owing to the formation of sulphide of copper, and traces of copper were found in the gutta-percha covering.

WM. ALLEN MILLER, M.D.

To the Chairman of the
Commission on Electric Telegraphs.

Supplementary report by Professor Miller.

SUPPLEMENTARY REPORT upon the ABSORBENT QUALITIES of GUTTA PERCHA and CAOUTCHOUC, and of the Effect of such Absorption on their Insulating Powers, by WILLIAM ALLEN MILLER, M.D., F.R.S.

King's College, London, April 25, 1861.

The following experiments were instituted with a view of testing still further the comparative tendency of gutta percha and caoutchouc to absorb moisture when these two bodies are submerged in water.

They show clearly that whether this submersion takes place under ordinary pressures or under very great pressures maintained for several weeks, there is a marked difference between the two substances. In both materials the amount of moisture absorbed is always less when they are immersed in sea water than when fresh water is employed.

The absorption of water by gutta percha is very slight, indeed almost *nil* in sea water. In fresh water it is appreciable though trifling.

The absorption of water by caoutchouc after submersion is always sensible, the surface being usually rendered white and opaque, owing to the amount of water which has penetrated into the substance. This absorption, however, reaches only to a small depth, and does not destroy or in anywise impair the insulating power of that portion of the caoutchouc which is beneath the moistened layer.

As might have been expected, the experiments prove clearly that the amount of the absorption is dependent upon the extent of surface exposed to the water. For example, in three samples of india-rubber covered wire supplied to me by Mr. Rowland, which had each been submerged for an equal interval in sea water, the thickest coating had absorbed the least when equal weights of cross sections of the coatings were compared.

100 grains, No. 1, 1 inch in circumference, lost 11·1 on drying.

100 grains, No. 2, $1\frac{1}{16}$ inch in circumference, lost 9·7 on drying.

100 grains, No. 3, $1\frac{1}{16}$ inch in circumference, lost 7·4 on drying.

The materials employed in the experiments about to be described consisted of sheets of gutta percha and of caoutchouc of two different thicknesses, and of wires coated with both gutta percha and india-rubber from various sources. Besides these, four bags of each material enclosing known weights of certain deliquescent salts, were sealed, and immersed in fresh water. The sheets and coated wires were each subjected to the action of both sea and fresh water in closed bottles exposed to the action of diffused light.

They were also exposed to the action of sea water for six weeks under a pressure maintained as steadily as was practicable by the hydraulic press, averaging about three tons upon the square inch, though occasionally during the night and on Sundays the pressure gradually declined to 0.

Comparative trials of the insulating power of the coated wires were made before and after subjection to the above-mentioned experiments.

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* The quantity of viscous matter was too small to admit of satisfactory analysis. I ascertained, however, that no copper was present in the viscous mass, and that the wire was not corroded. I could not determine whether grease was present in small quantity, as was not unlikely.

TABLE showing the ABSORPTION of WATER (in Grains) by Gutta Percha and Caoutchouc.

APP. No. 4. Supplementary report by Professor Miller.

Description of Specimen.	Under Pressure, Sea-water, 43 Days.	Sea-water (bottle), 50 Days.	Fresh Water (bottle), 50 Days.	Absorption in Grains per Square Foot of Surface. Sea-water under Pressure.	Sea-water (bottle).	Fresh Water (bottle).
GUTTA PERCHA.						
Sheet $\frac{1}{16}$ inch thick, 8 × 2 inch	1·5	0·4	2·4	5·7	1·8	10·8
Sheet $\frac{1}{32}$ inch thick, 8 × 2 inch	—	0·0	0·6	—	0·0	2·7
Sheet $\frac{1}{32}$ inch, West Ham Company, 8 × 2 inch	0·6	0·4	0·8	2·7	1·8	3·6
9 inches long. Coated wire, A 5,* wire $\frac{1}{16}$ in. diam. gutta percha $\frac{1}{16}$	0·0	0·0	0·4	0·0	0·0	3·2
9 inches long. Coated wire, A 6,* wire $\frac{1}{16}$ in. diam. gutta percha $\frac{1}{16}$	0·3	0·1	0·2	6·15	2·05	4·1
9 inches long. Coated wire, West Ham Company, $\frac{1}{16}$ in. diam.	0·5	†0·4	†0·8	8·0	†6·4	†12·8
		—	‡4·2	—	—	‡4·7
Sealed bags, Gutta Percha Company, 8 inches square, $\frac{1}{32}$ inch thick. Carb. Potash	—	—	Spoiled.	—	—	Spoiled.
Carb. Potash	—	—	‡4·6	—	—	‡5·0
Chloride Calcium	—	—	‡4·5	—	—	‡5·0
Chloride Calcium						
Mean	—	—	—	4·51	2·01	7·1
CAOUTCHOUC.						
Sheet $\frac{1}{16}$ inch thick, 8 × 2 inch } Silver and Co.	—	2·3	3·7	—	18·9	17·3
Sheet $\frac{1}{32}$ inch thick, 8 × 2 inch }	—	4·2	8·3	—	37·2	74·7
Slips, virgin, 8 × 2 inch (Hall and Wells)	3·9	2·1	5·9	17·5	9·45	26·5
Slips, manufactured, 8 × 2 inch (Hall and Wells)	3·8	2·1	4·5	17·1	9·45	20·2
Slips, manufactured, 8 × 2 inch (Silver)	7·0	—	—	31·5	—	—
9 inches long. Coated wire, virgin, $\frac{1}{32}$ inch } Hall and Wells.	1·0	†0·8	†1·5	16·0	†12·8	†24·0
9 inches long. Coated wire, manufactured, $\frac{1}{32}$ inch } Wells.	0·9	†0·5	†1·5	14·4	†8·0	†24·0
9 inches long. Coated wire, manufactured, $\frac{1}{32}$ inch (Silver)	2·2	2·0	3·4	39·6	36·0	57·2
9 inches long. Coated wire, manufactured, $\frac{1}{32}$ inch (Silver)	—	3·0	4·3	—	36·0	51·6
		—	‡40·6	—	—	‡45·7
Bags from Silver and Co., $\frac{1}{32}$ inch in thickness, 8 inches square. Carb. Potash	—	—	‡45·8	—	—	‡51·5
Carb. Potash	—	—	‡47·9	—	—	‡53·9
Chloride Calcium	—	—	‡48·1	—	—	‡54·1
Chloride Calcium						
Mean	—	—	—	22·7	21·0	41·7

* A 5 is copper wire, $\frac{1}{16}$ diameter, covered with two coatings of improved gutta percha, $\frac{1}{32}$ thick, or $\frac{1}{16}$ diameter.
A 6 is copper wire, $\frac{1}{16}$ diameter, covered with two alternate coatings, each of compound and improved gutta percha, $\frac{1}{32}$ thick, or $\frac{1}{16}$ diameter.
† Submerged 41 days. ‡ Submerged 46 days.

The specimens of gutta percha (with the exception of those of the West Ham Company) were from the Gutta Percha Company, City Road. Those of caoutchouc from Messrs. Silver and Co., North Woolwich, and Messrs. Hall and Wells, Borough Road, Southwark. The wire in each case was 16 of copper gauge, or $\frac{1}{16}$ inch thick.

In the foregoing table, the results which are given as the absorption per square foot are calculated from the actual experimental numbers contained in the first three columns of figures.

It will be seen that though the amount of absorption is liable to considerable variation in different specimens of the same substance, that the absorbent action of the caoutchouc is from 5 to 10 times as great as that of the gutta percha.

In determining the insulating powers of the coated wires after submersion, no difficulty is experienced in obtaining accurate results with the gutta percha, but the case is otherwise with the caoutchouc. The white film of caoutchouc which has absorbed moisture is not an insulator. It is therefore necessary to allow the cut surfaces of the wire which have been submerged to become dry by exposure to the air before proceeding to test the insulating power. If this were not attended to the most serious errors would arise.

In order to make these experiments, the coated wires after they had been completely submerged were bent into the form of a horse-shoe, and the middle portions were placed in a jar of water, so as to keep them completely moistened, whilst the ends were allowed to project into the air for some days. The white aspect of the projecting portions of the coating gradually disappeared as the caoutchouc became dry, and the insulating power was completely restored, although the body of the wire remained submerged during the whole time.

The electrical tests and their results are contained in the report of Mr. Rowland.

To the Chairman of the Commission on Electric Telegraphs. WM. ALLEN MILLER.

Experiments made at Silvertown and at King's College under the directions of Professor Wheatstone, F.R.S., and in the presence of Dr. Miller, F.R.S., on the 5th March, 17th, 19th, and 24th April 1861, on the loss of insulation on short lengths of variously-coated copper wires, before and after being subjected to a continuous pressure of three tons on the square inch in iron tubes filled with sea water, from the 5th March to the 17th April 1861, a period of six weeks. Instrument employed, a Peltier electrometer.

Description of Wires.

No. 1. Gutta percha, A 5* (Gutta Percha Company).
2. Do. A 6* Do.
3. Do. West Ham Company.
4. Silver and Co. (masticated).
5. Hall and Wells (do.) No cotton next wire.
6. Hall and Wells (virgin unmasticated rubber). No cotton next wire.

The wires for insulation test, after they were removed from the hydraulic press, were bent in a horseshoe form, the middle portion being immersed in water whilst the ends were allowed to project into the air for rather more than an

App. No. 4. Supplementary report by Professor Miller.

inch, and thus gradually became dry. (Each wire was similarly tested previously to its being subjected to pressure.)

The gutta percha coated wires speedily acquired their maximum insulating power, but the insulating power of the masticated india-rubber increased from day to day as the moisture of the ends in air evaporated.

RESULT of TESTS before and after being subjected to Pressure.

LOSS of INSULATION on INSTRUMENT 3° in one hour. FULL TENSION 42°.

Before Pressure.	Loss of Insulation.	After Pressure.	Loss of Insulation.
	° ′ ″		° ′ ″
No. 1	12 in 1. 38	- -	12 in 1. 17
No. 2	12 in 2. 35	- -	12 in 2. 39
No. 3	12 in 0. 58	- -	12 in 0. 43
No. 4 *	2·45 in 20. 50	- -	2·45 in 27. 0 4 in 37. 10
No. 5	12 in 21. 50	- -	2·45 in 24. 20 4 in 30. ·0
No. 6	12 in 5. 20	The loss of insulation was too great to sustain full tension. This proved to be the case on its being subjected to test on several different occasions; viz., on the 17th, 18th, 19th, and 22d April, 1861.	

* The coating of this wire was violently stretched to the extent of about two inches in being pulled out of the pipe.

The above tests were not instituted to afford a comparison between the insulating powers of the various pieces of covered wires, as they differed in their lengths, from 7½ inches to 9 inches, and in the thickness of their coatings from $\frac{3}{32}$ to $\frac{5}{32}$ of an inch, but to determine the effect of absorption upon the insulation of each material after having been immersed in sea water under pressure, as above described.

OWEN ROWLAND.

Memorandum of Experiments on the effect of Absorption of Water by Gutta-percha and India-rubber.

EXPERIMENTS upon the ACTION of FRESH and SEA WATER upon the insulatory properties of GUTTA-PERCHA in sheets $\frac{1}{16}$ inch in thickness.

Experiments upon the effect of absorption of water by gutta-percha and india rubber.

A sheet weighing 500 grains was put in an open glass bottle filled with fresh water, and exposed to air and light in the office on the 26th October 1859. On the 17th November it was taken out and examined. Very shiny and much lighter in colour. It was dried between sheets of blotting-paper and towels and weighed. Increase in weight 10 grains. Tested for insulation for dynamic and static electricity with 512 elements. No loss.

Another sheet (500 grains) at the same time was put in a bottle filled with sea water and tested in the same manner as above. It was rather firmer and darker in colour. Increase in weight 5 grains, insulation good. Sheet again replaced in water. On the 8th February 1860 this sheet was again examined. Appearance much the same. It was this time exposed in the office for 4 hours. Increase of weight 3 grains. Insulation good.

On the 4th August the sheet in fresh water was finally examined, dried between towels and sheets of blotting paper. In appearance very slimy; the slime being scraped off no change observable in colour or texture. Dried between towels, &c.; increase of weight 16 grains. Exposed to air for 2 hours, 13 grains. Insulation good.

Sheet in sea water examined; no change in texture—darker in colour—very slimy; slime scraped off. Increase in weight 8 grains; in half-an-hour 7 grains.

EXPERIMENTS upon the ACTION of FRESH and SEA WATER upon the insulating properties of MASTICATED INDIA-RUBBER in sheet $\frac{1}{16}$ inches in thickness.

A sheet weighing 500 grains was put in an open glass bottle filled with fresh water, and exposed to air and light in the office on the 1st Nov, 1859. On the 17th it was taken out and examined. Slimy and lighter in colour; dried between towels and sheets of blotting-paper and weighed; increase in weight 29 grains; tested for insulation for dynamic and static electricity with 512 elements; no loss.

Another sheet (500 grains) was put at the samestime in bottle filled with sea water, and treated as above described. Rather slimy to the touch; of a light brown colour; increase in weight 21 grains on 521 grains; insulation good.

On the 8th Feb. 1860 this sheet was again examined. It was very slimy; very much lighter in colour; no change in its elasticity. It was exposed for about 4 hours in the office; increase of weight 15 grains, or 515 grains; insulation good.

On the 4th August 1860 the sheet in fresh water was finally examined, dried between towels and sheets of blotting-paper. Increased weight (put down by Mr. Clark), 435 grains; colour very light and very slimy; no loss of insulation.

Sheet in sea water, &c. Increase in weight, 24½ grains; appearance much the same; very slimy; insulation no loss.

A large sheet of masticated rubber of the same thickness was put in a pan filled with fresh water, together with several india-rubber bags containing acetate of potash on the 18th Oct. 1859. It was taken out on the 3rd July 1860 and examined. In colour it was rather whitish and slimy to the touch. The sheet was formed into a bag or bottle by drawing and tying its sides together, and filled with sea water, and the bottle part left in a basin containing sea water until the edges (out of the water) of the sheet were perfectly dry to avoid surface conduction. For electrical test it was placed upon a copper plate in connection with earth through instrument. The battery wire was then inserted in the water inside the bottle; the insulation was found to be perfect. It was afterwards tested in various ways with the same result. Having washed the sheet in clean fresh water and exposed it to the open air it regained its original colour; appeared perfectly clean and glossy. The result of these severe tests and the rapidity with which sheets of gutta-percha and india-rubber dry in open air seems to favour an impression that water does not permeate the substances but forms a slimy excrescence, more especially in the case of masticated india-rubber, upon their surfaces. A great quantity of water adheres to the surface of the india-rubber sheet in consequence of the furrows caused by the action of the cutting machine.

The acetate of potash put into the india-rubber bags was found liquefied, and had been so for some months. I attribute this to the imperfect sealing of the bags; the insulation of the bags was in each case perfect.

The surface of gutta-percha becomes *darker* in water; of india-rubber *lighter*.

OWEN ROWLAND.

App. No. 5.

APPENDIX No. 5.

On the PERMEABILITY of various kinds of INSULATORS for SUBMARINE ELECTRIC CABLES, by WILLIAM FAIRBAIRN, Esq., C.E., F.R.S.

On the permeability of various kinds of insulators for submarine electric cables, by W. Fairbairn, F.R.S.

THE following experiments were prosecuted at the request of the Commission, with a view to determine how far the numerous kinds of material proposed as insulating coverings for electric submarine cables were reliable when placed at the bottom of the ocean under a great pressure of water, what amount of water they absorbed, and how far their permeability was affected by temperature. The inquiry was an important one, and the results are in some respects unexpected. It appears that all insulators which have been subjected to experiment absorb more or less water under pressure, even those which are closest in texture, such as vulcanized india-rubber and gutta-percha; and it appears that this absorption increases the longer the specimen is retained under water, the greater the pressure to which it is subjected, and the higher the temperature of the water in which it is immersed. The very limited time which has been available for these experiments has prevented our doing more than indicate decisively these general facts without determining the numerical relations of the quantities absorbed under different conditions of time, pressure, or temperature. But already the experiments point out a very important inquiry, some of the methods by which that

inquiry may be prosecuted, and some of the conditions which must be attended to to ensure reliable and corresponding results.

Generally in regard to insulating power, the various materials tried arrange themselves in the following order of permeability, the first absorbing least water, and the last absorbing most:—

1. Chatterton's compound.
2. Gutta-percha.
3. Masticated india-rubber.
4. Vulcanized india-rubber.
5. Carbonized india-rubber.
6. Wray's compound.
7. Unmasticated bottle india-rubber.

Our experiments on the insulating power of various cores under pressure are less complete than those on absorption, and have been prosecuted under greater difficulties and with less variety of conditions.

So far as the experiments go, however, Wray's core exhibited very high insulating powers, retaining the charge longer than any other tried. Next in order to this we may place a core of pure india-rubber coiled in two coats over a wire, but this very rapidly lost its insulating power under pressure. Then we may place a core of pure gutta-percha cured by the Mackintosh process, and the experiment on this is perhaps the most satisfactory of the series; the pressure was retained upon the cable for 406 hours, in which period it exhibited considerable diminution of insulation. A core of 20 alternate coats of gutta-percha and Chatterton's compound also exhibited good insulation unimpaired after 170 hours immersion. The experiments on core subjected to pressure in an insulating liquid before being placed in our hands give anomalous results. The insulation increased instead of diminishing as the liquid dissolved out.

SECTION I. APP. No. 5.

On the permeability of various kinds of insulators for submarine electric cables, by W. Fairbairn, F.R.S.

Experiments on the permeability of various insulators under extreme pressure, as indicated by increase of weight.

Experiments on the Permeability of various Insulators under extreme Pressure, as indicated by Increase of Weight.

The first experiments have for their object the determination of the increase of weight of various insulating materials, when subjected to enormous pressure under water. A series of insulators were selected, such as gutta-percha, india-rubber, Wray's compound, Chatterton's compound, vulcanized india-rubber, india-rubber compounded with carbon, and marine glue. Of these suitable sized pieces were prepared and placed in a strong steel cylinder, and subjected to pressure by means of a lever and plunger. Before their introduction into the cylinder, and whilst dry, they were carefully weighed in a delicate balance. Then, after having been subjected to pressure for a shorter or longer period, as the case might be, they were again dried on the surface, and immediately weighed. The increase of weight due to the pressure under water is the measure of the quantity of water which had been absorbed, or rather forced into the pores of the insulator.

Fig. 1 represents the apparatus employed in these experiments. C is the large cylinder of wrought iron in which the specimens were placed; p, its plunger, two inches diameter. Fig. 2 shows the general arrangement of the apparatus; L, L, the large lever; F, its fulcrum; and P, the plunger of the cylinder C, in which the weighed specimens were placed. The plunger is guided vertically by the box B, B, forming part of the general case or stand in which the lever is placed. By means of weights suspended on the extremity of the lever, the requisite pressure could be applied to the water in the cylinder C.

Fig. 1.

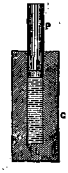


Fig. 2.

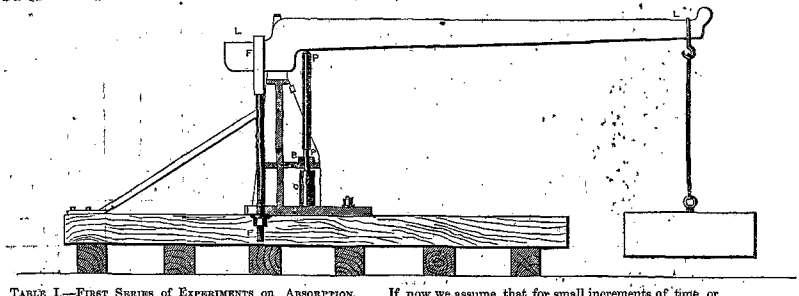


TABLE I.—FIRST SERIES of EXPERIMENTS on ABSORPTION, under a PRESSURE of 20,000 lbs. per SQUARE INCH, and at the ORDINARY ATMOSPHERIC TEMPERATURE.

No. of Experiment.	Insulator.	Pressure in lbs. per sq. in.	Thickness of Specimen.	Area of Specimen.	Weight Before Immersion.	Weight After Immersion.	Gain or Water absorbed.	Duration of Immersion.
	Masticated * india-rubber.	20,000	ins. $\frac{1}{8}$	sq. ins. 13·66	grains. 92·345	grains. 92·385	grains. 0·040	hours. 20
					92·385	92·408	0·023	20
					92·408	92·548	0·140	50
	Total -	—	—	—	—	—	0·203	90
2	India-rubber compounded with carbon.	20,000	$\frac{1}{8}$	13·66	145·851	145·865	0·014	20
					145·865	145·900	0·035	50
	Total -	—	—	—	—	—	0·049	70
3	Wray's compound.	20,000	$\frac{1}{8}$	10	140·423	140·420	loss.	20
					140·420	140·440	0·020	20
					140·440	140·485	0·045	50
	Total -	—	—	—	—	—	0·065	90
4	Gutta-percha	20,000	$\frac{1}{8}$	9	66·735	66·770	0·035	65
					66·770	66·770	0·000	23
	Total -	—	—	—	—	—	0·035	88

The temperature in all these experiments was low, sometimes several degrees below the freezing point. In the first experiment with Wray's compound, the cylinder when opened was found to be filled with loose ice.

* Manufactured by Mackintosh.

If now we assume that for small increments of time, or of area of surface exposed to pressure, the water absorbed is proportional to the time or the area, we may reduce these experiments to common terms of comparison. Let us take 20,000 lbs. for the common pressure, and reduce the results to 100 hours of exposure, and 10 inches area.

REDUCTION of PRECEDING RESULTS.

No. of Experiment.	Insulator.	Pressure in lbs. per sq. in.	Equivalent Column of Water, in Miles.	Duration of Immersion, in Hours.	Area of Specimen, in sq. in.	Water absorbed, in Grains.
1	India-rubber -	20,000	8·720	100	10	0·177
2	India-rubber with carbon.	20,000	8·720	100	10	0·055
3	Wray's compound -	20,000	8·720	100	10	0·072
4	Gutta-percha -	20,000	8·720	100	10	0·044

Here the last column shows the gutta-percha to be least absorbent, and the india-rubber most so. Wray's compound absorbed more than carbonized india-rubber, but less than pure india-rubber. The pure india-rubber appears to combine superficially with water as the surface becomes white, either at parts, as in the present experiment, or over the whole surface. The carbon appears to prevent the formation of this hydrate, and at the same time reduces the elasticity of the native rubber, and enables it to be worked more kindly.

In the next series, the whole of the specimens were placed in the same cylinder, fig. 1, and remained under pressure during the same period and under the same conditions.

App. No. 5.

On the permeability of various kinds of insulators for submarine electric cables, by W. Fairbairn, F.R.S.

TABLE II.—EXPERIMENTS ON ABSORPTION, under a PRESSURE of 5,000 lbs. and at the ORDINARY TEMPERATURE.

No. of Experiment.	Insulator.	Pressure in lbs. per sq. in.	Thickness of Specimen.	Area of Specimen.	Weight Before Immersion.	Weight After Immersion.	Gain, or absorbed.	Duration of Immersion, in Hours.
			ins.	sq. ins.	grs.	grs.		
1	Unmasticated india-rubber.	5,900	¼	3	117·025	118·225	1·23	450
2	Gutta-percha, -	5,900	1/12	4·75	175·77	175·95	0·18	450
3	„ -	5,900	1/12	9	66·72	66·88	0·16	450
4	Masticated india-rubber.	5,900	1/12	4·25	118·465	118·475	0·10	450
5	Wray's compound	5,900	¼	4	141·80	142·1	0·30	450
6	Chatterton's compound.	5,900	¼	4	131·68	132·03	0·25	450
7	India-rubber with carbon.	5,900	¼	5	110·88	111·37	0·49	450
8	Masticated india-rubber.	5,900	1/12	5·5	90·03	90·38	0·35	450
9	„ „	5,900	1/12	5	68·57	68·92	0·35	450
10	„ „	5,900	1/12	9	62·505	63·145	0·64	450
11	Vulcanized india-rubber.	5,900	⅛	7·5	89·865	90·015	0·11	450
12	Chatterton's compound.	5,900	1	6	256·35	256·46	0·11	450
13	Wray's compound	5,900	1	6	296·35	296·77	0·42	450
14	Gutta-percha -	5,900	1	6	253·33	253·55	0·22	450
15	Marine glue -	5,900	1	—	197·30	196·75	(loss) 0·55	450

These tables show that of all the substances tried, native unmasticated india-rubber absorbs by far the most water. The whole surface of the specimen had lost its black colour, and become whitened during the experiment. Taking the mean of three experiments very closely agreeing, we find that india-rubber, after manufacture, absorbs less water than in its native state, in the proportion 0·682 to 3·07, or 1 to 4½. Vulcanized india-rubber appears to be the least absorbent substance tried, but when combined with carbon, it absorbs nearly one-third more water (according to the results in this table) than in its pure masticated state. Gutta-percha and Chatterton's compound are nearly alike in their resistance to absorption, the latter being superior. In these experiments they increased in weight only one-half as much as pure india-rubber (masticated), and twice as much as vulcanized india-rubber. Wray's compound absorbed rather more than masticated india-rubber. Marine glue lost instead of increasing its weight.

RESULTS REDUCED to 10 INCHES AREA.

No. of Experiment.	Insulator.	Pressure in lbs. per sq. in.	Equivalent Volume of Water, in Miles.	Duration of Experiment, in Hours.	Area of Specimen, in sq. ins.	Water absorbed, in Grains.	
1	India-rubber, unmasticated.	5,900	2·575	450	10	3·075	
4	India-rubber, masticated.*	5,900	2·575	450	10	0·023	
8	„ „	5,900	2·575	450	10	0·636	0·682
9	„ „	5,900	2·575	450	10	0·700	
10	„ „	5,900	2·575	450	10	0·711	
11	India-rubber, vulcanized.	5,900	2·575	450	10	0·146	
7	India-rubber, carbonized.	5,900	2·575	450	10	0·980	
2	Gutta-percha -	5,900	2·575	450	10	0·378	0·327
3	„ „	5,900	2·575	450	10	0·177	
14	„ „	5,900	2·575	450	10	0·366	
5	Wray's compound -	5,900	2·575	450	10	0·750	0·725
13	„ „	5,900	2·575	450	10	0·700	
6	Chatterton's compound.	5,900	2·575	450	10	0·375	0·279
12	„ „	5,900	2·575	450	10	0·183	

Comparing these experiments with the last, we find that these materials are far from following a law of simple proportion in the amount of water absorbed in different times. The present experiments were made under a pressure of 5,900 lbs. per square inch, and lasted for a period of 450 hours. The last were made under a pressure of 20,000 lbs., and lasted less than 100 hours. In the present experiments carbonized india-rubber absorbed 17 times as much as in the former; Wray's compound, 10 times; gutta-percha, 7 times; and masticated india-rubber only 4 times. Hence it appears that, other things being equal, masticated india-rubber would be most advantageous, and carbonized india-rubber least so, as insulators; because, so far as these experiments afford data for generalizing, masticated india-rubber follows a rate of absorption diminishing most with time, and carbonized india-rubber least so. This deduction, however, is complicated by the fact of a difference of pressure, and possibly of temperature, in the two experiments.

* The india rubber, in experiment 4, was manufactured by that in 8, 9, and 10 by Mackintosh.

The order of merit in resisting absorption, as derived from this series of experiments, is,—

1. Vulcanized india-rubber.
2. Chatterton's compound.
3. Gutta-percha.
4. Masticated india-rubber.
5. Wray's compound.
6. Carbonized india-rubber.
7. Not masticated india-rubber.

The next series of experiments were made under greater pressure, but in the same manner and for the same period of immersion.

TABLE III.—THIRD SERIES of EXPERIMENTS on ABSORPTION at ORDINARY TEMPERATURES.

No. of Experiment.	Insulator.	Pressure in lbs. per sq. in.	Thickness of Specimen.	Area of Surface.	Weight Before Immersion.	Weight After Immersion.	Gain, or Water absorbed.	Duration of Exposure.
			ins.	sq. in.	grs.	grs.		
1	Wray's compound	15,000	1/12	5·5	194·04	194·35	0·31	450
2	„	15,000	1/12	4·5	196·96	197·29	0·33	450
3	Chatterton's compound.	15,000	1/12	5·5	87·79	87·82	0·03	450
4	„	15,000	1/12	4·5	165·04	165·06	0·02	450
5	Gutta-percha -	15,000	1/12	4·5	161·95	162·03	0·08	450
6	Raw India-rubber	15,000	1/12	4·0	111·73	112·39	0·66	450
7	Masticated india-rubber.	15,000	1/12	4·0	107·12	107·21	0·09	450
8	„ „	15,000	1/12	10·25	170·87	171·17	0·30	450
9	„ „	15,000	1/12	9·6	135·25	135·54	0·29	450
10	Carbonized india-rubber.	15,000	1/12	9·6	132·55	132·84	0·29	450

REDUCTION of RESULTS to 10 INCHES AREA.

No. of Experiment.	Insulator.	Pressure in lbs. per sq. in.	Equivalent Volume of Water, in Miles.	Duration of Experiment, in Hours.	Area of Specimen, in sq. ins.	Water absorbed, in Grains.	
6	Raw india-rubber -	15,000	6·54	450	10	1·65	0·62
7	Masticated india-rubber.	15,000	6·54	450	10	0·22	
8	„ „	15,000	6·54	450	10	0·29	
9	„ „	15,000	6·54	450	10	0·30	
10	Carbonized india-rubber.	15,000	6·54	450	10	0·29	
5	Gutta-percha -	15,000	6·54	450	10	0·18	
1	Wray's compound	15,000	6·54	450	10	0·56	0·57
2	„ „	15,000	6·54	450	10	0·58	
3	Chatterton's compound.	15,000	6·54	450	10	0·054	0·057
4	„ „	15,000	6·54	450	10	0·058	

The temperature during these experiments was generally lower than in the second series, being frequently at the freezing point. There was loose ice in the cylinder when opened.

The higher pressure in these experiments seems to bring out more decisively the differences in the amount of absorption, but it is remarkable that whilst the relative absorption does not widely differ, and the order of the insulators in their resistance to absorption is the same, the absolute quantity absorbed under greater pressure is less than in the previous series of experiments. The only discrepancy between the two series of experiments is the relatively low absorption of masticated india-rubber.

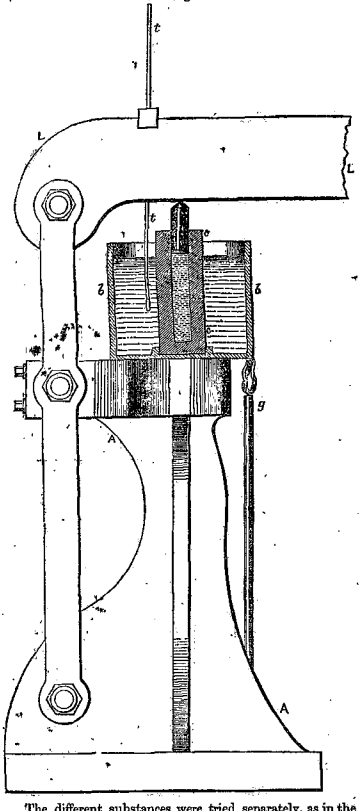
The order of merit, or power of resisting absorption, is in these experiments,—

1. Chatterton's compound.
2. Gutta-percha.
3. Masticated india-rubber.
4. Carbonized india-rubber.
5. Wray's compound.
6. Raw india-rubber.

The last in this series absorbed twenty-seven times as much as the first; gutta-percha and Chatterton's compound hold, as before, the highest place; but the superiority of the latter was more manifest; it had become whitened at the surface, but apparently the water had only penetrated the thinnest possible film.

The next experiments were made with a view to determine the effect of temperature on the absorption of water by these insulators. Recourse was had to the small cylinder *c*, fig. 3, which was surrounded by the water bath *b*, *b*, maintained at a uniform temperature of 100° Fahr. by the gas jet *g*. *t*, *t*, is the thermometer. The lever by which the pressure was applied to the plunger is shown at L, L_2 attached to the firm cast-iron base A, A.

Fig. 3.



The different substances were tried separately, as in the first series, and the weighings were repeated at intervals. During the night it was necessary to remove the gas jet, as the uniformity of temperature could not be depended upon; hence, for half the period of immersion the specimens were at a temperature of 50° only, and for the remainder at a temperature of 100°. The loss of weight, after removal from the cylinder, in consequence of the evaporation of the water absorbed, was in these experiments noted, and it was found that the specimens decreased in weight below their original weight when dry.

In the whole of these experiments the pressure was 20,000 lbs. per square inch; area of specimens, 8 square inches, and thickness about $\frac{1}{8}$ inch.

TABLE IV.—FOURTH SERIES of EXPERIMENTS on ABSORPTION at INCREASED TEMPERATURES.

No. of Experiment.	Insulator.	Duration of Exposure, in Hours. At 50° F.	Duration of Exposure, in Hours. At 100° F.	Weight Before Immersion. grains.	Weight After Immersion. grains.	Gain, or water absorbed, in grains.	Ultimate weight after drying.
1.	Gutta-percha (Remained exposed to air during the night).	—	11	116·48	116·48	0·0	
		—	29	116·48	116·56	0·08	
2.	Gutta-percha	13	13	102·73	102·91	0·18	
		26					

Table IV.—Fourth Series of Experiments, &c.—*continued.*

No. of Experiment.	Insulator.	Duration of Exposure, in Hours. At 50° F.	Duration of Exposure, in Hours. At 100° F.	Weight Before Immersion. grains.	Weight After Immersion. grains.	Gain, or water absorbed, in grains.	Ultimate weight after drying.
3.	Gutta-percha (Same specimen as last).	0	8	102·32	102·68	0·16	
		14	18	102·68	102·75	0·07	
		92	54	102·75	102·83	0·08	
	Total	146				0·31	99·94
4.	India-rubber (Masticated).*	40	12	105·235	105·455	0·220	
	" "	52	24	105·455	105·570	0·115	
	" "	76	48	105·570	105·840	0·270	
	" "	100	66	105·840	105·980	0·140	
	" "	142	70	105·980	106·000	0·020	
	Total	212				0·765	105·20
5.	Wray's compound	20	12	100·17	100·52	0·35	
	" "	32	24	100·52	100·60	0·08	
	" "	86	48	100·60	100·94	0·34	
	" "	98	60	100·94	101·00	0·06	
	" "	122	84	101·00	101·13	0·13	
	Total	206				0·96	100·40
6.	Chatterton's compound.	52	48	93·73	93·89	0·16	
		100					93·42
7.	Vulcanized India-rubber.†	52	21	110·68	111·32	0·64	
		114	78	111·32	111·92	0·60	110·1
		192				1·24	

RESULTS REDUCED TO 100 HOURS, and 10 INCHES AREA.

No. of Experiment.	Insulator.	Pressure per square inch, in lbs.	Duration of Exposure, in Hours.	Temperature, Fahrenheit. (Mean.)	Area of Specimen, in square inches.	Water absorbed, in grains.	Loss of weight in drying.
3.	Gutta-percha	20,000	100	75	10	0·27	0·61
4.	India-rubber	20,000	100	75	10	0·45	0·87
5.	Wray's compound	20,000	100	75	10	0·58	0·91
6.	Chatterton's compound	20,000	100	75	10	0·20	0·40
7.	Vulcanized rubber	20,000	100	75	10	0·30	2·37

Comparing the numbers in this table with those in the first series, which were made under precisely similar conditions in all respects, except temperature, which then did not exceed an average of 40° or 45° Fahr., it becomes evident that temperature has a considerable effect on the amount of water absorbed. Thus, gutta-percha at 45° absorbed 0·044 grains, at 75°, 0·27 grains, or six times at much. In like manner, india-rubber absorbed 0·177 grains at the lower temperature, and 0·45 at the higher, or 2½ times as much. Wray's compound, 0·072 at the lower temperature, and 0·58 at the higher, or seven times as much.

Reasoning upon the foregoing experiments, a question arises as to the ratio or quantity of water absorbed in different times, and the condition of the specimens after a much more lengthened immersion. The present experiments, although showing the relative permeability of different insulators, do not afford data to determine the ultimate condition of the material intended to surround and insulate the conducting wires of the electric cable. To ascertain these facts, a much more enlarged series of experiments are required, extending over a much greater length of time. If, for example, gutta-percha absorbs 0·15 grain of water in 100 hours, under a pressure of 20,000 lbs. on the square inch, we want to determine the corresponding quantity absorbed in 1,000 hours; and further, at what period will the continuous absorption cease? These are questions of vital importance as regards the porosity of the specimens; and when ascertained, we should then still require to know to what extent the insulation of the electric current would be impaired in the cable saturated with moisture.

Should our best insulators, such as Chatterton's compound ‡ or gutta-percha, as given in the experiments, arrive at a point at which they will absorb no more water under a given pressure, it then becomes necessary that we should ascertain whether the water imbibed is sufficient to carry off the whole or a part of a voltaic current, and whether the passage of the current through the insulator would accelerate, in turn, the oxidation and consequent destruction of the conductor. To solve these questions, in my opinion, we require a long series of carefully conducted experiments, which would tend to give a reliability to these important undertakings which at present they have not attained.

* Manufactured by Mackintosh.

† These specimens became quite black at the cut edges, and over a considerable portion of the surface.

‡ Chatterton's compound, however, is not intended to be employed alone, but as an accessory with gutta-percha,

On the permeability of various kinds of insulators for submarine electric cables, by W. Fairbairn, F.R.S.

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On the permeability of various kinds of insulators for submarine electric cables, by W. Fairbairn, F.R.S.

Experiments on the insulating power of various cores when placed under pressure.

SECTION 2.

Experiments on the Insulating Power of various Cores when placed under Pressure.

The earlier experiments with these cores were made with voltaic electricity, but owing to the shortness of the specimens it was found impossible so far to destroy their insulation by the absorption of water as to permit a current from a small battery to pass through the covering.

Failing in this, recourse was had to frictional electricity, which, from its high intensity, passed with greater or less facility through the insulating coverings of the wire. Still the difficulty of deciding upon the period at which, after remaining under pressure, the insulation began to grow less perfect, remained to a large extent unremoved. This difficulty was very much increased by the necessarily short period in which the experiments had to be completed. It was impossible in many cases to leave the cores long enough under pressure to ascertain clearly the entrance of water, and only in one or two instances was any defect in the cable detected, beyond question, by the gradual loss of insulating power in the specimen under trial. To inadequacy of time were added manipulative difficulties, such as the making of a packed joint which should hold tight against so enormous a pressure as 10,000 lbs. per square inch, and also the variable hygrometric condition of the atmosphere.

The earlier and preliminary experiments were made with a simple double pith ball electrometer suspended from one of the exposed ends of the cable. This method, however, did not allow of sufficient accuracy in the measurement and regulation of the charge and the rate of loss, to afford satisfactory results.

The following method was then adopted:—The core was placed in a steel cylinder C, C, figs. 4 and 5, with the ends

Fig. 4.—½ Section.

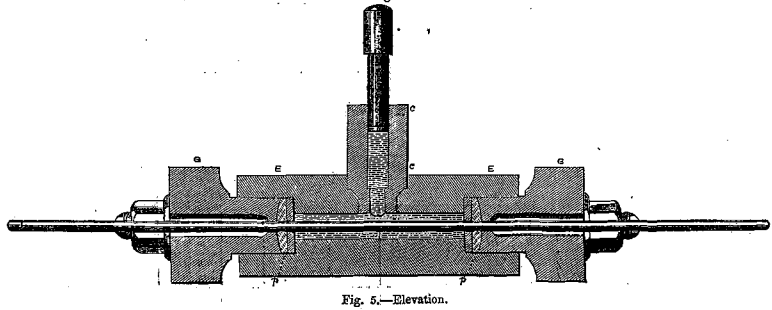
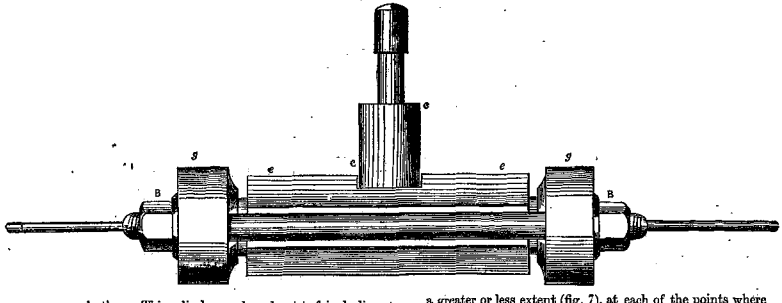
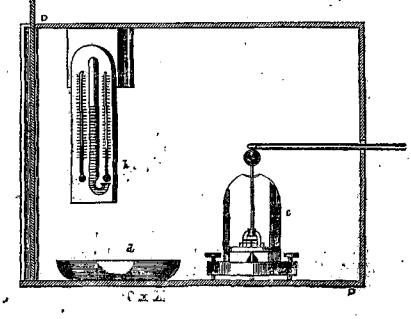


Fig. 5.—Elevation.



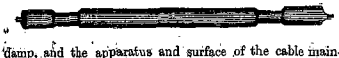
projecting. This cylinder was bored out to ¾ inch diameter, and at either end a pair of strong brass glands were fitted (G, G), so as to compress round the core the vulcanized india-rubber packings *p*, *p*, by the aid of the bolts and nuts B, B. The compression thus applied indented the core to a greater or less extent (fig. 7), at each of the points where the india-rubber packings were applied; and this indentation was greater or less according to the pliability of the insulator. Communicating with the large cylinder C, C, is a smaller cylinder *c*, *c*, fitted with a solid plunger. The pressure was applied, through the medium of the plunger, by a lever L, L (fig. 3), after the cylinders had been filled with water. Up to about 10,000 lbs. pressure per square inch, or a pressure equivalent to the weight of a column of water 4·36 miles high, the cylinder would stand without leakage, but beyond this pressure the water forced its way amongst the packings, and either with or without external leakage, prevented the attainment of any higher pressure from the fall of the plunger on its bearings.

Fig. 6.



One end of the core was hermetically sealed in all but the earliest experiments. The other end was covered with a rounded brass cap, and surrounded by a closed box D, D (fig. 6), containing dishes, *d*, of concentrated sulphuric acid, an electrometer, *e*, and a hygrometer, *h*. By means of the acid the atmosphere round the cable was maintained in a tolerably uniform condition of dryness in a room otherwise

Fig. 7.



damp, and the apparatus and surface of the cable maintained under similar conditions throughout the whole of the experiment.

The electrometer employed is known as the Peltier's electrometer. In this instrument the electricity being simultaneously communicated to a fixed bar and to a metallic index, the latter is repelled. A directive force is given to the index by means of a small magnetic needle in order to retain it at zero when no electric force acts upon it.

The charge was given from an electrophorous, and was ordinarily of such intensity as to deflect the needle through an arc of 70°. The fall of the needle from loss of charge, was then watched at intervals as nearly uniform as was convenient, until the needle had sunk to 20°.

TABLE I.—CORE CABLE of GIBRALTAR, cured by MACKINTOSH PROCESS; diameter, 0·45 ins.; diameter reduced at collars, 0·86.

No. of Experiment.	Duration of Exposure, in Hours.	Hygrometer.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Duration of Retention of Charge.	Charge.	Relative Intensity of Charge.
					′ ″	°	
1.	0	- - -	6,110				
	96		7,060				
	216		9,190				
	240		10,000				
	280		10,000	4·364	0. 0	78	1,040·9
					10. 0	70	1,000·0
					37.20	50	815·1
					57.15	40	684·0
					83.20	30	532·1
					138. 40	20	364·0
2.	285	- - -	10,000	4·363	− 9.10	78	1,040·9
					+ 0. 0	70	1,000·0
					30. 0	44·5	745·9
					60. 0	28	499·6
					90. 0	28·5	454·4
					120. 0	21·5	390·0
					134. 0	20	364·0
3.	328	- - -	10,000	4·363	− 12. 0	78	1,040·9
					+ 0. 0	70	1,000·0
					30. 0	46·7	761·6
					60. 0	33·3	559·9
					120. 0	14·8	371·8
					158. 0	10·0	184·8
4.	401	- - -	10,000	4·363	0. 0	70	1,000
					30. 0	16	293·2
					35.20	10	184·8
5.	408	- - -	10,000	4·363	0. 0	70	1,000·0
					15. 0	46·5	771·9
					30. 0	25	449·7
					35. 0	20	364·0
					60. 0	15	275·4
					73. 0	10	184·8

Remarks.—In the first two experiments the rate of diminution of the charge was nearly the same, the core having been under pressure 280 and 285 hours respectively, and the needle returning from 78° to 20° in 139 and 143 minutes respectively. The third experiment shows a decrease of insulating power, the same loss of charge occurring in 100 minutes. In the fourth and fifth experiments, made on the same day, but with a drier atmosphere in the later (afternoon) experiment, the deterioration is more strikingly exhibited. The charge diminished from 70° to 10° in 35 minutes, in one case, and 73 in the other.

TABLE II.—CORE received from GUTTA-PERCHA COMPANY, without label, sealed at each end, precisely similar to one labelled as steeped in an insulating liquid; diameter, 0·45 ins.; reduced diameter at packings 0·86 ins.

No. of Experiment.	Duration of Exposure, in Hours.	Hygrometer.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Retention of Charge.	Charge.	Relative Intensity of Charge.
					′ ″	°	
1.	0	Wet bulb, 56°. Dry bulb, 69°. Water in a c. ft. of air, = 4·4 grs. Humidity, 78.	0	0	0. 0	70	1,000·0
					1. 0	60	921·6
					2.15	50	815·1
					3.35	40	684·0
					5.50	30	532·1
					7. 5	20	364·0
							184·8
2.	0	Wet bulb, 57½°. Dry bulb, 61½°. Water in a c. ft. of air, = 4·7 grs. Humidity, 72.	0	0	0. 0	70	1,000·0
					2.30	50	815·1
					3.40	40	684·0
					4.50	30	532·1
					6.20	20	364·0
					8.45	10	184·8
3.	0	Wet bulb, 57°. Dry bulb, 61°. Water in a c. ft. of air, = 4·6 grs. Humidity, 77.	0	0	0. 0	70	1,000·0
					0.50	60	921·6
					2.10	50	815·1
					4.10	30	532·1
					7. 5	10	184·8
4.	24	Wet bulb, 45½°. Dry bulb, 50°. Water in a c. ft. of air, = 2·8 grs. Humidity, 69.	10,000	4·363	−1. 47	75	1,027·9
					+0. 0	70	1,100·0
					3.30	60	921·6
					4.20	50	815·1
					6.15	40	684·0
					8.40	30	532·1
					11.40	20	364·0

TABLE II.—Core received from Gutta-Percha Company—*cont.*

No. of Experiment.	Duration of Exposure, in Hours.	Hygrometer.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Retention of Charge.	Charge.	Relative Intensity of Charge.
					′ ″	°	
5.	48	Dry bulb, 60°. Wet bulb, 54½°. Water in a c. ft. of air, = 3·8 grs. Humidity, 66.	10,000	4·363	0. 0	70	1,000·0
					4.40	60	921·6
					11.50	40	684·0
					18. 0	30	532·1
					25.30	20	364·0
6.	48	Ditto.	10,000	4·363	0. 0	70	1,000·0
					12.20	40	684·0
					22.50	30	532·1
					33.40	20	364·0
7.	56	Ditto.	10,000	4·363	0. 0	70	1,000·0
					2.15	60	921·6
					4.10	50	815·1
					7.35	40	684·0
					9.10	30	532·1
					13. 0	20	364·0
8.	76	Dry bulb, 54°. Wet bulb, 49°. Water in a c. ft. of air, = 2·3 grs. Humidity, 69.	10,000	4·363	0. 0	70	1,000·0
					19. 0	50	815·1
					24.20	40	684·0
					41.30	30	532·1
					64.50	20	364·0
9.	78	Ditto.	10,000	4·363	0. 0	70	1,000·0
					12.20	50	815·1
					17. 5	40	684·0
					21. 5	30	532·1
					29. 20	20	364·0
10.	120	Dry bulb, 47°. Wet bulb, 43°. Water in a c. ft. of air, = 2·6 grs. Humidity, 67.	10,000	4·363	0. 0	70	1,000·0
					5. 0	60	921·6
					11.40	50	815·1
					47.10	30	532·1
					97. 0	20	364·0
11.	170		10,000	4·363	0. 0	70	1,000·0
					19.45	40	684·0
12.	170		10,000	4·363	0. 0	70	1,000·0
					99.30	25	440·7
13.	170		10,000	4·363	0. 0	76	1,090·0
					116. 0	14	257·5

In the earlier experiments this cable showed very feeble insulation, but the insulation increased in a remarkable manner as the experiment proceeded. Contemporaneously a black liquid dissolved out of the cable, probably the insulating liquid which had been forced into it previously. The water in the cylinder was changed between the sixth and seventh and between the eighth and ninth experiments.

The charge decreased from 70° to 20° in an average period of 7 minutes 40 seconds in the first three experiments; in 11 minutes 40 seconds after 24 hours immersion; in 27½ minutes after 48 hours immersion; but after the black liquid in the steel cylinder had been changed, in 13 minutes. After 77 hours immersion, the charge fell from 70° to 20° in 62 minutes; after 120 hours, in 97 minutes; and after 170 hours, in 105 minutes.

My attention has been called to the influence of temperature on the duration of the charge. It is possible that this may have something to do with the smaller variations in the recorded results. Thus, above, in experiment 3, at a temperature of 61°, the charge was retained 7′ 5″; in experiment 4, at a temperature of 50°, the charge was retained for 11′ 40″, or 4′ 35″ longer.

TABLE III.—SINGLE WIRE covered by WHAT'S COMPOUND; diameter ·42 in., reduced at packings to ·33.

No. of Experiment.	Duration of Exposure, in Hours.	Hygrometer.	Pressure in lbs. per sq. in.	Equivalent Column of Water, in Miles.	Retention of Charge.	Charge.	Relative Intensity of Charge.
					H. M.	°	
1.	0	Dry bulb, 63°. Wet bulb, 56°. Water in a c. ft. of air, 2·7 grs. Humidity, 59.	0	0	0. 0	80	1,048·0
					2.15	77½	1,039·2
					4.20	71½	1,008·1
					5.30	67	989·8
2.	0	Ditto.	0	0	0. 0	80	1,048·0
					12.45	44½	745·9
					15. 5	45	684·0
					16. 0	37½	627·7
					18.15	33½	586·9
					20.25	27	483·7
					23.45	23½	454·4
					26. 0	20	364·0

It was found impossible to put any pressure on this core, on account of its small diameter. Its insulating power in the condition in which it was tried, however, was very remarkable. The retention of the charge for 26 hours distinguished it from all the other cables.

On the permeability of various kinds of Insulators for submarine electric cables, by W. Fairbairn, F.R.S.

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TABLE IV.—WRAY'S CORE, similar to the last diameter, 0·43 ins.; reduced diameter at packing, 0·27 ins.

No. of Experiment.	Duration of Exposure, in Hours.	Hygrometer.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Retention of Charge.	Charge.	Relative Intensity of Charge.
					′ ″	°	
1.	0	Dry bulb, 57°. Wet bulb, 50°. Water in a c. ft. of air, Humidity,	0	0	0. 0 1. 0 3.15	77 68 40	1,036·6 985·3 684·0
2.	0	Ditto.	0	0	0. 0 1.11 5.36 8. 6	80 70 31 25	1,048·0 1,000·0 548·4 440·7

The pressure was then put on, but owing to the pliability of the insulator, the packings gave way.

TABLE V.—CABLE subjected to PRESSURE under an INSULATING LIQUID, by the GUTTA-PERCHA COMPANY; diameter, 0·46 ins.; reduced diameter at packings, 0·32 ins.

No. of Experiments.	Duration of Exposure, in Hours.	Hygrometer.	Pressure in lbs. per sq. in.	Equivalent Column of Water, in Miles.	Retention of Charge.	Charge.	Relative Intensity of Charge.
					′ ″	°	
1.	2	Dry bulb, 52°. Wet bulb, 46°. Water in a c. ft. of air, 2·8 grs. Humidity, 64.	10,000	4·363	0. 0 23.46 49.10	76 46 29	1,000·0 634·0 518·8
2.	4	Ditto.	10,000	4·363	0. 0 27. 0 45. 0 52.30 68.30	70 38 28 25 20	1,000·0 660·7 499·6 449·7 364·0
3.	10	Ditto.	10,000	4·363	0. 0 4.10 48. 0	80 70 20	1,048·0 1,000·0 364·0
4.	11		10,000	4·363	0. 0 26.10 44.40	70 30 20	1,000·0 532·1 364·0

This cable was not left very long under pressure; it exhibits, however, considerable variation of insulating power.

TABLE VI.—CORE of TWENTY ALTERNATE COATS of GUTTA-PERCHA and CHATTERTON'S COMPOUND; diameter, 0·42 ins.; reduced diameter at packings, 0·33 ins.

No. of Experiment.	Duration of Exposure, in Hours.	Hygrometer.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Retention of Charge.	Charge.	Relative Intensity of Charge.
					′ ″	°	
1.		Dry bulb, 57½°. Wet bulb, 50°. Water in a c. ft. of air, 3·2 grs. Humidity, 59.	0	0	0. 0 18. 0 34. 0 21.30 75.30 78.30	80 55 37 30 21 20	1,048·0 871·7 640·4 529·1 381·0 364·0
2.	120	Dry bulb, 55°. Wet bulb, 49°. Water in a c. ft. of air, 3·1 grs. Humidity, 65.	10,000	4·363	0. 0 1. 0 6. 0 11. 0 21. 0 31. 0 38. 0	75 70 69 54 41 27 20	1,027·9 1,000·0 921·6 860·4 698·2 433·4 364·0
3.	122	Dry bulb, 55°. Wet bulb, 49°. Water in a c. ft. of air, 3·1 grs. Humidity, 65.	10,000	4·363	0. 0 31.30 36.30 41.30 46.30 48.30	70 27 25 23 21 20	1,000·0 433·4 440·7 425·3 381·0 364·0
4.	150	Dry bulb, 52°. Wet bulb, 46°. Water in a c. ft. of air, 2·8 grs. Humidity, 64.	10,000	4·363	0. 0 17.20 35. 0	80 70 47	1,048·0 1,000·0 778·
5.	151	Ditto.	10,000	4·363	0. 0 15. 0 30. 0 45. 0 79. 0	70 60 51 47 33	1,000·0 921·6 827·5 778·3 579·6
6.	170	Dry bulb, 59°. Wet bulb, 44°. Water in a c. ft. of air, 2·6 grs. Humidity, 63.	10,000	4·363	0. 0 1.10 18.50 39. 0 59. 0 79. 0 101. 0	75 73 51 38 29 24 20	1,027·9 1,000·0 827·5 655·1 515·8 433·5 364·0

This cable, although exhibiting some variation in the insulation, resulting in part from a variable hygrometric condition of the atmosphere, did not appear, after 170 hours of immersion, to have lost any part of its power of retaining the charge, but rather to have gained in insulation.

TABLE VII.—CABLE of PURE INDIA-RUBBER, coiled on in TWO COATS; diameter, 0·44 ins.; reduced diameter at packings, 0·33 ins.

No. of Experiment.	Duration of Exposure, in Hours.	Hygrometer.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Return of Charge.	Charge.	Relative Intensity of Charge.
					′ ″	°	
1.	0	Dry bulb, 55°. Wet bulb, 49°. Water in a c. ft. of air, = 3·1 grs. Humidity, 65.	0	0	0. 0 4. 0 11.30 37. 0 67. 0 122. 0 187½ 0	80 75 70 60 55 49 43	1,048·0 1,027·9 1,000·0 921·6 871·7 802·8 725·7
2.	80	Dry bulb, 55°. Wet bulb, 49°. Water in a c. ft. of air, = 3·0 grs. Humidity, 60.	10,000	4·363	0. 0 3. 0 18.30	80 70 20	1,048·0 1,000·0 364·0

This core showed an almost entire loss of insulation after exposure for 80 hours to a pressure of 10,000 lbs. per square inch.

TABLE VIII.—CORE of GUTTA-PERCHA; diameter, 0·44 ins.; reduced diameter at packings, 0·32 ins.

No. of Experiments.	Duration of Exposure, in Hours.	Hygrometer.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Duration of Retention of Charge.	Charge.	Relative Intensity of Charge.
					′ ″	°	
1.	0	Dry bulb, 59°. Wet bulb, 52°. Water in a c. ft. of air, =3·5 grs. Humidity, 61.	0	0	0. 0 1. 0 2. 0 3.40 4. 0 5. 0 5.40	80 75 68 55 49 38 20	1,048·0 1,027·9 960·8 871·7 684·0 499·6 364·0
2.	0	Ditto.	0	0	0. 0 1. 0 2. 0 3. 0 4. 0 5. 0 6. 0 6.15	80 75 65 52 36 26 22½ 20	1,048·0 1,027·9 960·8 857·8 623·2 466·2 415·0 364·0
3.	264	Dry bulb, 60°, F.	10,000	4·363	−0. 0 +0. 0 1. 0 2. 0 3. 0 4. 0 5. 0 6. 0 7. 0 8. 0	75 70 63 56 49 44 38 30 26 20	1,027·9 1,000·0 943·9 889·8 803·5 741·9 653·0 532·1 492·2 364·0
4.	480	Dry bulb, 62°. Wet bulb, 55½°. Water in a c. ft. of air, = 4·0 grs. Humidity, 65.	10,000	4·363	0. 0 1. 0 2. 0 3. 0 3.50	70 59 40 27 20	1,000·0 913·8 634·4 483·7 364·0
5.	480	Ditto - -	10,000	4·363	−0.30 +0. 0 1. 0 2. 0 3. 0 4. 0 4.50	75 70 63 50 35 27 20	1,027·9 1,000·0 944·0 791·9 610·0 483·7 364·0
6.	482	Dry bulb, 67°. Wet bulb, 61°. Weight of water in a c. ft. of air, 5 grs. Humidity, 68.	10,000	4·363	−0.20 0. 0 1. 0 2. 0 3. 0 3.35	75 70 55 35 27 20	1,027·9 1,000·0 857·7 610·0 483·7 364·0
7.	576	Dry bulb, 66°. Wet bulb, 53½°. Weight of water in a c. ft. of air 4·6grs. Humidity, 66.	10,000	4·363	−0.20 0. 0 1. 0 2. 0 3. 0 3.50	75 70 57 38 26 20	1,027·9 1,000·0 803·0 655·1 492·2 364·0
8.	576	Ditto - -	10,000	4·363	−0.40 +0. 0 1. 0 2. 0 3. 0 3.25	80 70 54 35 24 20	1,048·0 1,000·0 860·4 610·0 433·5 364·0

This core was considerably but unequally reduced in diameter throughout the whole portion under pressure; indeed it appeared to have squeezed part of the gutta-percha along the wire and out of the cylinder. Some deterioration in insulating power had evidently taken place after 576 hours of immersion, as in the earlier experiments the electrometer fell in 5′ 40″, 6′ 15″, and 9′ 0″ respectively, whilst in the two last experiments it fell in 4′ 0″ and 4′ 5″; but the deterioration is not great when the pressure and time are considered, although as an insulator gutta-percha appears to be inferior to india-rubber and Wray's compound.

TABLE IX.—CORE of BOTTLE INDIA-RUBBER, coiled upon Copper Wire, diameter, 0·32 ins.; reduced at packings to 0·20 ins.

No. of Experiment.	Duration of Pressure, in Hours.	Hygrometer.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Retention of Charge.	Charge.	Relative Intensity of Charge.
1.	0	Dry bulb, 76°. Wet bulb, 68°. Water in a c. ft. of air, = 6·1 grs. Humidity, 62.	0	0	′ ″ −2. 0 +0. 0 5. 0 20. 0 26. 0	° 74 70 66 31 20	1,022·0 1,000·0 921·6 433·4 304·0
2.	330	〃	3,977	1·72	Insulation entirely destroyed.		

The experiments on india-rubber coiled on wires are certainly not favourable to their use. In this experiment, as in the previous one, on cores of this kind, although considerable insulation was exhibited at first, an almost entire loss took place after exposure to pressure. In the case of Messrs. Silver's core, however, this objection does not in all probability apply, as the union between the coils in consequence of the annealing appears to be very complete. In these experiments it is a consideration of some importance to ascertain what relation Messrs. Silver's process of annealamation, wherein the union of the spiral joints is attained by immersion at an increased temperature, bears to Mr. Siemen's method of effecting the same object by pressure, as described in that gentleman's specification.

TABLE X.—CORE of INDIA-RUBBER, prepared by Messrs. SILVER'S process; diameter, ·29 ins.

No. of Experiment.	Duration of Pressure, in Hours.	Hygrometer.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Retention of Charge.	Charge.	Relative Intensity of Charge.
1.	0	Dry bulb, 64°. Wet bulb, 58°. Vapour in a c. ft. of air, = 4·6 grs. Humidity, 67.	0	0	′ ″ 0′. 0 90′. 0 120′. 0 240′. 0 300′. 0 420′. 0	° 80 77½ 76 63 48 36	1,048·0 1,039·2 1,027·6 953·0 740·1 625·2
2.	0	Dry bulb, 62°. Wet bulb, 56°. Vapour in a c. ft. of air, = 4·1 grs. Humidity, 67.	0	0	0′. 0 45′. 0 165′. 0 219′. 0 333′. 0 453′. 0	80 79 77 64 55 36	1,048·0 1,044·2 1,030·6 955·2 871·7 625·2
3.	0	Dry bulb, 62° —64°. Wet bulb, 56½° —53½°. Vapour in a c. ft. of air= 4·3—4·6 grs. Humidity. 74 —89.	0	0	0′. 0 60′. 0 90′. 0 194′. 0 250′. 0 325′. 0 396′. 0	80 73 65 46 36 27 29	1,048·0 1,014·0 977·0 760·0 623·2 482·7 564·0

This core, from its plasticity, was unadapted to the pressure apparatus, and it could not be fixed in the same way as the former cables of gutta-percha; there are, therefore, no experiments on its insulation after being subjected to pressure, although its high insulating power and the perfect union of the coats of india-rubber mark it out favourably as contrasted with other cores.

SUMMARY of the PRECEDING EXPERIMENTS, showing approximately the TIME required in each for a Loss of CHARGE equivalent to a FALL of the ELECTROMETER NEEDLE of 50°.

No. of Experiment.	Description of Core.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Duration of Pressure, in Hours.	Time required for loss of 50°.
I. 1, 2. 3. 4, 5.	Gibraltar core, cured by Mackintosh.	10,000 10,000 10,000	4·363 4·363 4·363	〃 328 328 405	〃 135. 20 100. 0 32. 30
II. 1, 2, 3. 4. 5, 6. 7. 8, 9. 10. 11, 12, 13.	Core impregnated with insulating liquid.	0 10,000 10,000 10,000 10,000 10,000 10,000	0 4·363 4·363 4·363 4·363 4·363 4·363	0 24 48 56 27 120 170	6. 20 11. 40 27. 35 13. 0 32. 0 97. 0 105. 0
III. 1, 2.	Wray's core	0	0	0	1,300. 0
IV. 2.	Wray's core	0	0	0	411. 0
V. 2. 3, 4.	Core impregnated with insulating liquid.	10,000 10,000	4·363 4·363	4 10½	68. 30 44. 15

Summary of the Preceding Experiments—*continued*.

No. of Experiment.	Description of Core.	Pressure in lbs. per sq. inch.	Equivalent Column of Water, in Miles.	Duration of Pressure, in Hours.	Time required for loss of 50°.
VI. 1. 2, 3. 5. 6.	Core of 20 alternate coats of gutta-percha and Chatterton's compound.	0 10,000 10,000 10,000	0 4·363 4·363 4·363	0 121 150 170	93. 30 82. 45 113. 0 100. 50
VII. 1. 2.	Core of pure india-rubber.	0 10,000	0 4·363	0 80	488. 0 18. 0
VIII. 1, 2. 3. 4, 5, 6. 7, 8.	Gutta-percha core	0 10,000 10,000 10,000	0 4·363 4·363 4·363	0 264 480 576	4. 30 〃 4. 5 3. 37
IX. 1. 2.	India-rubber core	0 3,977	0 1·72	0 390	28. 0 0 0
X. 1. 2. 3.	Silver's india-rubber core	0 0 0	0 0 0	0 0 0	389. 0 387. 0 382. 0

On a careful inspection of the above summary, it will be seen that a great difference exists in the retentive powers of the different insulators under severe pressure; these anomalies almost defy attempts at comparison. If we take No. 1 the Gibraltar core, cured by Mackintosh, we have, after an immersion of 282 hours, at the enormous pressure of 10,000 lbs. per square inch, a power of retention of 136 minutes; at 325 hours immersion it is reduced to 100 minutes, and at 405 hours it is still farther reduced to 32 minutes, showing that the insulation is very considerably affected when a sufficiently long period of time is allowed for the permeation of the cable. In the next series of experiments on a core impregnated with an insulating liquid, we have totally different results, as there is a steady and progressive gain in the insulating powers of the core. At 24 hours of immersion we have 11 minutes 40 seconds, at 48 hours 27 minutes 25 seconds, and so on, till at 170 hours the charge is retained for a period of 105 minutes. Wray's core was too small to be fixed in the cylinder, but it retained a charge under atmospheric pressure for 1,300 minutes, and hence manifested a superiority to all the other cables tried. In another trial with a larger cable this insulator also gave very satisfactory results. In No. 5 core, of 20 alternate coats of gutta-percha and Chatterton's compound, there are the variable results of an increase in the first five experiments from 43 minutes in 121 hours to 118 minutes in 150 hours; whilst in the sixth experiment the retention after 170 hours' immersion again falls to 100 minutes. These discrepancies are difficult to account for, and a more lengthened series of experiments is required for the attainment of accurate results. No. 6, a core of pure india-rubber, indicated very good insulation before the pressure was applied, but after 80 hours' immersion the insulation was almost entirely destroyed.

The very important question of insulation in deeply submerged cables, is far from having received, as yet, a satisfactory solution. The foregoing experiments are satisfactory, so far as they show approximately the relative porosity of various materials; but they do not point out how we are to obtain an insulator impermeable to water, and at the same time a good non-conductor. This desideratum has yet to be attained, and I would earnestly recommend that a much more extended series of experiments should be instituted under conditions calculated to secure these objects, and to give to the country that security in the interchange of communications which the interests of the community demand.

I have not entered into the question of the strengths, thicknesses, or density of the electric cables intended for submersion at *great* depths, nor have I ventured to give an opinion on the machinery necessary to lay these cables without injury at the bottom of the ocean. The first thing to be done is to determine which is *the best cable*, and having attained this point, the machinery of submersion will be easily devised. One principle is, however, evident, that the conducting wires, whether of copper or other metal, ought to constitute the strength of the cable, unless it is provided with exterior coverings of fibrous material calculated to resist a tensile strain without injury to the central core. The cable should also be from 40 to 50 per cent. heavier than the water, and, with the insulator and its protecting covering of hemp or other material, not to exceed half an inch in diameter. With these precautions, accompanied with principles of construction founded on experimental data, we may reasonably look forward to communications by means of submarine cables, with the four quarters of the globe, as we now have between the capitals of the United Kingdom.

Manchester, April 5, 1860. WM. FAIRBAIRN.

APP. No. 5.

On the permeability of various kinds of insulators for submarine electric cables. By W. Fairbairn F.R.S.

APP. No. 6.

APPENDIX No. 6.

EXPERIMENTS made to determine the INFLUENCE of TEMPERATURE and PRESSURE on various INSULATING MATERIALS under the circumstances in which they are employed in the MANUFACTURE of SUBMARINE CABLES.—By E. G. BARTHOLOMEW.

It having been thought desirable by the Committee that the inductive capacities and insulating properties of wires covered with various insulating materials should be ascertained when subjected to various degrees of temperature, the experiments recorded in the following tables were entrusted to Mr. E. G. Bartholomew.

The wires (with two exceptions hereafter specified) were each one mile in length. These were coiled and placed in a tank, in which the water, at first maintained at the temperature of 32° F. by means of ice, was elevated gradually 10 degrees at a time by the application of heat; the maximum temperature obtained was 92° F. Twenty-one of the wires had the uniform diameter of $\frac{1}{16}$ of an inch, but their insulating coverings of different materials varied from $\frac{9}{32}$nds to $\frac{4}{32}$nds of an inch in thickness. Five other wires, forming part of the series of nine employed to determine the dependence of the charge and insulation on the diameter of the wire and the thickness of the covering, were also experimented upon at different temperatures. The tables besides contain the results of experiments made with two short lengths of wire; one 176 yards in length, covered with a modification of Wray's compound; the other, 440 yards in length, manufactured on Mr. Hearder's plan, with cotton interposed between the external and internal layers of gutta percha.

For the experiments on induction and on the insulation of dynamic electricity, the wires were charged by a Daniell's battery consisting of 128 cells. The galvanometer employed to measure the amounts of charge and discharge, and the loss of insulation for dynamic electricity, had 30,500 coils of copper wire $\frac{1}{110}$th of an inch in diameter covered with silk; the needles, which were suspended by an unspun silk thread, were not astatic, but performed 10 oscillations in from 26 to 27 seconds; the readings in all the experiments were taken on the same side of zero.

For the experiments on the insulation of static electricity 512 cells of Daniell's battery were employed. The instrument made use of to measure the retention of the charges of static electricity was an electrometer on Peltier's construction, which, in order to distinguish it from another employed in experiments recorded elsewhere in this report, will be designated as B.

Five hundred and twelve cells of Daniell's battery were employed to charge the short lengths of wire for the inductive experiments as well as for those on static insulation.

TABLE I.

THIS Table shows the Deflections of the Needle of the Galvanometer on charging and discharging the respective Wires. Four experiments of each kind are recorded, and their means are also given. The numbers placed in the column containing the description of the wires indicate the permanent currents arising from absence of perfect insulation, or from the escape of a very small portion of the charge whilst one end of the wires remain connected with the battery.

TEMPERATURE 32° F.—Swing of Needle, 10 oscillations in 26 seconds.—March 12, 1860.

Description of Wire.	Charge.	Discharge.
	°	°
Chatterton's comp. on wire.	20·7	20·4
Thickness of gut. per. & comp. $\frac{4}{32}$ in.	20·8	20·7
Diameter of copper - $\frac{9}{32}$ in.	20·7	20·6
(Standard always at 32° F.)	20·7	20·5
Earth - 0·2		
	20·7	20·5
Ditto - - No. 1.	21·2	21·0
	21·3	20·5
Earth - 0·4	21·1	20·6
	21·1	20·9
	21·2	20·7
Ditto - - No. 2.	21·0	21·1
	21·1	21·0
Earth - 0·4	21·2	21·0
	21·1	20·5
	21·1	20·9
Ditto - - No. 3.	21·1	20·9
	21·2	21·0
Earth - 0·4	20·9	20·7
	21·2	21·0
	21·1	20·9
Plain gutta percha - - (A.)	20·4	20·1
Thickness of gutta percha $\frac{4}{32}$ in.	20·4	19·8
Diameter of copper - $\frac{9}{32}$ in.	20·5	19·9
(Standard always at 32° F.)	20·6	20·1
Earth - 0·3	20·5	20·0
Ditto - - - - (C.)	20·3	20·2
	20·2	20·5
Earth - nil.	20·4	20·1
	20·3	20·3
	20·3	20·3
Chatterton's comp. on wire and between gutta percha layers. (D.)	20·8	20·6
Thickness of gut. per. & comp. $\frac{4}{32}$ in.	20·7	20·5
Diameter of copper - $\frac{9}{32}$ in.	20·7	21·0
(Standard always at 32° F.)	20·6	20·5
Earth - nil.	20·7	20·7
Ditto - - - - (E.)	20·4	20·5
	20·3	20·2
Earth - nil.	20·5	20·3
	20·4	20·0
	20·4	20·3
Ditto - - - - (F.)	20·1	19·9
	20·3	20·2
Earth - nil.	20·0	19·9
	20·4	20·2
	20·2	20·1

Description of Wire.	Charge.	Discharge.
	°	°
Ratcliffe's gutta percha - - (A.)	17·5	17·1
Thickness of gutta percha $\frac{6}{32}$ in.	17·7	17·2
Diameter of copper - $\frac{9}{32}$ in.	17·6	17·3
Earth - nil.	17·5	17·3
	17·6	17·2
Ditto - - - - (B.)	16·8	16·7
	17·1	17·2
Earth - 0·1	17·1	17·1
	17·1	16·8
	17·0	16·9
New gutta percha - - (A.)	16·8	16·5
Thickness of gutta percha $\frac{6}{32}$ in.	16·8	16·7
Diameter of copper - $\frac{9}{32}$ in.	16·9	16·5
Earth - 0·4	16·5	16·3
	16·8	16·6
Ditto - - - - (B.)	15·8	15·7
	15·8	15·8
Earth - 0·2	15·6	15·7
	15·9	15·6
	15·8	15·7
Ditto - - - - (C.)	11·0	10·6
Thickness of gutta percha $\frac{6}{32}$ in.	10·6	10·7
Diameter of copper - $\frac{9}{32}$ in.	10·7	10·5
Earth - 0·3	10·8	10·6
	10·8	10·6
Godefroy's compound.	22·5	22·3
Thickness of compound $\frac{6}{32}$ in.	22·5	22·2
Diameter of copper - $\frac{9}{32}$ in.	22·5	22·3
Earth - 0·6	22·3	22·3
	22·5	22·3
Gut. per. & Chatterton's comp., 10 layers of each, alternate.	14·4	14·3
	14·4	14·3
Thickness of combined layers $\frac{6}{32}$ in.	14·4	14·5
Diameter of copper - $\frac{9}{32}$ in.	14·0	14·2
Earth - 0·3	14·8	14·3
Hughes' fluid between gutta percha layers.	14·8	14·9
	14·9	14·9
Thickness together $\frac{6}{32}$ in.	14·8	14·8
Diameter of copper $\frac{9}{32}$ in.	14·5	14·9
Earth - 0·2.	14·7	14·9
India rubber; Hall & Wells (A.)	20·5	20·0
Thickness of rubber $\frac{4}{32}$ in.	20·5	20·2
Diameter of copper $\frac{9}{32}$ in.	20·5	20·5
Earth - 0·3	20·3	20·3
	20·5	20·3
Ditto ditto (B.)	15·5	15·5
Earth - nil.	15·8	15·2
	15·3	15·3
	15·6	15·2
	15·4	15·3

Description of Wire.	Charge.	Discharge.
	°	°
India rubber; Silver's (B.)	11·2	11·1
Thickness of rubber 5·2385/32 in.	11·3	11·2
Diameter of copper - $\frac{9}{32}$ in.	11·2	11·0
Earth - 0·1	11·1	11·3
	11·2	11·2
Ditto ditto - (C.)	9·5	9·4
Thickness of rubber 6·272/32 in.	9·3	9·5
Diameter of copper $\frac{9}{32}$ in.	9·3	9·3
Earth - nil.	9·6	9·6
	9·4	9·5
Wray's compound.	10·0	10·1
Thickness of compound $\frac{6}{32}$ in.	10·3	10·2
Diameter of copper (No. 16 gauge)	9·8	9·7
Earth - 0·1	10·1	9·8
	10·15	9·95
Gutta percha (plain).	20·5	19·6
Thickness of gutta percha $\frac{6}{32}$ in.	20·8	19·8
Diameter of copper - $\frac{9}{32}$ in.	20·4	19·4
Earth - 0·2	20·7	19·8
	20·5	19·7
Ditto.	14·6	14·4
Thickness of gutta percha $\frac{6}{32}$ in.	15·0	14·2
Diameter of copper - $\frac{9}{32}$ in.	15·0	14·1
Earth - 0·4	14·6	14·1
	14·8	14·2
Ditto.	12·3	12·2
Thickness of gutta percha $\frac{13}{32}$ in.	12·4	12·3
Diameter of copper - $\frac{9}{32}$ in.	12·0	12·3
Earth - 0·2	12·3	12·4
	12·2	12·3
Plain gutta percha.	22·2	22·0
Thickness of gutta percha $\frac{6}{32}$ in.	22·5	21·7
Diameter of copper - $\frac{9}{32}$ in.	22·5	22·0
	22·4	22·2
	22·4	22·0
Ditto.	33·6	33·6
Thickness of gutta percha $\frac{6}{32}$ in.	33·7	33·4
Diameter of copper - $\frac{9}{32}$ in.	33·5	33·4
	33·7	33·4
	33·6	33·5
Wray's compound - 176 yards.	This wire had not arrived at this time.	
Hearder's wire, 440 yards.	5·9	5·7
Thickness of insulating material (fibre between gut. per. layers) $\frac{4}{32}$ in.	6·0	6·0
	6·1	6·0
Diameter of copper - $\frac{9}{32}$ in.	6·1	6·1
Earth - 0·1	6·0	6·0

Experiments made to determine the Influence of Temperature, &c.—By E. O. Bartholomew.—Table I.—*continued*.

TEMPERATURE 42° F.

Swing of Needle, 10 oscillations in 26 seconds.—March 22, 1860.

Description of Wire.	Charge.	Discharge.
	°	°
Chatterton's compo on wire.	19·7	18·8
Thickness of gut. per. & comp. $\frac{3}{32}$ in.	19·4	19·2
Thickness of copper wire - $\frac{1}{16}$ in.	19·5	19·1
	19·3	19·4
Earth - nil.		
	19·4	19·3
Ditto - - No. 1.	20·5	19·3
	20·3	19·5
Earth - 1°·2	20·3	19·4
	20·1	19·7
	20·3	19·5
Ditto - - No. 2.	19·8	19·7
	19·8	19·7
Earth - °·5	20·0	19·5
	19·8	19·7
	19·8	19·6
Ditto - - No. 3.	20·1	19·5
	20·0	19·5
Earth - °·1	19·6	19·4
	19·9	19·4
	19·9	19·5
Plain gutta percha - - (A.)	19·1	18·8
Thickness of gutta percha $\frac{3}{32}$ in.	18·9	19·0
Diameter of copper - - $\frac{1}{16}$ in.	19·2	18·7
	19·4	18·9
Earth - °·1		
	19·1	18·8
Ditto - - (C.)	19·2	18·5
	18·6	18·7
Earth - °·4	18·9	18·7
	18·6	18·8
	18·8	18·7
Chatterton's compound on wire and between gut. per. layers (B.)	19·2	19·0
Thickness of comp. & gut. per. $\frac{3}{32}$ in.	19·3	19·2
Diameter of copper - - $\frac{1}{16}$ in.	19·1	19·1
	19·2	19·2
Earth - °·1		
	19·2	19·1
Ditto - - (E.)	19·2	18·7
	19·1	18·6
Earth - nil.	19·0	18·6
	18·9	18·7
	19·1	18·6
Ditto - - (F.)	18·9	18·9
	18·7	19·0
Earth - °·1	18·9	18·9
	18·8	19·0
	18·8	18·9
Ratcliffe's gutta percha - (A.)	16·5	16·3
Thickness of gutta percha - $\frac{3}{32}$ in.	16·4	16·0
Diameter of copper - - $\frac{1}{16}$ in.	16·4	16·1
Earth - °·3	16·3	16·0
	16·4	16·1

Description of Wire.	Charge.	Discharge.
	°	°
Ratcliffe's gutta percha - (B.)	15·8	15·7
Thickness of gutta percha - $\frac{3}{32}$ in.	15·8	15·7
Diameter of copper - - $\frac{1}{16}$ in.	16·0	15·6
	15·8	15·7
Earth - °·3		
	15·9	15·7
New gutta percha - - (A.)	15·1	15·2
Thickness of gutta percha - $\frac{3}{32}$ in.	15·4	15·4
Diameter of copper - - $\frac{1}{16}$ in.	15·1	15·3
Earth - nil.	15·1	15·0
	15·2	15·2
Ditto - - (B.)	14·8	14·8
	14·7	14·9
Earth - nil.	15·0	14·8
	14·6	14·7
	14·8	14·8
Ditto - - (C.)	10·0	9·9
	9·9	9·9
Earth - nil.	10·0	9·9
Thickness of gutta percha - $\frac{3}{32}$ in.	9·8	10·0
Diameter of copper - - $\frac{1}{16}$ in.	9·9	9·9
Godefroy's compound - -	21·9	20·9
Thickness of compound - $\frac{3}{32}$ in.	21·1	21·0
Diameter of copper - - $\frac{1}{16}$ in.	21·2	20·9
Earth - °·4	21·1	20·9
	21·2	20·9
Gutta percha and Chatterton's comp., 10 layers of each, alternate thickness of combined layers - - $\frac{3}{32}$ in.	13·5	13·1
	13·4	13·4
Diameter of copper - - $\frac{1}{16}$ in.	13·4	13·3
Earth - nil.	13·3	13·4
	13·4	13·3
Hughes' fluid between gutta percha layers.	14·0	13·8
Thickness together - $\frac{3}{32}$ in.	14·3	14·0
Diameter of copper - $\frac{1}{16}$ in.	14·1	14·0
Earth - nil.	13·9	13·8
	14·1	13·9
Indian rubber, Hall & Wells (A.)	19·8	19·8
Thickness of rubber - $\frac{3}{32}$ in.	20·0	19·8
Diameter of copper - $\frac{1}{16}$ in.	19·9	20·0
Earth - °·1	20·0	19·8
	19·9	19·9
Ditto - - (B.)	14·5	14·7
	14·5	14·8
Earth - °·2	14·9	14·9
	14·8	14·7
	14·8	14·8
Ditto - - Silver's (B.)	10·8	10·4
Thickness of rubber 5·238532 in.	10·6	10·7
Diameter of copper - $\frac{1}{16}$ in.	10·6	10·7
Earth - nil.	10·8	10·6
	10·7	10·6

Description of Wire.	Charge.	Discharge.
	°	°
Indian rubber - Silver's (C.)	9·4	9·5
Thickness of rubber 5·22432 in.	9·1	9·3
Diameter of copper - $\frac{1}{16}$ in.	9·5	9·4
Earth - nil.	9·3	9·5
	9·4	9·4
Wray's compound.	10·6	10·6
Thickness of compound - $\frac{1}{32}$ in.	10·5	10·6
Diameter of copper, No. 16 gauge	10·5	10·5
Earth - nil.	10·7	10·6
	10·6	10·6
Plain gutta percha.	19·8	19·5
Thickness of gutta percha $\frac{3}{32}$ in.	20·1	19·6
Diameter of copper - $\frac{1}{16}$ in.	19·8	19·6
Earth - °·3	20·2	19·8
	19·7	19·7
Ditto.	14·9	14·7
Thickness of gutta percha $\frac{3}{32}$ in.	13·8	14·8
Diameter of copper - $\frac{1}{16}$ in.	14·8	14·8
Earth - nil.	14·6	14·8
	14·9	14·8
Ditto.	11·7	11·7
Thickness of gutta percha $\frac{1}{32}$ in.	11·8	11·6
Diameter of copper - $\frac{1}{16}$ in.	11·9	11·8
Earth - nil.	11·7	11·6
	11·8	11·7
Ditto.	20·7	20·8
Thickness of gutta percha $\frac{3}{32}$ in.	21·3	21·1
Diameter of copper - $\frac{1}{16}$ in.	21·1	21·1
Earth - nil.	21·0	20·1
	21·0	21·0
Ditto.	31·6	30·9
Thickness of gutta percha $\frac{5}{32}$ in.	31·6	31·1
Diameter of copper - $\frac{1}{16}$ in.	31·7	31·6
Earth - °·5	31·6	31·4
	31·6	31·2
Wray's compound - 176 yards	*4·9	4·5
Earth - nil.	4·8	4·7
	4·7	4·7
* With 512 cells.	4·8	4·7
	4·8	4·7
Hearder's wire - 440 yards	6·6	6·2
Thickness of insulating material, fibre between gutta percha layers - - $\frac{1}{32}$ in.	6·7	6·4
	6·7	6·3
Diameter of copper - $\frac{1}{16}$ in.	6·4	6·4
Earth - nil.	6·6	6·3

APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By E. C. Bartholomew.—Table I.—*continued.*

TEMPERATURE 52° F.

Swing of Needle, 10 oscillations in 27 seconds.—March 30, 1860.

Description of Wire.	Charge.	Discharge.
	°	°
Chatterton's compound on wire.	20·2	19·9
Thickness of gut. per. & comp. $\frac{3}{32}$ in.	20·5	20·0
Thickness of copper wire - $\frac{3}{32}$ in.	20·3	20·3
	20·2	20·3
Earth - 0·2		
	20·3	20·1
Ditto - - No. 1.	21·3	20·2
Earth - 1°·2	21·1	20·3
	21·2	20·4
	21·2	20·4
	21·2	20·3
Ditto - - No. 2.	20·8	20·3
	20·8	20·3
Earth - 0·5	20·7	20·5
	20·7	20·5
	20·8	20·4
Ditto - - No. 3.	20·9	20·9
	21·2	20·8
Earth - 0·7	20·9	20·6
	21·2	20·6
	21·0	20·7
Plain gutta percha - - (A.)	20·0	19·9
Thickness of gutta percha $\frac{3}{32}$ in.	20·2	20·1
Diameter of copper - $\frac{3}{32}$ in.	20·0	20·1
	20·3	19·8
Earth - 0·4		
	20·1	20·0
Ditto - - - (C.)	20·0	19·8
	20·1	19·8
Earth - 5°	20·0	19·8
	20·1	19·8
	20·1	19·6
Chatterton's comp. on wire and between gutta percha layers. (D.)	20·4	20·3
Thickness of comp. & gut. per. $\frac{3}{32}$ in.	20·3	20·2
Diameter of copper - $\frac{3}{32}$ in.	20·4	20·2
	20·4	20·0
Earth - 0·2		
	20·4	20·2
Ditto - - - (E.)	19·8	19·8
	20·1	19·7
Earth - 0·3	20·2	20·0
	20·0	19·8
	20·0	19·8
Ditto - - - (F.)	20·1	19·9
	20·0	20·0
Earth - nil.	20·0	20·0
	20·2	19·8
	20·1	19·9
Ratcliffe's gutta percha - (A.)	17·6	16·8
Thickness of gutta percha $\frac{3}{32}$ in.	17·3	17·2
Diameter of copper - $\frac{3}{32}$ in.	17·6	17·3
	17·3	16·9
Earth - 0·7	17·4	17·0

Description of Wire.	Charge.	Discharge.
	°	°
Ratcliffe's gutta percha - (B.)	17·2	16·6
Thickness of gutta percha $\frac{3}{32}$ in.	16·8	16·7
Diameter of copper - $\frac{3}{32}$ in.	17·0	16·7
	16·9	16·5
Earth - 0·8		
	16·9	16·6
New gutta percha - (A.)	16·0	16·1
Thickness of gutta percha $\frac{3}{32}$ in.	16·4	16·1
Diameter of copper - $\frac{3}{32}$ in.	16·8	16·2
	16·4	16·1
Earth - nil.		
	16·3	16·1
Ditto - - - (B.)	15·8	15·8
	15·7	15·6
Earth - nil.	15·9	15·8
	16·1	16·0
	15·9	15·8
Ditto - - - (C.)	10·7	10·8
	10·9	10·6
Earth - nil.	10·8	10·8
Thickness of gutta percha $\frac{3}{32}$ in.	10·9	10·7
Diameter of copper - $\frac{3}{32}$ in.	10·8	10·7
	22·8	22·3
Godefroy's compound.	22·8	22·4
of compound $\frac{3}{32}$ in.	22·8	22·5
Diamater of copper - $\frac{1}{16}$ in.	22·6	22·5
Earth - 0·6		
	22·8	22·4
Gutta percha and Chatterton's compound, 10 layers of each, alternate thickness of combined layers - $\frac{3}{32}$ in.	14·4	14·2
	14·5	14·4
	14·4	14·4
Diameter of copper $\frac{1}{32}$ in.	14·6	14·5
Earth - nil.	14·5	14·4
Hughes' fluid between gutta percha layers, thickness together $\frac{3}{32}$ in.	15·4	15·1
	15·5	15·1
Diameter of copper - $\frac{3}{32}$ in.	15·4	15·0
Earth - 0·3	15·5	15·1
	15·4	15·1
Indian rubber. Hall and Wells (A.)	22·4	22·4
Thickness of rubber $\frac{3}{32}$ in.	22·6	22·4
Diameter of copper $\frac{3}{32}$ in.	22·5	22·5
Earth - nil.	22·5	22·4•
	22·5	22·4
Do. do. - - (B.)	16·3	16·1
	16·1	16·2
Earth - nil.	16·3	16·1
	16·4	16·2
	16·3	16·2
Ditto Silver's - - (B.)	11·3	11·2
Thickness of rubber 5·2385/32 in.	11·2	11·2
Diameter of copper - $\frac{3}{32}$ in.	11·3	11·3
Earth - nil.	11·2	11·3
	11·2	11·2

Description of Wire.	Charge.	Discharge.
	°	°
Indian rubber, Silvers - - (G.)	10·1	10·1
Thickness of rubber 6·272/32 in.	10·2	10·3
Diameter of copper - $\frac{3}{32}$ in.	10·3	10·2
	10·2	10·0
Earth - nil.		
	10·2	10·2
Wray's compound.	11·4	11·2
Thickness of compound $\frac{3}{32}$ in.	11·5	11·5
Diameter of copper No. 16 gauge.	11·4	11·5
Earth - nil.	11·4	11·4
	11·4	11·4
Plain gutta percha.	21·3	20·6
Thickness of gutta percha $\frac{3}{32}$ in.	21·3	20·6
Diameter of copper - $\frac{3}{32}$ in.	21·1	20·8
Earth - 0·4	21·3	21·2
	21·3	20·8
Ditto.	15·6	15·7
Thickness of gutta percha $\frac{3}{32}$ in.	15·5	15·6
Diameter of copper - $\frac{3}{32}$ in.	15·9	15·5
Earth - nil.	15·5	15·4
	15·6•	15·5
Ditto.	12·4	12·0
Thickness of gutta percha $\frac{1}{32}\frac{6}{2}$ in.	12·3	12·3
Diameter of copper - $\frac{3}{32}$ in.	12·3	12·3
Earth - 0·3	12·3	12·1
	12·3	12·1
Ditto.	22·3	21·8
Thickness of gutta percha $\frac{6}{32}$ in.	22·4	21·8
Diameter of copper - $\frac{4}{32}$ in.	22·4	21·9
Earth - 0·4	22·4	22·0
	22·4	21·9
Ditto.	33·5	32·6
Thickness of gutta percha $\frac{3}{32}$ in.	33·4	32·8
Diameter of copper - $\frac{3}{32}$ in.	33·3	32·7
Earth - 0·6	33·6	32·9
	33·5	32·8
Wray's compound - 176 yds.	*5·4	5·4
Earth - nil.	5·4	5·4
	5·4	5·4
* With 512 cells.	5·4	5·2
	5·4	5·3
Hearder's wire, 440 yards.	†25·0	24·8
Thickness of insulating material (fibre between g. p. layers) $\frac{4}{32}$ in.	24·8	24·6
Diameter of copper $\frac{3}{32}$ in.	24·9	24·8
Earth - 3.	24·8	24·7
(† With 512 cells.)	24·9	24·7

APPENDIX No. 6—*continued.* App. No.

Experiments made to determine the Influence of Temperature, &c.—By E. G. Bartholomew.—Table I.—*continued.*

TEMPERATURE 62° F.

Swing of Needle, 10 oscillations in 26 seconds.—April 1860.

Description of Wire.	Charge.	Discharge.
Chatterton's comp. on wire	19°9	19°3
Thickness of gut. per. & com. $\frac{8}{32}$ in.	19·9	19·2
Diameter of copper - - $\frac{8}{32}$ in.	20·1	19·5
Earth - °·3	20·0	19·5
	20·0	19·4
Ditto - - No. 1.	21·2	20·0
	21·2	20·0
Earth - 1°·5	21·0	19·7
	21·0	20·2
	21·1	20·0
Ditto - - No. 2.	20·5	20·0
	20·4	19·8
Earth - °·6	20·5	19·8
	20·4	19·7
	20·4	19·8
Ditto - - No. 3.	20·6	20·0
	20·4	20·0
Earth - °·5	20·6	19·8
	20·6	20·1
	20·6	20·0
Plain gutta percha - - (A.)	19·3	19·0
Thickness of gutta percha $\frac{8}{32}$ in.	19·6	19·1
Diameter of copper - $\frac{8}{32}$ in.	19·5	19·2
Earth - °·2	19·6	19·1
	19·5	19·1
Ditto - - - (C.)	19·6	19·0
	19·6	19·0
Earth - °·7	19·8	19·0
	19·5	19·0
	19·6	19·0
Chatterton's comp. on wire and between gutta percha layers. (D.)	19·4	19·2
	19·4	19·0
Thickness of compound and gutta percha layers - $\frac{8}{32}$ in.	19·2	19·5
Diameter of copper - $\frac{8}{32}$ in.	19·3	19·5
Earth - °·1	19·3	19·3
Ditto - - - (E.)	19·5	19·0
	19·5	19·0
Earth - °·5	19·7	19·2
	19·5	19·0
	19·5	19·0
Ditto - - - (F.)	19·2	18·8
	19·2	18·9
Earth - °·5	19·4	18·9
	19·1	18·8
	19·2	18·8
Ratcliffe's gutta percha - (A.)	*16·7	16·2
Thickness of gutta percha $\frac{8}{32}$ in.	16·8	16·5
Diameter of copper - $\frac{8}{32}$ in.	16·8	16·5
Earth - °·5	16·4	16·4
	16·7	16·4

Description of Wire.	Charge.	Discharge.
Ratcliffe's gutta percha - (B.)	16°1	15°1
Thickness of gutta percha $\frac{6}{32}$ in.	16·1	15·4
Diameter of copper - $\frac{8}{32}$ in.	16·0	15·6
Earth - °·5	16·1	15·4
	16·1	15·4
New gutta percha - - (A.)	15·6	15·3
Thickness of gutta percha $\frac{6}{32}$ in.	15·6	15·5
Diameter of copper - $\frac{8}{32}$ in.	15·5	15·4
Earth - nil.	15·5	15·5
	15·5	15·4
Ditto - - - (B.)	15·3	15·3
Earth - nil.	15·4	15·1
	15·1	15·0
	15·1	15·0
	15·2	15·1
Ditto - - - (C.)	10·1	10·4
Thickness of gutta percha $\frac{6}{32}$ in.	10·3	10·1
Diameter of copper - $\frac{8}{32}$ in.	10·3	10·2
Earth - nil.	10·3	10·3
	10·3	10·2
Godefroy's compound.	22·0	21·7
Thickness of compound $\frac{3}{16}$ in.	22·0	21·8
Diameter of copper - $\frac{8}{32}$ in.	22·0	21·7
Earth - °·8	22·0	21·8
	22·0	21·7
Gutta percha and Chatterton's compound, 10 layers of each, alternate.	14·0	13·9
Thickness of combined layers $\frac{6}{32}$ in.	13·8	14·0
Diameter of copper - $\frac{8}{32}$ in.	14·0	14·0
Earth - °·1	14·1	13·8
	14·0	13·9
Hughes' fluid between gutta percha layers.	15·3	14·8
	15·5	14·8
Thickness together $\frac{6}{32}$ in.	15·1	14·5
Diameter of copper $\frac{8}{32}$ in.	15·0	14·7
Earth - 1°·0	15·2	14·7
Indian rubber; Hall & Wells (A.)	23·5	23·0
Thickness of rubber $\frac{6}{32}$ in.	23·5	23·2
Diameter of copper $\frac{8}{32}$ in.	23·5	23·4
Earth - °·4	23·4	23·4
	23·5	23·3
Ditto - - (B.)	15·6	15·6
Earth - °·3	15·8	15·6
	15·6	15·6
	15·7	15·5
	15·7	15·6
Indian rubber; Silver's - (B.)	10·6	10·5
Thickness of rubber 5·2385/32 in.	10·6	10·4
Diameter of copper $\frac{8}{32}$ in.	10·6	10·5
Earth - nil.	10·6	10·5

Description of Wire.	Charge.	Discharge.
India rubber; Silver's - (C.)	9°5	9°5
Thickness of rubber 6·272/32 in.	9·6	9·3
Diameter of copper $\frac{8}{32}$ in.	9·5	9·3
Earth - nil.	9·5	9·4
	9·5	9·4
Wray's compound.	11·1	11·0
Thickness of compound - $\frac{4}{32}$ in.	11·2	11·0
Diameter of copper, No. 16 gauge	11·3	11·1
Earth - nil.	11·3	11·0
	11·2	11·0
Plain gutta percha.	20·8	20·0
Thickness of gutta percha - $\frac{4}{32}$ in.	20·8	20·1
Diameter of copper - $\frac{8}{32}$ in.	20·9	20·0
Earth - °·8	20·8	20·1
	20·8	20·0
Ditto.	15·3	15·1
Thickness of gutta percha - $\frac{4}{32}$ in.	15·5	14·8
Diameter of copper - $\frac{8}{32}$ in.	15·4	14·9
Earth - 1°·0	15·5	14·9
	15·4	14·9
Ditto.	12·4	12·0
Thickness of gutta percha - $\frac{12}{32}$ in.	12·2	11·8
Diameter of copper - - $\frac{8}{32}$ in.	12·2	12·0
Earth - °·3	12·3	12·0
	12·3	12·0
Ditto.	22·3	21·2
Thickness of gutta percha - $\frac{8}{32}$ in.	22·3	21·5
Diameter of copper - - $\frac{8}{32}$ in.	22·3	21·4
Earth - °·9	22·4	21·4
	22·3	21·4
Ditto.	33·4	32·0
Thickness of gutta percha - $\frac{8}{32}$ in.	33·0	32·1
Diameter of copper - - $\frac{8}{32}$ in.	33·0	32·0
Earth - 1°·0	33·0	32·0
	33·0	32·0
Wray's compound 176 yards.	*5·2	5·2
Earth - nil.	5·1	5·2
* With 512 cells.	5·1	5·0
	5·1	5·1
Hearder's wire, 440 yards.	*24·0	23·0
Thickness of insulating material - - $\frac{4}{32}$ in.	23·8	23·0
Diameter of copper - $\frac{8}{32}$ in.	24·0	23·0
* With 512 cells.	23·9	23·2
Earth - °·1	23·9	23·0

APPENDIX No. 6.—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By E. G. Bartholomew.—Table I.—*continued.*

TEMPERATURE 72° F.

Swing of Needle, 10 oscillations in 27 seconds.—April 13, 1860.

Description of Wire.	Charge.	Discharge.
	°	°
Chatterton's comp. on wire.	19·5	19·3
Thickness of gut. per. & comp. $\frac{1}{32}$ in.	19·6	19·3
Diameter of copper wire - $\frac{8}{32}$ in.	19·8	19·4
Earth - ·8	20·2	19·5
	19·8	19·4
Ditto - - No. 1.	21·8	20·5
	22·0	20·5
Earth - 2°·6	21·7	20·8
	22·0	20·5
	21·9	20·6
Ditto - - No. 2.	21·0	20·5
	21·0	20·5
Earth - 1°·2	21·0	20·4
	21·1	20·5
	21·0	20·5
Ditto - - No. 3	20·9	20·5
	21·3	20·6
Earth - 1°·5	21·1	20·6
	21·3	20·7
	21·2	20·6
Plain gutta percha - - (A.)	20·0	19·9
Thickness of gutta percha - $\frac{8}{32}$ in.	20·1	19·5
Diameter of copper - $\frac{8}{32}$ in.	20·0	19·5
	20·0	19·5
Earth - ·5	20·0	19·6
Ditto - - (C.)	20·6	19·7
	20·4	19·9
Earth - 1°·6	20·7	19·7
	20·7	19·6
	20·6	19·7
Chatterton's comp. on wire, and between gutta percha layers (D.)	20·2	19·7
	20·2	19·5
Thickness of gutta percha and compound - - $\frac{8}{32}$ in.	20·3	19·5
Diameter of copper - $\frac{8}{32}$ in.	20·3	19·6
Earth - ·7	20·2	19·6
	20·4	19·6
Ditto - - (E.)	20·2	19·6
Earth - 1°·1	20·1	19·4
	20·1	19·5
	20·2	19·5
Ditto - - (F.)	20·2	19·5
	20·2	19·3
Earth - 1°·0	20·0	19·5
	20·1	19·5
	20·1	19·5
Ratcliffe's gutta percha - (A.)	17·8	16·7
Thickness of gutta percha - $\frac{8}{32}$ in.	17·7	16·9
Diameter of copper - $\frac{8}{32}$ in.	17·8	17·1
Earth - 1°·4	17·8	17·1
	17·8	17·0

Description of Wire.	Charge.	Discharge.
	°	°
Ratcliffe's gutta percha - (B.)	17·0	16·3
Thickness of gutta percha $\frac{1}{32}$ in.	17·1	16·4
Diameter of copper - $\frac{8}{32}$ in.	17·1	16·4
Earth - 1°·2	17·2	16·4
	17·1	16·4
New gutta percha - - (A.)	15·8	15·4
Thickness of gutta percha $\frac{8}{32}$ in.	15·8	15·5
Diameter of copper - $\frac{8}{32}$ in.	15·6	15·7
Earth - ·4°	15·7	15·5
	15·7	15·5
Ditto - - (B.)	15·4	15·3
	15·4	15·1
Earth - ·0°2	15·2	15·0
	15·4	15·3
	15·4	15·2
Ditto - - (C.)	10·5	10·6
	10·6	10·5
Thickness of gutta percha $\frac{8}{32}$ in.	10·6	10·5
Diameter of copper - $\frac{8}{32}$ in.	10·5	10·5
Earth - nil.	10·6	10·5
	14·5	14·5
Gutta percha and Chatterton's compound, 10 layers of each alternate.	14·6	14·5
	14·6	14·4
Thickness of combined layers $\frac{8}{32}$ in.	14·6	14·4
Diameter of copper - $\frac{8}{32}$ in.		
Earth - ·0°2	14·6	14·5
Hughes' fluid between gutta percha layers.	16·8	15·3
	16·8	15·2
Thickness together - $\frac{8}{32}$ in.	16·7	15·3
Diameter of copper - $\frac{8}{32}$ in.	16·6	15·1
Earth - ·0°2	16·7	15·2
India rubber, Hall & Wells (A.)	26·0	25·4
Thickness of rubber - $\frac{8}{32}$ in.	26·1	25·5
Diameter of copper - $\frac{8}{32}$ in.	26·0	25·6
Earth - ·0·4	26·1	25·6
	26·1	25·5
Ditto - - - (B.)	17·0	16·8
	16·9	16·8
Earth - ·0·1	17·2	17·1
	17·3	17·1
	17·1	17·0
Ditto, Silver's - (B.)	10·6	10·8
Thickness of rubber 5·2385/32 in.	10·5	10·4
Diameter of copper - $\frac{8}{32}$ in.	10·5	10·5
Earth - nil.	10·7	10·6
	10·6	10·6
India rubber, Silver's - - (C.)	9·4	9·6
Thickness of rubber 6·272/32 in.	9·7	9·5
Diameter of copper - $\frac{8}{32}$ in.	9·6	9·6
Earth - nil.	9·6	9·5
	9·6	9·6

Description of Wire.	Charge.	Discharge.
	°	°
Godefroy's compound.	23·5	22·2
Thickness of compound - $\frac{8}{32}$ in.	23·8	22·1
Diameter of copper - $\frac{8}{32}$ in.	23·6	22·2
Earth - 2°·3	23·6	22·3
	23·6	22·2
Wray's compound.	10·8	10·8
Thickness of compound - $\frac{8}{32}$ in.	11·2	11·0
Diameter of copper, No. 16 gauge.	11·1	11·0
Earth - nil.	11·1	10·9
	11·0	10·9
Plain gutta percha.	21·7	20·5
Thickness of gutta percha $\frac{8}{32}$ in.	21·8	20·4
Diameter of copper - $\frac{8}{32}$ in.	21·7	20·5
Earth - 2°·1	21·9	20·5
	21·8	20·5
Ditto.	16·2	15·3
Thickness of gutta percha $\frac{8}{32}$ in.	16·3	15·2
Diameter of copper - $\frac{8}{32}$ in.	16·2	15·2
Earth - 1°·5	16·2	15·3
	16·2	15·2
Ditto.	13·0	12·1
Thickness of gutta percha $\frac{12}{32}$ in.	12·9	12·0
Diameter of copper - $\frac{8}{32}$ in.	13·0	12·2
Earth - 1°·6	13·1	12·0
	13·0	12·1
Ditto.	24·0	22·1
Thickness of gutta percha $\frac{8}{32}$ in.	23·9	22·3
Diameter of copper - $\frac{8}{32}$ in.	24·0	22·2
Earth - 2°·1	24·1	22·3
	24·0	22·2
Ditto.	34·8	33·2
Thickness of gutta percha $\frac{8}{32}$ in.	34·7	33·0
Diameter of copper - $\frac{8}{32}$ in.	34·9	33·1
Earth - 2°·2	34·9	33·1
	34·8	33·1
Wray's compound - 176 yards.	*4·8	*5·0
Earth - nil.	4·9	5·0
* With 512 cells.	5 0	5·0
	5·0	4·9
	4·9	5·0
Hearder's wire, 440 yards.	†24·8	†24·0
Thickness of insulating material (fibre between gutta percha layers) - - - $\frac{8}{32}$ in.	24·9	24·0
	23·0	24·1
Diameter of copper - $\frac{8}{32}$ in.	24·9	24·1
Earth - 1°·2	24·9	24·1
† With 512 cells.		

APPENDIX No. 6.—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By E. C. Bartholomew.—Table I.—*continued.*

TEMPERATURE 82° F.

Swing of Needle, 10 oscillations in 26·5 seconds.—April 19, 1860.

Description of Wire.	Charge.	Discharge.
Chatterton's compound on wire.	20°0	19°8
Thickness of gut. per. & comp. $\frac{5}{32}$ in.	20·0	20·1
Diameter of copper wire - $\frac{3}{32}$ in.	20·8	20·1
	20·0	20·0
Earth - 0·2		
	20·0	20·0
Ditto - - No. 1.	23·4	21·7
	23·2	21·4
Earth - 2°·8	23·4	21·5
	23·4	21·7
	23·4	21·6
Ditto - - No. 2.	22·4	21·4
	22·5	21·6
Earth - 1°·7	22·4	21·6
	22·5	21·4
	22·4	21·5
Ditto - - No. 3.	22·8	21·4
	22·9	21·5
Earth - 2°·3	23·0	21·5
	22·9	21·5
	22·9	21·5
Plain gutta percha - - (A.)	20·0	19·7
Thickness of gutta percha $\frac{3}{32}$ in.	20·0	19·6
Diameter of copper - $\frac{3}{32}$ in.	20·0	19·6
	20·1	19·7
Earth - 0·4		
	20·0	19·6
Ditto - - - (C.)	22·8	21·2
	23·0	20·8
Earth - 3°·0	22·5	20·5
	22·7	20·9
	22·7	20·8
Chatterton's comp. on wire and between gutta percha layers (D.)	21·8	20·5
	21·8	20·5
Thickness of gut. per. & comp. $\frac{5}{32}$ in.	21·4	20·0
Diameter of copper - $\frac{3}{32}$ in.	21·4	20·3
Earth - 2°·0		
	21·6	20·3
Ditto - - - (E.)	21·7	20·0
	21·4	20·2
Earth - 1°·9	21·4	20·1
	21·6	20·1
	21·5	20·1
Ditto - - - (F.)	21·5	20·1
	21·8	20·1
Earth - 2°·0	21·7	20·0
	21·8	20·2
	21·7	20·1
Ratcliffe's gutta percha - (A.)	19·4	17·4
Thickness of gutta percha $\frac{3}{32}$ in.	19·2	17·3
Diameter of copper - $\frac{3}{32}$ in.	19·4	17·3
	19·3	17·3
Earth - 2°·2		
	19·3	17·3

Description of Wire.	Charge.	Discharge.
Ratcliffe's gutta percha - (B.)	18°0	16°7
Thickness of gutta percha $\frac{5}{32}$ in.	18·1	16·9
Diameter of copper - - $\frac{3}{32}$ in.	18·1	16·9
	18·0	17·0
Earth - 1°·8		
	18·0	16·9
New gutta percha - (A.)	15·8	15·5
Thickness of gutta percha $\frac{3}{32}$ in.	15·8	15·5
Diameter of copper - $\frac{3}{32}$ in.	15·8	15·5
Earth - 0·5	15·7	15·5
	15·8	15·5
Ditto - - - (B.)	15·7	15·5
	15·5	15·4
Earth - 0·2	15·5	15·4
	15·6	15·4
	15·6	15·4
Ditto - - - (C.)	10·5	10·7
	10·6	10·5
Earth - nil.	10·5	10·6
	10·6	10·5
	10.5	10·6
Gutta per. and Chatterton's comp., 10 layers of each alternate.	15·3	14·5
	15·3	14·7
Thickness of combined layers $\frac{5}{32}$ in.	15·3	14·7
Diameter of copper - $\frac{3}{32}$ in.	15·4	14·5
Earth - 0·6		
	15·3	14·6
Hughes' fluid between gutta percha layers.	19·7	16·7
	19·7	17·0
Thickness together - $\frac{5}{32}$ in.	19·8	16·8
Diameter of copper - $\frac{3}{32}$ in.	19·8	16·8.
Earth - 4°·2	19·7	16·8
India rubber, Hall & Wells (A.)	28·0	27·8
Thickness of rubber - $\frac{5}{32}$ in.	28·0	27 5
Diameter of copper - $\frac{3}{32}$ in.	28·0	27·5
Earth - 1°·3	27·9	27·4
	28·0	27·5
Ditto - - - (B.)	19·1	18·9
	19·2	18·7
	19·0	18·7
Earth - 0·3	19·2	18·8
	19·1	18·8
Ditto - - Silver's (B.).	10·8	11·0
Thickness of rubber 5·2385/32 in.	10·7	10·9
Diameter of copper - $\frac{3}{32}$ in.	10·8	10·9
Earth - 0·1	10·8	10·9
	10·8	10·9
India rubber, Silver's - (C.)	10·1	10·0
Thickness of rubber 6·272/32 in.	10·0	10·1
Diameter of copper - $\frac{3}{32}$ in.	10·0	10·0
Earth - nil.	10·0	10·0
	10·0	10·0

Description of Wire.	Charge.	Discharge.
Godefroy's compound - -	26°1	23°0
Thickness of compound - $\frac{3}{32}$ in.	25·9	23·5
Diameter of copper - $\frac{3}{32}$ in.	26·0	23·5
Earth - 4°·2	26·0	23·3
	26·0	23·3
Wray's compound - - -	12·0	11·9
Thickness of compound - $\frac{3}{32}$ in.	12·1	11·8
Diameter of copper, No. 16 gauge	12·0	11·8
Earth - 0·1	12·1	11·9
	12·0	11·8
Plain gutta percha - - -	24·3	21·4
Thickness of gutta percha $\frac{3}{32}$ in.	24·1	21·8
Diameter of copper - - $\frac{3}{32}$ in.	24·0	21·3
Earth - 3°·9	24·1	21·3
Ditto.	17·2	15·7
Thickness of gutta percha $\frac{5}{32}$ in.	17·4	15·5
Diameter of copper - $\frac{3}{32}$ in.	17·4	15·4
Earth - 2°·5	17·2	15·6
	17·3	15·5
Ditto.	14·2	12·5
Thickness of gutta percha $\frac{12}{32}$ in.	14·3	12·6
Diameter of copper - $\frac{3}{32}$ in.	14·3	12·5
Earth - 2°·5	14·3	12·4
	14·3	12·5
Ditto.	27·3	24·3
Thickness of gutta percha $\frac{5}{32}$ in.	27·4	24·5
Diameter of copper - $\frac{3}{32}$ in.	27·3	24·4
Earth - 4°·8	27·3	24·4
	27·3	24·4
Ditto.	37·6	34·6
Thickness of gutta percha $\frac{5}{32}$ in.	37·6	34·8
Diameter of copper - $\frac{3}{32}$ in.	37·3	34·8
Earth - 4°·4	37·7	34·7
	37·6	34·7
Wray's compound - 175 yards	5·7	5·5
Earth - nil.	5·6	5·5
* With 512 cells.	5·6	5·5
	5·6	5·5
Hearder's wire - 440 yards	27·0	25·3
Thickness of insulating material, (fibre between gutta percha layers) - - - $\frac{5}{32}$ in.	27·0	25·5
	26·9	25·5
Diameter of copper - $\frac{3}{32}$ in.	26·9	25·4
Earth - 2°·8	27·0	25·4
* With 512 cells.		

APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By E. G. Bartholomew.—Table I.—*continued.*

TEMPERATURE 32° F.

Swing of Needle, 10 oscillations in 26·5 seconds.—April 25, 1860.

Description of Wire.	Charge.	Discharge.
	°	°
Chatterton's comp. on wire.	20·7	20·2
Thickness of gut. per. & comp. $\frac{8}{32}$ in.	21·3	20·0
Diameter of copper wire - $\frac{3}{32}$ in.	21·1	20·2
	21·2	20·1
Earth - 0·9		
	21·1	20·1
Ditto - - No. 1.	23·7	23·8
	23·6	23·9
Earth - 6°·6	23·6	23·9
	23·7	23·9
	23·7	23·9
Ditto - - No. 2.	25·5	22·5
	25·0	22·5
Earth - 4°·2	25·1	22·6
	25·0	22·5
	25·1	22·5
Ditto - - No. 3.	25·4	22·5
	25·1	22·5
Earth - 4°·4	25·2	22·5
	25·2	22·5
Thickness of gutta percha - $\frac{8}{32}$ in.		
Diameter of copper - - $\frac{3}{32}$ in.	25·2	22·5
Plain gutta percha - (A.)	21·1	19·9
Thickness of gutta percha - $\frac{8}{32}$ in.	21·1	20·0
Diameter of copper - $\frac{3}{32}$ in.	21·0	20·0
	21·2	20·1
Earth - 0·7		
	21·1	20·0
Ditto - - (C.)	27·3	23·0
	27·6	23·2
Earth - 6°·0	27·4	23·2
	27·3	23·0
	27·3	23·1
Chatterton's compound on wire and between gut. per. layers (D.)	21·4	20·2
	21·3	20·3
Thickness of gut. per. & comp. $\frac{8}{32}$ in.	21·3	20·3
Diameter of copper - - $\frac{3}{32}$ in.	21·3	20·2
Earth - 0·9	21·3	20·2
Ditto - - (E.)	23·5	21·6
	23·3	21·5
Earth - 3°·0	23·4	21·5
	23·3	21·6
	23·4	21·6
Ditto - - (F.)	23·5	20·5
	23·5	20·3
Earth - 2°·0	23·4	20·3
	23·5	20·3
	23·5	20·3
Ratcliffe's gutta percha - (A.)	19·8	17·8
Thickness of gutta percha - $\frac{8}{32}$ in.	19·8	17·8
Diameter of copper - - $\frac{3}{32}$ in.	19·9	17·8
Earth - 2°·5	19·8	17·9
	19·8	17·8

Description of Wire.	Charge.	Discharge.
	°	°
Ratcliffe's gutta percha - (B.)	19·1	17·1
Thickness of gutta percha - $\frac{8}{32}$ in.	19·0	17·1
Diameter of copper - - $\frac{3}{32}$ in.	19·1	17·1
	19·0	17·2
Earth - 2°·2		
	19·1	17·1
New gutta percha - - (A.)	16·4	15·5
Thickness of gutta percha - $\frac{8}{32}$ in.	16·5	15·7
Diameter of copper - - $\frac{3}{32}$ in.	16·5	15·7
Earth - 1°·0	16·5	15·6
	16·5	15·6
Ditto - - (B.)	15·7	15·4
	15·6	15·5
Earth - 0·1	15·6	15·5
	15·4	15·5
	15·6	15·5
Ditto - - (C.)	10·4	10·3
	10·6	10·3
Earth - nil.	10·4	10·2
Thickness of gutta percha - $\frac{8}{32}$ in.	10·4	10·4
Diameter of copper - - $\frac{3}{32}$ in.	10·4	10·3
Gutta percha and Chatterton's compound, 10 layers of each, alternate.	15·8	15·3
	16·0	15·3
Thickness of combined layers $\frac{32}{32}$ in.	15·9	15·3
Diameter of copper - $\frac{3}{32}$ in.	15·9	15·4
Earth - 1°·0	15·9	15·3
Hughes' fluid between gutta percha layers.	28·3	20·5
	28·3	20·7
Thickness together - $\frac{32}{32}$ in.	28·0	20·5
Diameter of copper - $\frac{3}{32}$ in.	28·5	20·7
Earth - 9°·5	28·3	20·6
India rubber, Hall and Wells (A.)	33·8	30·5
Thickness of rubber - $\frac{3}{32}$ in.	33·7	30·2
Diameter of copper - $\frac{3}{32}$ in.	33·8	30·4
Earth - 4°·9	33·7	30·3
	33·8	30·2
Do. do. - - (B.)	20·7	20·3
	20·7	20·1
Earth - 0°·5	20·6	20·2
	20·6	20·2
	20·7	20·2
Ditto, Silver's - - (B.)	10·8	11·0
Thickness of rubber 5·2383/32 in.	11·0	10·8
Diameter of copper - $\frac{3}{32}$ in.	10·6	10·7
Earth - nil.	10·9	10·8
	10·8	10·8
India rubber, Silver's - (C.)	9·7	9·6
Thickness of rubber 6·272/32 in.	9·7	9·6
Diameter of copper - $\frac{3}{32}$ in.	9·6	9·6
Earth - nil.	9·7	9·6
	9·7	9·6

Description of Wire.	Charge.	Discharge.
	°	°
Godefroy's compound.	28·3	24·6
Thickness of compound $\frac{8}{32}$ in.	28·0	24·3
Diameter of copper - $\frac{3}{32}$ in.	28·2	24·3
	28·0	24·4
Earth - 5°·4		
	28·1	24·4
Wray's compound.	13·2	13·2
Thickness of compound $\frac{8}{32}$ in.	13·0	13·1
Diameter of copper No. 16 gauge	13·2	13·1
	13·2	13·2
Earth - 0·4		
	13·2	13·2
Plain gutta percha.	30·2	23·8
Thickness of gutta percha $\frac{8}{32}$ in.	30·0	24·1
Diameter of copper - $\frac{3}{32}$ in.	30·0	24·0
	30·1	23·9
Earth - 8°·4		
	30·1	23·9
Ditto.	21·5	17·0
Thickness of gutta percha $\frac{8}{32}$ in.	21·1	17·4
Diameter of copper - $\frac{3}{32}$ in.	21·4	17·3
	21·2	17·0
Earth - 5°·4		
	21·3	17·2
Ditto.	18·3	14·2
Thickness of gutta percha $\frac{32}{32}$ in.	18·3	14·2
Diameter of copper - $\frac{3}{32}$ in.	18·2	14·2
	18·4	14·3
Earth - 5°·4		
	18·3	14·2
Ditto.	34·2	28·0
Thickness of gutta percha $\frac{8}{32}$ in.	34·5	28·2
Diameter of copper - $\frac{3}{32}$ in.	34·5	28·1
	34·2	28·1
Earth - 9°·5		
	34·3	28·1
Ditto.	44·0	38·3
Thickness of gutta percha $\frac{8}{32}$ in.	43·6	38·5
Diameter of copper - $\frac{3}{32}$ in.	44·2	38·5
	43·8	38·5
Earth - 9°·7		
	43·9	38·5
Wray's compound - 176 yards.	6·0	6·0
	6·1	6·0
Earth - nil.	6·1	6·1
	6·0	6·0
*With 512 cells.		
	6·0	6·0
Hearder's wire - 440 yards.	29·5	26·2
Thickness of insulating material	29·5	26·4
(fibre between g. p. layers) $\frac{8}{32}$ in.	29·7	26·4
Diameter of copper - $\frac{3}{32}$ in.	29·6	26·4
Earth - 4°·5		
† With 512 cells.	29·6	26·4

APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By E. O. Bartholomew.—*continued.*

TABLE II.

The first column designates the insulating material with which the wire is covered; also its thickness and the diameter of the wire.

The second and third columns show the amount of charge and discharge measured by the galvanometer, 128 cells of Daniell's battery having been employed to charge the wires. Each number is the mean of four readings, which are given in detail in the preceding table.

The fourth column shows the amount of defective insulation for dynamic electricity as shown by the galvanometer. After the strong current occasioned by the charging of the wire has ceased, this weak current exists as long as the wire remains connected with the battery.

The fifth, sixth, seventh, eighth, and ninth columns record the experiments on static electricity made with Peltier's electrometer (B.) To obtain these results, 512 cells of a Daniell's battery were employed.

The fifth, sixth, and seventh columns show the time in which the index of the electrometer fell from the maximum tension given by 512 cells of the battery, respectively, to three-fourths, one-half, and one-quarter of that tension. The positions of the index indicating these reduced tensions were ascertained by charging the wire with 384, 256, and 128 cells of the battery.

The eighth column shows the time required to reduce the tension to one-half, ascertained by a different process, recommended by Mr. Varley. In this method the base of the electrometer is insulated, and is maintained in permanent contact with one-half the battery; the tension of the whole of the battery is then, for a moment, communicated to the stem in connexion with the needle, and the time is counted which the needle takes to descend to zero; when this happens it is obvious that the electricity of the stem is in equilibrium with that of the base, and is therefore equal to half the battery tension. The results obtained by this method differ but slightly from those of the sixth column.

The ninth column shows the number of degrees which the index of the electrometer fell in one minute. For the best insulating substances it has been necessary to take five minutes instead of one.

TEMPERATURE 32° F.

Tension of Battery 65°·75; 66°·5.—March 12–19.

Description of Wire.	Induction. Charge.	Induction. Discharge.	Earth.	Insulation. The index fell from full to 89° or ¾ tension in	Insulation. The index fell from full to 49°·5 or ½ tension in	Insulation. The index fell from full to 32°·25 or ¼ tension in	Insulation. The index fell to zero (⅛ tension) in	Insulation. The index fell in 1 minute to
	°	°	°	Min. Secs.	Min. Secs.		Min. Secs.	°
Chatterton's Compound on Wire	20·7	20·5	0·2	0 13 0 14	0 28		0 26 0 26½	32
Do. - - No. 1.	21·2	20·7	0·4	0 6	0 12 0 12½		0 12 0 12	8 7
Do. - - No. 2.	21·1	20·9	0·4	0 12	0 26		0 28 0 26½	37 36
Do. - - No. 3.	21·1	20·9	0·4	0 15 0 14	0 30½ 0 31		0 32 0 34	44 43½
Plain Gutta Percha - (A.)	20·5	20·0	0·3	0 23	0 51 0 50		0 51 0 52	45 44½
Do. - - (C.)	20·3	20·3	0·0	0 32 0 30	1 16 1 11		1 15 1 21	53½ 52½
Chatterton's Compound on Wire and between Gutta Percha coatings - - - (D.)	20·7	20·7	0·0	0 40	1 18		1 11 1 16	55½ 55
Do. - - (E.)	20·4	20·3	0·0	0 15 0 14	0 30 0 29		0 31 0 30	28
Do. - - (F.)	20·2	20·1	0·0	0 21 0 20	0 41		0 42 0 45	39½ 39
Ratcliffe's Gutta Percha - (A.)	17·6	17·2	0·0	0 10 0 11	0 21 0 25 0 28		0 26 0 28	28½ 33 35½
Do. - do. (B.)	—	—	0·1					
Special Gutta Percha - (A.)	16·8	16·6	0·4	1 57 2 11	3 55 4 33		3 50 3 51	42 47 in 5′
Do. - - - (B.)	15·8	15·7	0·2	0 35 0 38	1 22 1 28		1 30 1 36	54½ 55
Do. - - - (C.)	10·8	10·6	0·3	6 0 7 25	17 30		20 15	62½ in 5′
Godefroy's Compound - -	22·5	22·3	0·6	0 13	0 33 0 36		0 39 0 42	38½ 40½
Gutta Percha and Chatterton's Compound, 10 layers of each, alternate - - -	14·3	14·3	0·3	5 0 6 23	- -		- -	60 61 in 5′
Hughes's fluid between Gutta Percha layers - - -	14·7	14·9	0·2	1 20 1 15	3 16 3 25		3 58 3 39	60 61½
Indian Rubber, Hall & Wells (A.)	20·5	20·3	0·3	0 58 1 2	2 26 2 30		2 30 2 28	60 60½
Do. - do. (B.)	15·4	15·3	0·0	0 43 0 41	1 25 1 30		1 30	56
Do. Silver's - (B.)	11·2	11·2	0·1	2 30 2 45	5 50 5 45		- -	54½ in 5′
Do. - do. (C.)	9·4	9·5	0·0	3 20 3 0	7 45 7 20		- -	56 in 5′
Wray's Compound - -	10·15	9·95	0·1	60 0.	- -		- -	60 in 60′
Gutta Percha $\frac{4}{32}$ in., copper $\frac{8}{32}$ in.	20·5	19·7	0·2	0 17	0 35 0 36		0 37	34½ 36½
Do. $\frac{4}{32}$ do. $\frac{9}{32}$ -	14·8	14·2	0·4	1 20 1 32	3 20 3 46		4 8 4 7	62½
Do. $\frac{12}{32}$ do. $\frac{8}{32}$ -	12·2	12·3	0·2	0 28 0 35	1 3 1 15		1 22	51 54
Do. $\frac{4}{32}$ do. $\frac{4}{32}$ -	22·4	22·0	—	0 55 1 0	2 7 2 44		3 16 3 5	59½ 60
Do. $\frac{4}{32}$ do. $\frac{4}{32}$ -	33·8	33·5	—	0 46 0 43	1 39 1 45		2 0 2 3	58½ 57½
Hearder's Wire (440 yards) -	6·0	6·0	0·1	1 20 1 55	3 55 4 45		- -	48½ in 5′

App. No. 6.

Appendix No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By E. G. Bartholomew.—Table II.—*cont.*

TEMPERATURE 42° F.

Swing of Needle, 10 oscillations in 26 seconds; Full Tension of Battery 65°.—March 24, 1860.

Description of Wire.	Induction. Charge.	Induction. Discharge.	Earth.	Insulation. The index fell from full to 90°·6 or ¼ tension in	Insulation. The index fell from full to 49°·5 or ½ tension in	Insulation. The index fell from full to 32°·25 or ¾ tension in	Insulation. The index fell to zero (¼ tension) in	Insulation. The index fell in 1 minute to
				Min. Sec.	Min. Sec.		Min. Sec.	
Chatterton's Compound on Wire -	19·4	19·1	0·0	0 15 0 14·5	0 35 0 33		0 34 0 36	36· 35·
Do. - - - No. 1.	20·3	19·5	1·2	0 5·5 0 6	0 13 0 12	26″ 26·5	0 17 0 17	6·5 6·
Do. - - - No. 2.	19·8	19·6	0·5	0 13 0 13	0 27 0 28		0 40 0 41	28· 29·5
Do. - - - No. 3.	19·9	19·5	0·1	0 15 0 16	0 36 0 38		0 43 0 43	38· 39·
Plain G. Percha - - (A.)	19·1	18·8	0·1	0 22 0 22	0 54 0 53		0 58 0 59	48· 48·
Do. - - - (C.)	18·8	18·7	0·4	0 37 0 35 0 44	1 27 1 29 1 49		1 53 1 51 1 59	54· 55·
Chatterton's Compound on Wire and between G. P. coatings (D.)	19·2	19·1	0·1	0 45 0 47	1 58 2 10		1 58 2 13	59· 59·
Do. - - - (E.)	19·1	18·6	0·0	0 12 0 11	0 27 0 23		0 28 0 27	26·5 25·
Do. - - - (F.)	18·8	18·9	0·1	0 27 0 27	1 8 1 13		1 26 1 26	52· 53.
Ratcliffe's Gutta Percha - (A.)	16·4	16·1	0·3	0 17 0 17	0 45 0 47		0 58 0 58	42·5 44·
Do. - - - (B.)	15·9	15·7	0·3	0 17 0 17	0 43 0 40		0 42 0 42·5	42 39
Special Gutta Percha - (A.)	15·2	15·2	0·0	2 30 2 20	7 20 6 10		5 0 4 40	53· in 5′ 49·25
Do. - - - (B.)	14·8	14·8	0·0	2 55 3 0	8 30 10 0		10 0	56· in 5′ 57·5
Do. - - - (C.)	9·9	9·9	0·0	5 30 6 45	20 0 23 0		21 0	62· in 5′ 61·5
G. P. and Chatterton's Compound 10 layers each, alternate.	13·4	13·8	0·0	3 0 2 45	13 18		12 40	58· in 5′ 58·5
Hughes' fluid between G. P. layers	14·1	13·9	0·0	0 40 0 40	1 40 1 45		1 47 1 42	58· 58·25
Indian rubber, Hall and Wells (A.)	19·9	19·9	0·3	0 43 0 43	1 35 1 42		1 28 1 35	58· 59·
Do. - do. - (B.)	14·8	14·8	0·0	1 40 2 3	5 10 5 50		11 22	62· 62·75
Do. - Silver's - (B.)	10·7	10·6	0·1	4 20 4 55	12 45 15 0		17 0	60·5 in 5′
Do. - do. - (C.)	9·4	9·4	0·0	11 0 11 0	29 0		23 0	
Godefroy's Compound - -	21·2	20·9	0·4	0 17 0 17	0 42 0 43		0 35 0 40	41· 39·
Wray's Compound - - -	10·6	10·6	0·1	61 0	- -		- -	44·5 in 5½ h.
Gutta Percha $\frac{4}{32}$ in.; copper $\frac{1}{32}$ in.	19·9	19·7	0·3	0 15 0 15	0 32 0 34		0 30 0 32	34· 34·
Do. - $\frac{6}{32}$ do. - $\frac{2}{32}$	14·8	14·8	0·0	0 43 1 0	2 5 2 19		1 55 2 20	58·5 50·5
Do. - $\frac{12}{32}$ do. - $\frac{2}{32}$	11·8	11·7	0·0	0 12 0 13	0 45 0 50		0 50 0 56	43· 46·
Do. - $\frac{4}{32}$ do. - $\frac{4}{32}$	21·0	21·0	0·0	0 33 0 32	1 20 1 31		1 30 1 40	54· 55·5
Do. - $\frac{28}{32}$ do. - $\frac{4}{32}$	31·6	31·2	0·5	0 20 0 21	1 10 1 15		1 12 1 25	52· 54·
Wray's Compound, 176 yards (with 512 cells).	4·8	4·7	0·0	Faulty at the ends.				
Hearder's Wire, 440 yards -	6·6	6·3	0·0	0 32 0 35	2 35 2 45		2 40	58· 58·5

APPENDIX No. 6—*continued.* APP. No. 6

Experiments made to determine the Influence of Temperature, &c.—By E. G. Bartholomew.—Table II.—*cont.*

TEMPERATURE 52° F.

Swing of Needle, 10 oscillations in 27 seconds; Full Tension of Battery 67°.—March 30-31, 1860.

Description of Wire.	Induction. Charge.	Induction. Discharge.	Earth.	Insulation. The index fell from full to 61°·5 or ¼ tension in	Insulation. The index fell from full to 52° or ½ tension in	Insulation. The index fell from full to 32°·25 or ¾ tension.	Insulation. The index fell to zero (¼ tension) in	Insulation. The index fell in 1 minute to
	°	°	°	Min. Sec.	Min. Sec.		Min. Sec.	
Chatterton's Compound on Wire	*20·3	*20·1	0·2	0 13	0 32		0 42	39·
				0 13	0 34		0 43	40·
Do. - - - No. 1	21·2	20·3	1·2	0 3	0 8	0′. 19″	0 10	5·5
				0 3	0 8	0′. 19″	0 12	4·5
Do. - - - No. 2	20·8	20·4	0·5	0 11	0 27	1′. 0″	0 30	32·25
				0 12	0 28	1′. 2″	0 31	33·
Do. - - - No. 3	21·0	20·7	0·7	0 14	0 33	1′. 20″	0 35	41·5
				0 14	0 34	1′. 20″	0 38	41·
Main Gutta Percha - (A.)	20·1	20·0	0·4	0 20	0 50		0 55	48·
				0 21	0 53		0 55	49·
Do. - - - (C.)	20·1	19·8	0·5	0 28	0 55		1 2	50·
				0 27	0 56		1 2	50·5
Chatterton's Compound on Wire and between G. P. coatings (D.)	20·4	20·2	0·2	0 53	2 7		2 30	62·
				0 51	1 50		2 30	61·
Do. - - - (E.)	20·0	19·8	0·3	0 7	0 17	0′. 37″	0 22	23·5
				0 7	0 16	0′. 37″	0 22	21·
Do. - - - (F.)	20·1	19·9	0·0	0 32	1 7		1 17	53·
				0 34	1 12		1 17	55·
Ratcliffe's G. Percha - (A.)	17·4	17·0	0·7	0 13	0 31		0 33	34·
				0 11	0 28		0 33	33·5
Do. - - - (B.)	16·9	16·6	0·8	0 13	0 30		0 33	36·5
				0 13	0 30		0 34	35·
Special G. Percha - (A.)	16·3	16·1	0·0	1 20	4 15		4 10	48·
				1 15	3 55		4 25	47·25 in 5′
Do. - - - (B.)	15·9	15·8	0·0	1 35	4 60		5 0	50·
				1 50	4 55		5 15	51·5 in 5′
Do. - - - (C.)	10·8	10·7	0·0	4 30	12 0		10 25	58·25
				4 30	10 30		9 15	57· in 5′
G. P. and Chatterton's Compound, 10 layers of each, alternate.	14·5	14·4	0·0	1 20	6 30		6 5	54·
				1 25	6 50		6 40	54·5 in 5′
Hughes' Fluid between G. P. layers	15·4	15·1	0·3	0 11	0 40		0 34	40·5
				0 11	0 40		0 38	41·75
Indian Rubber; Hall & Wells (A.)	22·5	22·4	0·0	0 15	0 50		0 48	48·
				0 18	0 53		0 51	49·
Do. do. - (B.)	16·3	16·2	0·0	0 55	3 40		2 0	61·25
				1 3	4 5		3 55	61·5
Do. Silver's - (B.)	11·2	11·2	0·0	7 7	25 0		30	62·5 in 5′
				6 30	21 5			62·5
Do. do. - (C.)	10·2	10·2	0·0	20 0	63 0		95 9	65·5
				25 0	95 0			
Godefroy's Compound - -	22·8	22·4	0·6	0 8	0 22	0′. 50″	0 23	27·
				0 8	0 21	0′. 50″	0 23	27·
Wray's Compound - -	11·4	11·4	0·0	22· 0	50 0		54 0	64· in 5′
Gutta Percha $\frac{3}{24}$ in.; Copper $\frac{3}{32}$ in.	21·3	20·8	0·4	0 7	0 18		0 21	17·
				0 7	0 19		0 22	18·
Do. $\frac{4}{32}$ do. $\frac{4}{32}$ -	15·6	15·5	0·0	0 16	0 55		0 50	50·
				0 17	0 55		0 55	50·
Do. $\frac{11}{32}$ do. $\frac{7}{32}$ -	12·3	12·1	0·3	0 7	0 22		0 28	26·
				0 8	0 24		0 27	25·5
Do. $\frac{5}{32}$ do. $\frac{5}{32}$ -	22·4	21·9	0·4	0 8	0 24		0 32	32·5
				0 10	0 30		0 34	38·
Do. $\frac{6}{32}$ do. $\frac{6}{32}$ -	33·5	32·8	0·6	0 11	0 36		0 37	41·
				0 10	0 42		0 41	41·
Wray's Compound, 176 yds. (with 512 cells).	5·4	5·3	0·0	65 0	3 hours.			
Hearder's Wire, 440 yds. (do.)	24·9	24·7	0·8	0 12	0 40		0 55	48·
				0 15	0 40		1 0	48·

APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By E. C. Bartholomew.—Table II.—*cont.*

TEMPERATURE 62° F.

Swing of Needle, 10 oscillations in 26 seconds; Full Tension of Battery 65°.—April 1860.

Description of Wire.	Induction. Charge.	Induction. Discharge.	Earth.	Insulation. The index fell from full to 60° or ½ tension in	Insulation. The index fell from full to 48°·2 or ¼ tension in	Insulation. The index fell from full to ½ tension in	Insulation. The index fell to zero (¼ tension) in	Insulation. The index fell in one minute to
				Min. Sec.	Min. Sec.		Min. Sec.	
Chatterton's Compound on Wire -	*20°0	*19°5	0°3	0 10 0 10	0 24 0 24		0 25 0 25	20° 22·5
Do. - - - No. 1	21·1	20·0	1·5	0 3 0 3	0 7 0 8	14″	0 8 0 8	1 1·75
Do. - - - No. 2	20·4	19·8	0·6	0 10 0 10	0 29 0 27	60″	0 27 0 27	31·5 30
Do. - - - No. 3	20·6	20·0	0·5	0 8 0 9	0 24 0 26	52″	0 24 0 25	25 26
Plain Gutta Percha - - (A.)	19·5	19·1	0·2	0 20 0 20	0 55 0 56		0 57 0 58	45·5 46
Do. - - - (C.)	19·6	19·0	0·7	0 11 0 11	0 26 0 28		0 33 0 35	30 31
Chatterton's Compd. on Wire and between G. Percha coatings (D.)	19·3	19·3	0·1	0 18 0 19	0 54 0 55		0 58 0 58	45 46
Do. - - - (E.)	19·5	19·0	0·5	0 6 0 6	0 16 0 16	30″ 30″	0 16 0 16	10·5 11·5
Do. - - - (F.)	19·2	18·8	0·5	0 14 0 14	0 41 0 43		0 45 0 46	40 41·5
Ratcliffe's G. Percha - (A.)	19·7	16·4	0·5	0 7 0 7	0 23 0 22	52″ 47″	0 24 0 24	25 22·5
Do. - - - (B.)	16·1	15·4	0·5	0 7 0 7	0 20 0 21	42″ 44″	0 20 0 20	20 21
Special G. Percha - - (A.)	15·5	15·4	0·0	0 37 0 40	1 45 1 53		2 5 2 2	56 57
Do. - - - (B.)	15 2	15·1	0·0	1 0 (Battery tension 60°) 1 6	2 50 (Battery tension 48°) 3 5		2 54 2 58	60 60·25
Do. - - - (C.)	10·3	10·2	0·0	4 10 (Battery tension 60°) 4 50	10 30 (Battery tension 48°) 13 30		10 30 13 30	56·5 in 5′ 59
G. P. and Chatterton's Compound, 10 layers of each alternate -	14·0	13·9	0·1	0 35 (Battery tension 60°) 0 40	1 50 (Battery tension 48°) 2 20		2 35 2 25	56 58
Hughes' finid between G. P. layers	15·2	14·7	1·0	0 6 0 6	0 15 0 13	32″ 33′	0 13 0 13	13·5 14
Indian rubber; Hall & Wells (A.)	23·5	23·3	0·4	0 13 0 13	0 38 0 40	55″ 57″	0 37 0 40	37·5 38
Do. Do. (B.)	15·7	15·6	0·3	1 15 1 19	4 5 4 15		4 15 4 10	61 61·5
Do. Silver's (B.)	20·6	10·5	0·0	7 30 7 50	20 30 21 45		21 15 22 0	61·5 in 5′. 61
Do. Do. (C.)	9·5	9·4	0·0	12 30 12 30	39 0 36 40		38 0 38 10	62·5 in 5′. 62·5
Godefroy's Compound - - -	22·0	21·7	0·8	0 6 0 6	0 15 0 14	28″ 28″	0 15 0 15	10·5 12·5
Wray's Compound - - -	11·2	11·0	0·0	11 0 13 0	30 30 37 4		38 0	62·25 in 5′.
Gutta Percha $\frac{3}{32}$ - Copper $\frac{3}{64}$ -	20·8	20·0	0·0	0 6 0 6	0 15 0 15	27″ 27″	0 15 0 15	7·5 7·5
Do. - $\frac{6}{32}$ - Do. $\frac{3}{32}$ -	15·4	14·9	1·0	0 10 0 10	0 26 0 25	50″ 48″	0 26 0 27	25 23·5
Do. - $\frac{12}{32}$ - Do. $\frac{3}{32}$ -	12·3	12·0	0·3	0 7 0 7	0 19 0 18	35″ 33″	0 15 0 16	13 12·75
Do. - $\frac{6}{32}$ - Do. $\frac{6}{32}$ -	22·3	21·4	0·9	0 6 0 7	0 18 0 18	35″ 33″	0 16 0 18	13·5 13·5
Do. - $\frac{6}{32}$ - Do. $\frac{6}{32}$ -	33·0	32·0	1·0	0 7 0 6	0 20 0 19	42″ 42″	0 17 0 18	20 20
Wray's Compound, 176 yards (with 512 cells).	5·1	5·1	0·0	1h. 20m.	4h. 15m.			
Hearder's wire, 440 yards (ditto) -	23·2	23·0	0·1	0 14 0 15	0 40 0 44	1′·20″ 1′·40″	0 38 0 44	38 41

(X.) ½ Battery on insulated base of stand, full tension on needle.

APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By E. G. Bartholomew.—Table II.—*cont.*

TEMPERATURE 72° F.

Swing of Needle, 10 oscillations in 27 seconds; Full Tension of Battery 64·25°.—April 13–14, 1860.

Description of Wire.	Induction. Charge.	Induction. Discharge.	Earth.	Insulation. The index fell from full to 60°, or ⅔ tension in	The index fell from full to 49·5, or ½ tension in	The index fell from full to 32·25, or ¼ tension in	The index fell to zero (⅓ tension) in	The index fell in 1 minute to
				Min. Sec.	Min. Sec.		Min. Sec.	°
Chatterton's compound on wire	19°·8	19°·4	0°·8	0 27 0 26	0 63 0 58		0 58 0 51	50· 49·
Do. - - No. 1.	21·9	20·6	2·6	0 4 0 4	0 8 0 8	0′16″ 0′17″	0 12·5 0 10	2·5 5·
Do. - - No. 2.	21·0	20·5	1·2	0 5 0 5	0 15 0 16	0′35″ 0′36″	0 15 0 16	17· 18·
Do. - - No. 3.	21·2	20·6	1·5	0 5 0 5	0 15 0 15	0′37″ 0′35″	0 19 0 20	18·5 20·5
Plain gutta percha - (A.)	20·0	19·6	0·5	0 21 0 23	0 52 0 55		0 50 0 54	47· 48·
Do. - - (C.)	20·6	19·7	1·6	0 5 0 5	0 14 0 13	0′31″ 0′29″	0 16 0 16	15· 13·5
Chatterton's compound on wire and between gutta percha coatings - - - (D.)	20·2	19·6	0·7	1 8 1 5	2 40 2 30		2 31 2 35	61· 61·
Do. - - (E.)	20·2	19·5	1·1	0 5 0 5	0 14 0 15	0′28″ 0′29″	0 15 0 16	15· 16·
Do. - - (F.)	20·1	19·5	1·0	0 12 0 12	0 30 0 32	1′ 2″ 1′19″	0 30 0 32	36· 40·
Ratcliffe's gutta percha - (A.)	17·8	17·0	1·4	0 5 0 5	0 15 0 15	0′35″ 0′35″	0 17 0 19	14·5 16·
Do. - - (B.)	17·1	16·4	1·2	0 5 (⅔ battery tension 59·5°.) 0 5	0 13 (⅔ battery tension 47·5°.) 0 13	0′30″ 0′27″	0 15 0 15	14·5 11·5
Special gutta percha - (A.)	15·7	15·5	0·4	0 21 0 22	0 53 0 54		0 55 0 55	47·5 47·5
Do. - (B.)	15·4	15·2	0·2	0 37 0 42	1 37 1 48		1 48 1 50	56· 57·
Do. - - (C.)	10·6	10·5	0·0	4 30 (⅔ battery tension 59·5°.) 4 50	12 0 (⅓ battery tension 47·5°.) 13 20		10 0 11 0	58·75 in 5′ 59·
Gutta percha and Chatterton's compound, 10 layers of each, alternate - - -	14·6	14·5	0·2	0 23 0 33	0 55 1 0		0 44 0 50	48· 49·5
Hughes' fluid between gutta percha layers - - -	16·7	15·2	0·2	0 4 0 4	0 8 0 7	0′15″ 0′15″	0 10 0 10	2·5 2·
India rubber, Hall & Wells' (A.)	26·1	25·5	0·4	0 11 0 9	0 24 0 23	0′55″ 0′53″	0 24 0 26	28·5 27·
Do. - do. (B.)	17·1	17·0	0·1	1 33 1 37	4 32 4 20		4 30 4 28	47·75 in 5′ 47·25
Do. - Silver's - (B.)	10·6	10·6	0·0	4 15 4 30	10 0 11 0		10 0 10 30	57·5 in 5′ 58·
Do. - do. (C.)	9·6	9·6	0·0	6 0 6 30	14 0 14 20		14 0 13 50	60·5 in 5′ 61·
Godefroy's compound - -	23·6	22·2	2·3	0 4 0 4	0 8 0 9	0′18″ 0′18″	0 12 0 11	6· 6·5
Wray's compound - -	11·0	10·9	0·0	3 25 3 40	8 30 8 50		8 0 8 20	56·75 57·5
Gutta percha $\frac{8}{32}$ in.; copper $\frac{5}{32}$ in.	21·8	20·5	2·1	0 4 0 4	0 9 0 9	0′17″ 0′16″	0 11 0 11	2· 2·
Do. - $\frac{6}{32}$ do. - $\frac{3}{32}$	16·2	15·2	1·5	0 5 0 6	0 12 0 13	0′27″ 0′25″	0 14 0 15	3· 8·
Do. - $\frac{4}{32}$ do. - $\frac{2}{32}$	13·0	12·1	1·6	0 5 0 5	0 10 0 9	0′20″ 0′20″	0 12 0 11	3·5 3·
Do. $\frac{6}{32}$ do. - $\frac{4}{32}$	24·0	22·2	2·1	0 5 0 5	0 10 0 10	0′21″ 0′20″	0 11 0 12	5·5 5·
Do. $\frac{6}{32}$ do. - $\frac{6}{32}$	34·8	33·1	2·2	0 6 0 6	0 13 0 14	0′27″ 0′27″	0 13 0 13	8· 8·
Wray's compound, 176 yards (with 512 cells) - - -	4·9	5·0		44 0 49 0	1 h. 50 m.			
Hearder's wire, 440 yards (do.)	24·9	24·1	1·2	0 6 0 7	0 16 0 17	0′37″ 0′40″	0 18 0 19	19· 21·

Experiments made to determine the Influence of Temperature, &c.—By E. G. Bartholomew.—Table II.—*cont.*

TEMPERATURE 82°·F.

Swing of Needle, 10 oscillations in 26·5 seconds; Full Tension of Battery 65·5°.—April 19–20, 1860.

Description of Wire.	Induction. Charge.	Induction. Discharge.	Earth.	Insulation. The index fell from full to 60°, or ⅔ tension in	Insulation. The index fell from full to 40·5, or ⅓ tension in	Insulation. The index fell from full to ⅓ tension in	Insulation. The index fell to zero (⅓ tension) in	Insulation. The index fell in 1 minute to
	°	°	°	Min. Sec.	Min. Sec.		Min. Sec.	°
Chatterton's compound on wire	20·0	20·0	0·2	0 28 0 30	1 5 1 15		1 7 1 12	51· 51·
Do. No. 1.	23·4	21·6	2·8	0 3 0 3	0 5 0 5	0′.9″ 0′.9″	0 7 0 7	1·5 2·5
Do. No. 2.	22·4	21·5	1·7	0 4 0 4	0 8 0 8	0′.20″ 0′.22″	0 12 0 12	10· 10·5
Do. No. 3.	22·9	21·5	2·3	0 4 0 4	0 7 0 8	0′.18″ 0′.20″	0 12 0 12	11· 10·5
Plain gutta percha (A.)	20·0	19·6	0·4	0 22 0 23	0 53 0 55		0 56 0 56	46· 47·
Do. (C.)	22·7	20·8	3·0	0 4 3 5	0 7 0 7	0′.14″ 0′.16″	0 9 0 9	zero
Chatterton's compound on wire, and between g. p. coatings. (D.)	21·6	20·3	2·0	1 2 1 6	2 25 2 35		2 36 2 38	60· 60·5
Do. (E.)	21·5	20·1	1·9	0 4 0 4	0 8 0 8	0′.20″ 0′.21	0 11 0 11	10· 11·
Do. (F.)	21·7	20·1	2·0	0 6 0 5	0 13 0 11	0′.30″ 0′.35″	0 15 0 15	16· 18·5
Ratcliffe's gutta percha (A.)	19·3	17·3	2·2	0 4 0 4	0 7 0 7	0′.17″ 0′.17″	0 10 0 10	4· 4·5
Do. (B.)	18·0	16·9	1·8	0 4 0 5	0 8 0 8	0′.17″ 0′.18″	0 11 0 11	2·5 2·
Special gutta percha (A.)	15·8	15·5	0·5	0 15 0 16	0 40 0 44		0 44 0 42	40· 41·
Do. (B.)	15·6	15·4	0·2	0 21 0 22	1 0 1 1		0 57 1 10	50· 50·
Do. (C.)	10·5	10·6	0·0	3 7 3 10	7 10 7 50		7 35 8 30	55· 56· in 5′
G. p. and Chatterton's compound, 10 layers of each, alternate.	15·3	14·6	0·6	0 13 0 15	0 31 0 35		0 34 0 38	34· 36·
Hughes' fluid between g. p. layers	19·7	16·8	4·2	0 2 0 2	0 5 0 5	0′.8″ 0′.8″	0 7 0 7	zero
India rubber, Hall and Wells' (A.)	28·0	27·5	1·8	0 5 0 6	0 11 0 12	0′.25″ 0′.25″	0 12 0 12	9·5 9·
Do. do. (B.)	19·1	18·8	0·3	0 36 0 34	1 28 1 22		1 35 1 35	55· 54·5
Do. Silver's (B.)	10·8	10·9	0·1	1 50 2 35	5 15 7 30		5 35 8 0	51· 55· in 5′
Do. do. (C.)	10·0	10·0	0·0	3 0 3 15	6 0 9 0		9 45	56·5 in 5′
Godefroy's compound	26·0	23·3	4·2	0 3 0 3	0 5 0 5	0′.10″ 0′.10″	0 8 0 8	2· 1·
Wray's compound	12·0	11·8	0·1	1 27 1 35	3 50 4 10		4 0 4 30	47·5 48· in 5′
Gutta percha $\frac{4}{32}$ in.; copper $\frac{2}{32}$ in.	24·1	21·3	3·9	0 3 0 3	0 5 0 5	0′.8″ 0′.8″	0 7 0 7	zero
Do. $\frac{3}{32}$ do. $\frac{2}{32}$	17·3	15·5	2·5	0 3 3 5	0 6 0 7	0′.13″ 0′.13″	0 9 0 9	zero
Do. $\frac{2}{32}$ do. $\frac{2}{32}$	14·3	12·5	2·5	0 4 0 4	0 6 0 6	0′.12″ 0′.12″	0 8 0 8	zero
Do. $\frac{3}{32}$ do. $\frac{1}{32}$	27·3	24·4	4·8	0 3 0 3	0 5 0 5	0′.10″ 0′.10″	0 8 0 8	zero
Do. $\frac{2}{32}$ do. $\frac{1}{32}$	37·5	34·7	4·4	0 4 0 4	0 7 0 7	0′.16″ 0′.16″	8 5 9 5	zero
Wray's compound, 176 yards (with 512 cells)	5·6	5·5	0·0	9 0 8 0	46 0 40 0		47 0	61·5 60·75 in 5′
Hearder's wire, 440 yards (do.)	27·0	25·4	2·3	0 4·5 0 5	0 9 0 10	0′.23″ 0′.22″	0 11 0 11	12·5 11·5

a battery tension 59·5°.

b battery tension 30·0°.

Experiments made to determine the Influence of Temperature, &c.—By E. G. Bartholomew.—Table II.—*cont.*

TEMPERATURE 92° F.

Swing of Needle, 10 oscillations in 26·5 seconds; Full Tension of Battery 65·5°.—April 30, 1860.

Description of Wire.	Induction. Charge.	Induction. Discharge.	Earth.	Insulation. The index fell from full to 69°·5, or ¼ tension in	Insulation. The index fell from full to 47·5, or ½ tension in	Insulation. The index fell from full to 32°, or ¾ tension in	Insulation. The index fell to zero (¼ tension) in (K.)	Insulation. The index fell in 1 minute to
	°	°		Min. Sec.	Min. Sec.		Min. Sec.	°
Chatterton's compound on wire	21·1	20·1	0·9	0 30 0 33	1 15 1 20		1 17 1 22	52· 54·
Do. No. 1.	28·7	23·9	6·6	0 3 0 3	0 5 0 5	0″.8″ 0″.8″	0 8 0 8	zero.
Do. No. 2.	25·1	22·5	4·2	0 4 0 3	0 7 0 7	0″.14″ 0″.16″	0 8 0 8	zero.
Do. No. 3.	25·2	22·5	4·4	0 3 0 3	0 7 0 7	0″.12″ 0″.12″	0 8 0 8	zero.
Plain gutta percha (A.)	21·1	20·0	0·7	0 23 0 26	0 54 1 0		1 3 1 10	46· 48·
Do. (C.)	27·4	23·1	6·0	0 3 0 3	0 5 0 5	0″.7″ 0″.8″	0 8 0 7	zero.
Chatterton's compound on wire and between g.p. coatings (D.)	21·3	20·2	0·9	0 55 0 58	2 22 2 30		2 30 2 34	58·5 59·0
Do. (E.)	23·4	21·6	3·0	0 4 0 4	0 7 0 8	0″.14″ 0″.14″	0 8 0 10	zero.
Do. (F.)	23·5	20·3	2·0	0 4 0 4	0 8 0 8	0″.18″ 0″.20″	0 10 0 10	2·5· zero.
Ratcliffe's gutta percha (A.)	19·8	17·8	2·5	0 3 0 3	0 5 0 5	9″.0″ 9″.0″	0 8 0 7	zero.
Do. (B.)	19·1	17·1	2·2	0 3 0 3	0 6 0 6	0″.12″ 0″.12″	0 8 0 9	zero.
Special gutta percha (A.)	16·5	15·6	1·0	0 12 0 14	0 26 0 29		0 30 0 33	21·5 22·5
Do. (B.)	15·6	15·5	0·1	0 9 0 10	0 30 0 30		0 34 0 33	34·5 35·0
Do. (C.)	10·4	10·3	0·0	2 30 2 50	5 10 7 5		7 10 7 28	50·0 51·4 in 8′
G.p. and Chatterton's compound, 10 layers of each, alternate.	15·9	15·3	1·0	0 8 0 10	0 20 0 24		0 26 0 28	10·0 11·0
Hughes' fluid between g.p. layers	28·3	20·6	9·5	0 2 0 3	0 5 0 5	0″.10″ 0″.10″	0 8 0 8	zero. 4·0
India rubber; Hall & Wells' (A.)	33·8	30·2	4·9	0 3 0 3	0 6 0 6	0″.14″ 0″.14″	0 9 0 55	4·5 35·0
Do. do. (B.)	20·7	20·2	0·5	0 18 0 24	0 45 0 53		1 0 1 0	38·0 38·0
Do. Silver's (B.)	10·8	10·8	0·0	1 10 1 28	4 10 4 50		5 0 5 5	48·0 in 5′ 49·5
Do. do. (C.)	9·7	9·6	0·0	1 25 1 35	3 35 4 5		4 9 4 10	41·8 Do. 43·0
Godefroy's compound	28·1	24·4	5·4	0 3 0 3	0 5 0 5	0″.7″ 0″.7″	0 8 0 8	zero.
Wray's compound	13·2	13·2	0·4	0 40 0 43	2 0 2 15		2 17 2 24	31·0 in 8′ 34·5
Gutta percha $\frac{8}{32}$ in.; copper $\frac{8}{32}$ in.	30·1	23·9	8·4	0 2 0 3	0 4 0 4	0″.6″ 0″.6″	0 8 0 7	zero.
Do. $\frac{6}{32}$ do. $\frac{8}{32}$	21·3	17·2	5·4	0 3 0 3	0 5 0 5	0″.8″ 0″.8″	0 8 0 8	zero.
Do. $\frac{4}{32}$ do. $\frac{8}{32}$	18·3	14·2	5·4	0 3 0 3	0 5 0 5	0″.8″ 0″.8″	0 8 0 8	zero in 5′
Do. $\frac{4}{32}$ do. $\frac{4}{32}$	34·3	28·1	9·5	0 3 0 3	0 5 0 5	0″.8″ 0″.8″	0 8 0 8	zero do.
Do. $\frac{6}{32}$ do. $\frac{8}{32}$	43·9	38·5	9·7	0 3 0 3	0 5 0 5	0″.8″ 0″.8″	0 8 0 8	zero do.
Wray's compound, 176 yards (with 512 cells)	6·0	6·0	0·0	3 35 3 35	7 30 7 30		7 40 7 43	54·5 in 5′
Hearder's Wire, 440 yds. (do.)	29·6	26·4	4·5	0 4 0 4	0 7 0 8	0″.15″ 0″.16″	0 10 0 10	zero.

A few experiments were made to measure, by means of Professor Wheatstone's differential resistance measurer, the decrease of conductivity in several of the mile lengths of wire.

The gutta percha covered wire A was maintained at the constant temperature of 32° F, and its conductivity compared with the gutta percha wire C at different temperatures with the following results:—

At 32°	At 62° the conductivity of C decreased $\frac{44}{1000}$
42° the conductivity of C decreased $\frac{17}{1000}$	72° " " " $\frac{96}{1000}$
52° " " $\frac{27}{1000}$	82° " " " $\frac{141}{1000}$

In like manner the wire designated in the table as Chatterton's D was compared with the corresponding wires E and F, and the following results were obtained:—

	Of E.	Of F.		Of E.	Of F.
At 32°			At 62° the conductivity was decreased	$\frac{58}{1000}$	$\frac{84}{1000}$
42° the conductivity was decreased	$\frac{78}{1000}$	$\frac{108}{1000}$	72° " "	$\frac{126}{1000}$	$\frac{168}{1000}$
52° "	$\frac{102}{1000}$	$\frac{138}{1000}$	82° " "	$\frac{180}{1000}$	$\frac{192}{1000}$

In these experiments a single cell of Daniell's battery was employed.

APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By E. G. BARTHOLOMEW—*cont.*

TABLE III.

EXPERIMENTS on the INDUCTION and INSULATION of two COPPER WIRES, $\frac{1}{16}$ in. in DIAMETER, (each having 110 yards of their LENGTH in WATER), one covered with INDIA-RUBBER and one with GUTTA-PERCHA, made previous to and during Hydraulic Pressure, the Pressure being 3 Tons per Circular Inch, or 3·7854 Tons=8,479 lbs. per Square Inch.

Before Pressure (April 11th, 1860.)				During Pressure.			
INDUCTION.		INSULATION.		INDUCTION.		INSULATION.	
Direct.		The Index of the Electrometer		Direct.		The Index of the Electrometer	
Charge.	Discharge.	Fell from full to $\frac{3}{4}$ tension in	Fell to $\frac{1}{2}$ tension in	Charge.	Discharge.	Fell from full to $\frac{3}{4}$ tension in	Fell to $\frac{1}{2}$ tension in
GUTTA-PERCHA.							
°	°	″	′ ″	°	°	′ ″	′ ″
5·5	5·3	35	1.17	5·2	5·2	Directly after - 11.45	26. 0
5·4	5·4	37	1.20	5·4	5·2	21 hours after - 4. 0	9. 5
5·5	5·5			5·3	5·2	47 ditto - 3.45	8.25
5·5	5·5	mean 36	mean 1.18	5·3	5·3	72 ditto - 3.50	8.40
5·4	5·5			5·4	5·3	115 ditto - .55	1.55
5·5	5·4			5·4	5·4	163 ditto - 1. 0	2.12
						Temp. 42° F.	Temp. 42° F.
mean 5·47	mean 5·43			mean 5·33	mean 5·27	186 ditto - 5.10	16.25
						Temp. 48° F.	Temp. 48° F.
						214 ditto - .24	1. 0
INDIA-RUBBER.							
°	°			°	°	′ ″	′ ″
4·1	4·2	1h. 28m.	3h. 33m.	3·8	4·0	Directly after - 5.15	15. 0
4·2	4·1			3·8	3·9	21 hours after - 2.45	8.30
4·2	4·1			4·0	4·0	47 ditto - 2.55	9.40
4·2	4·2			4·0	3·9	72 ditto - 2.20	7.15
4·2	4·2			3·9	3·9	115 ditto - 1.45	3.58
4·1	4·2			4·0	3·9	163 ditto Insulation gone.	
mean 4·17	mean 4·17			mean 3·916	mean 3·93		

The conductivity of both wires was taken together by looping the distant ends. Before pressure the conductivity of the combined wires (225 yards, No. 16 copper) was equal to 89 ft. 5$\frac{1}{2}$ in. of copper wire, No. 22 gauge. After pressure their combined conductivity was equal to 92 ft. 1$\frac{1}{2}$ in. of No. 22 gauge copper wire. They had therefore diminished in conductivity 2 ft. 7$\frac{3}{4}$ in., or 2·968 ft. per cent.

Not the least interesting feature in these pressure experiments, was the extraordinary retention of charge, evinced by the gutta-percha covered wire whilst under pressure. This, which I observed accidentally, I found by measurement to be as follows :—

April 20th, 1860.—Charged the wire with full tension of battery, then put the wire to earth for 1″ ; I then found a charge remaining equal to about $\frac{3}{8}$ full battery tension ; after 2″ contact with earth, the tension left was equal to about $\frac{2}{8}$ full battery ; and after 5″ contact with earth, the remaining tension was equal to about $\frac{1}{8}$ full battery. I may observe that this effect was totally different from the "residuary" discharge I first observed last November, inasmuch as no time was allowed, after removal from earth, for an accumulation, and again, because when the pressure was removed, this effect ceased.

EXPERIMENTS made to determine the Influence of Temperature, &c.—*continued.*—By O. ROWLAND.

The Experiments detailed in the following Tables, IV., V., VI., and VII., were made by Mr. Owen Rowland, in continuation of those commenced by Mr. Bartholomew, who had been obliged to leave England. *See* Plates Nos. 1 and 2.

TABLE IV.

EXPERIMENTS on the Loss of Insulation for Dynamic Electricity of Wires of various diameters, with Coatings of Gutta-percha of different thicknesses. The Wires were each One Mile in length, and were immersed in a large Tank of Water, the Temperature of which was not artificially affected. The different Loss of Insulation of the same Wires at different times must be attributed mainly to the varying Temperatures of the different Months in which the Experiments were made. A large Galvanometer (C), with very numerous Coils of fine Wire, was employed for the Measures, and the Wires were charged with 512 Cells of a Daniell's Battery. For the Induction Experiments, Professor Wheatstone's Standard Galvanometer was used.

Designation of Wire.	Diameter of Copper.	Thickness of Covering.	Experiment No. 1. 6th March. Leakage of Insulation.	Mean.	Experiment No. 2. 3rd April. Leakage of Insulation.	Mean.	Experiment No. 3. 16th April. (Temp. 51°.) Leakage of Insulation.	Mean.	Experiment No. 4. 16th April. (Temp. 50°.) Leakage of Insulation.	Mean.	Experiment No. 5. 26th April. (Temp. 55°.) Leakage of Insulation.	Mean.	Experiment No. 6. 5th May. (Temp. 53°.) Leakage of Insulation.	Mean.	Induction. (Temp. 52°.) Discharge.	Mean.
Copper Wires, covered with plain Gutta Percha	9/32	3/32	3· 3· 2·8	2·9	5· 4·7 5·	4·9	5·25 5·25 5·5	5·3	6·5 6·1 6·	6·2	7·7 8· 8·	7·9	7· 7· 7·	7·	Removed.	
Do. do.	9/32	6/32	1·5 1·7 1·5	1·56	5· 5· 4·5	4·8	4·5 5· 5·	4·8	3·25 3·25 3·25	3·25	6· 6· 6·	6·	6· 6·1 6·1	6·06	5· 5· 5·	5·
Do. do.	9/32	9/32	5· 6· 5·	5·	8· 8· 8·25	8·08	9·5 8· 8·	8·3	9·5 9·25 9·5	9·4	8·5 8· 8·	8·16	10· 10·2 10·	10·06	7·4 7·4 7·5	7·43
Do. do.	9/32	12/32	1·5 1·7 1·7	1·6	6·25 6·25 6·5	6·3	4· 4· 4·5	4·16	4· 3·7 4·	3·9	5· 5· 5·	5·	6· 6· 6·	6·	3·8 3·9 3·8	3·83
Do. do.	9/32	6/32	4· 4·25 4·	4·08	9· 9· 9·25	9·	7· 7· 7·	7·	9· 9· 9·	9·	9· 9· 8·7	8·9	11· 10· 10·7	10·56	12· 12· 12·	12·
Do. do.	9/32	3/32	9· 9·5 9·	9·16	22· 22· 22·5	22·16	22· 22· 22·	22·	21·5 21·7 21·5	21·59	21·7 21·7 22·	21·8	24· 24· 23·5	23·8	15·7 15·6 15·7*	15·66
Do. do.	9/32	3/32	6·7 6·5 6·7	6·59	70· 72· 75·	3*	Bad.	- -	Bad.	- -	Bad.	- -	Bad.			
Do. do.	9/32	15/32	2·5 2·25 2·5	2·4	9· 8·75 9·	8·9	8· 7·7 8·	7·8	4· 4·25 4·	4·08	5· 5· 5·	5·	8· 8· 7·5	7·8	5· 5· 5·	5·
Do. do.	9/32	12/32	3·25 3·5 3·25	3·3	7· 7· 7·	7·	6·7 7· 7·	6·7	5·5 5· 5·	5·16	6· 6· 6·	6·	9· 9· 9·	9·	7·8 7·9 7·9	7·86
Gutta Percha Experimental Wires—Iron wire covered with plain Gutta Percha	3/32	3/32	6·5 6·7 6·7	6·6	15· 15· 14·5	14·8	13·5 13·5 13·7	13·56	13· 13· 13·25	13·08	13· 12·7 13·	12·9	15· 14·5 15·	14·8	12· 11·9 11·9	11·93

* Found to have been injured.

APPENDIX No. 6.—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By O. Rowland.—*continued.*

TABLE V.

EXPERIMENTS on the Insulation for Dynamic Electricity of Copper Wires coated with various insulating materials made Tank of Water, the Temperature of which was first reduced to 32° F., then raised successively to 42°, 52°, 62°, and Duplicates of the corresponding Wires in the preceding Table, but were not the same. The same Galvanometer

Designation of Wire.	Diameter of Copper.	Thickness of Covering.	Experiment No. 1. 32° F. Leakage of Insulation.	Experiment No. 1. 32° F. Mean.	Experiment No. 2. 42° F. Leakage of Insulation.	Experiment No. 2. 42° F. Mean.	Experiment No. 3. 52° F. Leakage of Insulation.	Experiment No. 3. 52° F. Mean.
GUTTA PERCHA EXPERIMENTAL WIRES.—Copper wire covered with plain gutta percha	0	3/32	5· 5· 5·	5·	5· 5· 4·7	4·9	15· 15· 15·	15·
Do. do.	0	6/32	1·5 1·25 1·25	1·3	1·5 1·25 1·5	1·4	6· 6· 6·5	6·16
Do. do.	0	9/32	3· 3· 3·	3·	4· 4· 4·	4·	13· 13·25 13·25	13·16
Do. do.	0	12/32	1· 1· 1·	1·	1·25 1· 1·25	1·16	7· 7· 7·	7·
Do. do.	0	6/32	4·25 4·25 4·25	4·25	4·25 4· 4·	4·08	14·7 15·0 15·0	14·9
GUTTA PERCHA ALTERNATE COATINGS.—Copper wire covered with 10 coatings of gutta percha and 10 of Chatterton's compound	0	6/32	·0 ·0 ·0	·0	·25 ·25 ·25	·25	·5 ·5 ·25 ·5	·23
GUTTA PERCHA SPECIAL.—Copper wire covered with improved material	0	3/32	·0 ·0 ·0	·0	·5 ·5 ·25	·4	5· 5· 5·	5·
Do., do. covered with alternate coatings of improved material and compound	0	3/32	·0 ·0 ·0	·0	·0 ·0 ·0	·0	4·7 4·7 4·6	4·6
Do., do.	0	6/32	·0 ·0 ·0	·0	·0 ·0 ·0	·0	·0 ·0 ·0	0·
HUGHES'.—Copper wire coated with gutta percha, and a viscid semi-fluid substance introduced between it and the next coating of gutta percha	0	6/32	1· 1· 1·	1·	1· 1· 1·	1·	10· 11· 10·	10·3
SILVER and Co., B.—Copper wire covered with india rubber	0	4·5/32	·0 ·0 ·0	·0	0· 0· 0·	·0	·0 ·0 ·0	·0
Do., C. do. (Covered with tape 1/32" thick.)	0	11·5/32	·0 ·0 ·0	·0	0· 0· 0·	·0	·0 ·0 ·0	·0
HALL and WELLS, A.—Copper wire covered with cotton thread and india rubber	0	6/32	2· 2· 2·	2·	2· 2·5 2·	2·16	9· 9· 9·	9·
Do. B. do.	0	6/32	·25 ·25 ·25	·25	2· 2· 2·	2·	3· 3· 3·	3·
WRAY'S.—Copper wire covered with Wray's compound	0	6/32	·0 ·0 ·0	·0	·0 ·0 ·0	·0	0· 0· 0·	0·
RADCLYFFE'S A.—Copper wire covered with gutta percha passed through a certain chemical process for increasing its durability	0	6/32	2· 2· 2·	2·	3·5 3·5 3·5	3·5	10· 10·5 10·	10·16
Do. B. do.	0	6/32	3·5 3·5 3·5	3·5	4· 3·5 3·7	3·7	14· 14· 14·5	14·16
GODEFROY'S.—Copper wire covered with patent cocoa nut gutta percha	0	3/32	3·5 3·5 3·5	3·5	5· 5·5 5·	5·16	18· 18· 18·5	18·16
HEARDER'S, 440 yards.—Copper wire covered with gutta percha (fibre between the gutta percha layers)	4/32	6/32	·1 ·1 ·1	·1	·5 ·25 ·5	·4	·1 ·1 ·1	·1

* This wire was accidentally injured

APPENDIX No. 6.—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By O. Rowland.—*continued.*

TABLE V.

at different Temperatures. The Wires were, with one exception, each One Mile in length, and were immersed in a large 72°, reduced to 49°, again raised to 82° and 92°, and finally reduced to 52°. The first Five Wires of this Table were and number of Cells of the Battery were employed as in the Experiments of the preceding Table. *See* Plate No. 1.

Experiment No. 4. 62° F.		Experiment No. 5. 72° F.		Experiment No. 6. 49° F.		Experiment No. 7. 82° F.		Experiment No. 8. 92° F.		Experiment No. 9. 52° F.		Induction. Temp. 52°.	
Leakage of Insulation.	Mean.	Leakage of Insulation.	Mean.	Leakage of Insulation.	Mean.	Leakage of Insulation.	Mean.	Leakage of Insulation.	Mean.	Leakage of Insulation.	Mean.	Discharge.	Mean.
° 15· 15·25 15·25	° 15·16	° 28 27·7 27·7	° 27·8	° 17· 17· 17·25	° 17·08	° 40· 40· 40·25	° 40·08	° 53·25 53·5 53·25	° 53·3	° 35·4 35· 35·3	° 35·2	° 7·2 7·2 7·2	° 7·2
7· 7·25 7·25	7·16	15·7 15·7 15·7	15·7	5· 5· 5·	5·	25·25 25·25 25·25	25·25	30·5 30·7 30·7	30·6	6· 6·1 6·1	6·06	5· 5· 4·9	4·96
14·7 14·5 14·7	14·6	27·5 27·7 27·7	27·6	10· 10· 10·	10	41· 40·7 40·7	40·8	51· 51· 51·	51·	10· 10·2 10·	10·06	7·9 7·9 7·9	7·9
7· 7· 7·	7·	14·7 15· 15·	14·9	5· 5· 6·	5·3	24·7 24·5 24·5	24·5	39· 38·7 38·5	38·6	9· 10· 10·	9·6	3·8 3·8 3·7	3·76
17· 17· 17·	17·	28· 27· 27·	27·3	10·25 10· 10·	10·08	41·25 41· 41·	41·08	52· 52·25 52·25	52·16	15· 14·5 15·	14·8	11 9 11·8 12·	11·9
2· 2· 2·	2·	5· 5·25 5·	5·08	·25 ·25 ·25	·25	6·5 6·5 6·5	6·5	8· 8·25 8·	8·08	3· 3· 3·	3·	5·2 5·1 5·1	5·13
3·5 3·5 3·5	3·5	5· 5· 5·	5·	1· 1· 1·	1·	6·25 6·25 6·5	6·5	17·7 17·7 18·	17·8	14·3 14· 14·2	14·16	5·4 5·4 5·3	5 36
2·5 2·25 2·5	2·4	3·25 3·25 3·5	3·3	·25 ·25 ·25	·25	3·0 3·0 3·25	3·08	5· 5·25 5·	5·08	4·5 4·6 4·5	4·53	5·3 5·3 5·4	5·33
·0 ·0 ·0	·0	·25 ·25 ·25	·25	·0 ·0 ·0	·0	·25 ·25 ·25	·25	·0 ·0 ·0	·0	·0 ·0 ·0	·0	3·4 3·5 3·5	3·46
10· 10· 10·	10·	26· 25·5 25·	25·5	7·0 6·5 6·5	6·66	39· 39· 39·25	39·08	49· 49·25 49·25	49·16	10 2 10·1 10·0	10·1	5·3 5·2 5·3	5·26
·0 ·0 ·0	·0	·12 ·12 ·12	·12	·0 ·0 ·0	·0	4· 4· 4·	4·	1·25 1·25 1·25	1·25	·5 ·4 ·5	·46	3·6 3·7 3·7	3·66
·0 ·0 ·0	·0	·0 ·0 ·0	·0	·0 ·0 ·0	0	·12 ·12 ·12	·12	·0 ·0 ·0	·0	·0 ·0 ·0	·0	3·2 3·2 3·3	3·23
9·5 9·25 9·5	9·4	12· 12· 11·7	11·9	13· 13· 13·	13·	27·25 28· 28·25	27·8	43·7 44· 44·	43·9	34·4 35· 36·	35·1	11· 11· 11·	11·
3·25 3· 3·25	3·16	4·25 4·25 4·	4·16	2· 2·5 2·5	2·3	6·25 6·25 6·25	6·25	7·7 8· 7·7	7·8	3·1 3·1 3·1	3·1	8· 8· 8·	8·
·25 ·25 ·25	·25	3· 3· 3·	3·	·0 ·0 ·0	·0	3· 3· 3·	3·	2· 1·7 2·0	1·9	2· 1·6 2·	1·86	4·5 4·6 4·6	4·56
10· 9·7 9·7	9·8	14· 14· 14·	14·	7· 6·5 7·	6·8	22· 21·7 21·7	21·8	27· 26·7 26·7	26·8	7· 7· 7·	7·	5·2 5·1 5·1	5·1
10·5 10·7 10·5	10·56	15· 15· 15·	15·	8· 8· 8·	8·	25· 25· 25·	25·	32·7 31· 32· 31·5	31·8	48·5 48· 48·	*48·16	5·7 5·7 5·6	5·66
19· 19· 19·	19·	28·5 29· 28·7	28·7	15·7 15·25 15·25	15·4	44· 44· 44·	44·	49· 49· 49·25	49·08	30· 30· 30·	30·	8·1 8·1 8·2	8·13
3· 3· 3·	3·	5· 5· 5·25	5·08	2· 2· 2·25	2·08	8· 8·25 8·5	8·25	13· 13·5 13·5	13·16	5· 5· 5·	5·	2·5 2·4 2·4	2·43

by contact with hot water pipe.

Appendix No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By O. Rowland.—*continued.*

TABLE VI.—Experiments at very High Temperature - - -

Experiments on the Leakage of Insulation on short Lengths of variously-coated Copper Wires, subjected Measurement of Time, a Roberts' Chronoscope.—

Designation of Wire.	Diameter of Copper.	Thickness of Covering.	Leakage of			
			Temperature 61°	Temperature 92°	Temperature 102°	Temperature 112°
			° m. s.	° m. s.	° m. s.	° m. s.
Gutta Percha - -	$\frac{2}{32}$	$\frac{2\cdot5}{32}$	8 in 0 25	50 in 0 42 †8 in 0 6·7	50 in 0 6 †8 in 0 0·9	50 in 0 4 †8 in 0 0·6
Do. - -	$\frac{2}{32}$	$\frac{3}{32}$	8 in 0 25	50 in 0 40 †8 in 0 6·4	50 in 0 4 †8 in 0 0·6	50 in 0 3 †8 in 0 0·45
Do. - -	$\frac{2}{32}$	$\frac{6}{32}$	8 in 0 25	50 in 0 14 †8 in 0 2·3	50 in 0 5 †8 in 0 0·8	50 in 0 4 †8 in 0 0·6
Do. - -	$\frac{2}{32}$	$\frac{12}{32}$	8 in 0 25	50 in 0 35 †8 in 0 5·6	50 in 0 15 †8 in 0 2·4	50 in 0 6 †8 in 0 0·9
Do. - -	$\frac{2}{32}$	$\frac{1}{32}$	8 in 0 24	50 in 0 25 †8 in 0 4·0	50 in 0 10 †8 in 0 1·6	50 in 0 4 †8 in 0 0·6
Do. - -	$\frac{2}{32}$	$\frac{2}{32}$	8 in 0 25	50 in 0 4 †8 in 0 0·6	50 in 0 4 †8 in 0 0·6	50 in 0 3 †8 in 0 0·45
Silver's India-Rubber -	$\frac{2}{32}$	$\frac{3}{32}$	2 in 3 0 †8 in 12 0	2 in 2 30 †8 in 10 0	2 in 0 25 8 in 1 40	8 in 0 45
Wray's - - -	$\frac{2}{32}$	$\frac{6}{32}$	2 in 1 36 †8 in 6 24	8 in 0 45	8 in 0 15	8 in 0 15
Hall and Wells - -	$\frac{2}{32}$	$\frac{8}{32}$	2 in 1 47 †8 in 7 8	8 in 1 20	8 in 0 30	8 in 0 15
Daft - - - - -	$\frac{2}{32}$	$\frac{4}{32}$	80 in 0 45 †8 in 3 0	8 in 0 35	8 in 0 16	8 in 0 5
Radclyffe - - -	$\frac{2}{32}$	$\frac{8}{32}$	8 in 0 45	8 in 0 10	2 in 0 25 †8 in 0 2·5	50 in 0 15 †8 in 0 2·4

* Reduced Temperatures.

The Wires in the above Experiments had one end hermetically sealed; the other end insulated with a coating of Hughes' vessel being also covered with a sheet of

These cables were placed in a tub of water at a temperature first of 61° Fahr., when the loss of insulation was taken, and lastly raised to 70°, 166° and 200°, the loss of insulation being taken each time. At 112° the gutta percha was *soft*, at 122° so lost its shape as to leave the copper wire bare in places. Hall and Wells' india-rubber had become crumpled up in places

Experiments on the Induction and Insulation of Gutta-Percha Plain, and India-rubber (Silver and Instrument employed for measuring induction discharge, "a Long Wire Galva-

GUTTA-PERCHA PLAIN.

Diameter of copper wire, $\frac{2}{32}$"; thickness of covering, $\frac{3}{32}$".

Induction.				Insulation.				
Date.	Hour.	Temperature of Water.	Discharge.	Date.	Hour.	Temperature of Water.	Full Tension.	Loss of Insulation.
1860.			Mean.	1860.				
July 16	2 p.m.	67°	4·9 5· 5· } 5· $\dot{2}$	July 16	2 p.m.	67°	50°	8° in 15 sec.
"	"	97	5·1 5· 5· } 5·03	"	"	97	50	8 in 3 sec.
"	"	104	4·9 5· 5· } 4·96	"	"	104	50	8 in 2 sec.
"	"	114	4·8 5· 5· } 4·93	"	"	114	50	zero in 3 sec.
"	"	125	None.	"	"	125	50	earth.

On the 28th September 1860, the above gutta-percha covered wire was examined by being slit or stripped in several places, to ascertain the position of the copper, when it appeared that, owing to the shrinking or contraction of the insulator during its exposure to high temperature, the copper had in several places shifted from the centre towards the outer side or bend of the core, as it lay in horizontal coils, and not downward by gravitation. (See woodcut, Figs. 1 and 2.) The time during which the core was subjected to the higher heat (125°) was very brief, about a quarter of an hour; still, externally the core had assumed a very irregular form, the lower layers or coils having been compressed and otherwise disproportioned by the superincumbent ones.

FIG. 1.

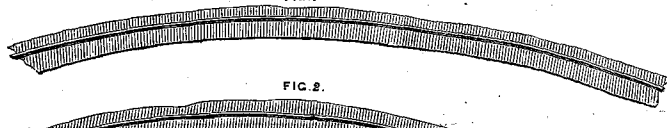
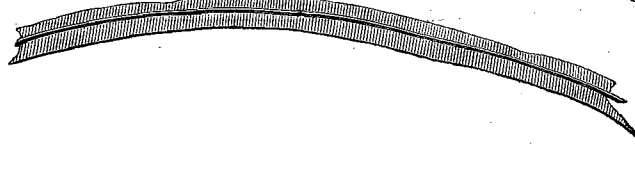


FIG. 2.



APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By O. Rowland.—*continued.*

TABLE VI.—EXPERIMENTS AT VERY HIGH TEMPERATURES.

to DIFFERENT high TEMPERATURES.—Instrument employed, a Peltier's Electrometer—Instrument used for Battery power, 512 elements.—Full tension, 50°.

INSULATION.

Temperature 64°	Temperature 122°	Temperature 142°	Temperature 152°	Temperature 64°	Temperature 70°	Temperature 166°	Temperature 200°
° m. s.	° m. s.	° m. s.	° m. s.	° m. s.	° m. s.	° m. s.	° m. s.
50 in 1 20	50 in 0 3	Bad.	Bad.	Bad.	Bad.	Bad.	Bad.
†8 in 0 12·6	†8 in 0 0·45						
50 in 1 35	50 in 0 4	do.	do.	do.	do.	do.	do.
†3 in 0 15	†8 in 0 0·6						
50 in 1 36	50 in 0 4	do.	do.	do.	do.	do.	do.
†8 in 0 15·3	†8 in 0 0·6						
50 in 1 45	50 in 0 4	do.	do.	do.	do.	do.	do.
†8 in 0 16·8	†8 in 0 0·6						
50 in 1 35	50 in 0 4	do.	do.	do.	do.	do.	do.
†8 in 0 15	†8 in 0 0·6						
50 in 1 50	50 in 0 4	do.	do.	do.	do.	do.	do.
†8 in 0 17·6	†8 in 0 0·6						
8 in 1 35	8 in 1 27	8 in 0 30	8 in 0 25	8 in 0 4	8 in 0 18	8 in 0 16	8 in 0 9
8 in 1 16	8 in 0 25	8 in 0 14	8 in 0 6	8 in 0 3	8 in 0 15	Bad.	Bad.
8 in 1 55	8 in 0 14	8 in 0 5	8 in 0 4	8 in 0 4	8 in 0 18	do.	do.
8 in 0 30	8 in 0 7	8 in 0 4	8 in 0 3	8 in 0 3	Bad.	do.	do.
50 in 3 2	50 in 0 4	50 in 0 4	Bad.	Bad.	do.	do.	do.
†8 in 0 29	†8 in 0 0·6	†8 in 0 0·6					

† Reduced to facilitate comparison.

semi-carbon fluid, and were before each test saturated with coal-tar naphtha to guard against surface conduction; the orifice of the vulcanized india-rubber to prevent the escape of steam.

afterwards raised to 92°, 102°, 112°, then reduced to 64°; again raised to 122°, 142°, 152°, and again reduced to 64°; and *very soft.* After being raised to 200° the whole was taken out of the water. The gutta percha had, in almost every case, by the contraction of the spirally wound external elastic thread. The other materials had not lost their shape.

Company's) COVERED WIRES, subjected to INCREASED TEMPERATURES. Length of Wires, each 330 feet. nometer;" for insulation a "Peltier's Electronometer." Battery power, 512 elements.

INDIA-RUBBER.

Diameter of copper wire, $\frac{9}{64}$"; thickness of covering, $\frac{1}{64}$".

INDUCTION.				INSULATION.				
Date.	Hour.	Temperature of Water.	Discharge.	Date.	Hour.	Temperature of Water.	Full Tension.	Loss of Insulation.
			Mean.					
July 16	2 p.m.	67°	2·8, 2·9, 2·8 } 2·83	July 16	2 p.m.	67°	50°	1 in 4 m. 7 sec.
"	"	97	2·8, 2·7, 2·6 } 2·70	"	"	97	50	1 in 1 m. 30 sec.
"	"	104	2·7, 2·8, 2·8 } 2·76	"	"	104	50	1 in 1 m. 28 sec.
"	"	114	2·8, 2·7, 2·7 } 2·73	"	"	114	50	1 in 30 sec.
"	"	125	2·8, 2·7, 2·7 } 2·73	"	"	125	50	6 in 30 sec.
"	"	150	2·8, 2·9, 2·8 } 2·83	"	"	150	50	6 in 15 sec.
"	"	165	2·7, 2·7, 2·8 } 2·73	"	"	165	50	6 in 10 sec.

OWEN ROWLAND.

APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature and Pressure, &c.—By O. Rowland.—*continued.*

TABLE VII.

EXPERIMENTS on the INDUCTION and INSULATION of 330 FEET LENGTHS of WRAY'S COMPOUND, GUTTA-PERCHA PLAIN, GUTTA-PERCHA SPECIAL, and INDIA-RUBBER COVERED WIRES, UNDER PRESSURE. The wires were placed in iron tubes, half-inch in diameter. The instrument employed for measuring Induction Discharges was a Suspension Galvanometer described in Mr. Bartholomew's Induction Experiments; and that for measuring the Loss of Insulation, a Peltier's Electrometer. A Roberts' Chronoscope was employed for measurement of time. Battery power, 512 elements. *See* Plate No. 2.

WRAY'S COMPOUND.

Diameter of copper wire, $\frac{9}{24}'$; thickness of coating, $\frac{5.7}{24}$.

INDUCTION.					INSULATION.				
Date.	Hour.	Temperature		Discharge.	Date.	Hour.	Temperature		Full tension.	Loss of Insulation.
		Of Pipes.	Of Hut.				Of Pipes.	Of Hut.		
1860.	Pressure of 3 tons on circular inch.			Mean.	1860.	*Without Pressure.*				
	Without Pressure.				May 3	9 p.m.	50·25	—	52	8 in 35 m.
May 3	9 p.m.	50·25	—	4·7, 4·6, 4·5, 4·7 } 4·62		*With Pressure.*				
					,,	9 p.m.	50·25	—	52	3 in 35 m.
	With Pressure.					*Without Pressure.*				
,,	9 p.m.	50·25	—	4·3, 4·2, 4·2, 4·2 } 4·2	May 4	9 p.m.	53	—	52	9.25 in 55 m.
						With Pressure.				
	Pressure on, 3 h. 20 m.				,,	12.30 p.m.	53	—	52·5	1 in 25 m.
	Without Pressure.					Pipe burst.				
May 4	9.15 p.m.	53	—	4, 3·9, 4·0, 4·1, 4·0, 4·1 } 4·01		*Without Pressure.*				
					May 16	9 p.m.	53·5	—	54	8 in 11 m.
					May 18	9.40 p.m.	54·5	—	54	8 in 25 m.
	With Pressure.					*With Pressure*				
,,	1.30 a.m.	49·5	—	4, 4·2, 4·1, 4·3, 4·2, 4·3 } 4·18	,,	10 p.m.	54	—	54	4 in 40 m.
						Injured, see Fig. 1 A.				
	Pressure on, 6 h. 30 m.					*Without Pressure.*				
	Without Pressure.				May 23	11.30 p.m.	54	—	54	8 in 17 m.
May 16	8.15 p.m.	54	—	4·1, 4·1, 4, 4, 4·1 } 4·06		*With Pressure.*				
					,,	12 p.m.	54·5	—	54	8 in 58 s. (bad).
May 18	8 p.m.	54·5	—	4·8, 4·8, 4·7, 4·7 } 4·65		Injured, see Fig. 1 A.				
,,	Pressure on, 49 h. 30 secs.									
	With Pressure.									
,,	10.30 p.m.	54·5	—	4·6, 4·5, 4·5, 4·4 } 4·5						
	Without Pressure.									
May 23	10 p.m.	57	—	4·9, 4·8, 4·8, 4·8 } 4·825						

Experiments made to determine the Influence of Temperature and Pressure, &c.—By O. Rowland.—Table VII.—*cont.*

GUTTA PERCHA (PLAIN).

Diameter of copper wire, $\frac{3}{28}$"; thickness of coating, $\frac{8}{28}$".

INDUCTION.					INSULATION.					
Date.	Hour.	Temperature Of Pipes.	Temperature Of Hut.	Discharge.	Date.	Hour.	Temperature Of Pipes.	Temperature Of Hut.	Full tension.	Loss of Insulation.
	Pressure of 3 tons on circular inch.			Mean.		*Without Pressure.*				
	Without Pressure.				1860.					
1860.					May 3	9 p.m.	50·25	—	52	8 in 5 s.
May 3	9 p.m.	44·5	—	5·5, 5·7, 5·6, 5·6 — 5·6		*With Pressure.*				
	With Pressure.				,,	9 p.m.	50·25	—	52	8 in 5 s. (bad).
,,	9 p.m.	44·5	—	5·8, 5·5, 5·6, 5·5 — 5·6		See Fig. 1 B.				
,,	Pressure on, 3 tons on square inch.					*Without Pressure.*				
	Without Pressure.				May 4	8.45 p.m.	53	—	52	8 in 20 s.
May 4	7 p.m.	53	—	4·5, 4·2, 4·4, 4·5, 4·5, 4·6 — 4·45		*With Pressure.*				
	With Pressure.				,,	12 p.m.	49·5	—	52	8 in 5 m.
May 4	12 p.m.	53	—	4·1, 4·2, 4·2, 4·4, 4·2, 4·3 — 4·23		Pipe burst, see Fig. 3.				
,,	Pressure on, 6 h. 30 secs.					*Without Pressure.*				
	Without Pressure.				May 16	8.40 p.m.	53·5	—	54	8 in 20 s.
May 16	8.15 p.m.	54	—	5·0, 5·1, 5·2, 5·1, 5·1 — 5·1	May 18	9.30 p.m.	54·5	—	54	8 in 30 s.
May 18	8 p.m.	54·5	—	5·0, 5·9, 5·0, 5·0 — 4·99		*With Pressure.*				
	With Pressure.				May 18	10 p.m.	54	60	54	8 in 1 m. 45 s.
,,	8 p.m.	52	—	5·0, 5·2, 5·0, 5·0 — 5·05	,,	11.30 p.m.	51·5	64	54	8 in 2 m. 15 s.
	12.5 p.m.	54	—	5·2, 5·0, 5·0, 5·0 — 5·04	May 19	10.45 a.m.	58·5	66	54	8 in 1 m. 30 s.
May 19	2 p.m.	55·5	—	4·8, 4·7, 4·7, 4·9 — 4·8	,,	2 p.m.	55·5	68	54	8 in 1 m.
May 21	9.30 a.m.	65·5	—	5·0, 5·0, 5·0, 5·1 — 5·02	,,	10.30 p.m.	56·5	—	54	8 in 2 m.
	Pressure on, 49 h. 30 secs.				May 21	9.10 a.m.	65·5	—	54	8 in 1 m. 15 s.
	Without Pressure.					*Without Pressure.*				
May 23	10.30 p.m.	57	—	5·2, 5·2, 5·1, 5·2 — 5·17	May 23	11.30 p.m.	54	—	54	8 in 25 s.
						With Pressure.				
					,,	12 p.m.	54·5	—	54	8 in 1 m.

The above experiments on the induction and insulation of gutta-percha plain and Wray's wires, on the 3rd of May, were interrupted through an injury to the gutta-percha covered wire in the packing box, as shown in Fig. 1 B.

The experiments on the 4th May were stopped by the bursting of the pipe; two fresh wires placed in pipes, both wires having been injured.

The experiments on the 16th May were not proceeded with under pressure, in consequence of new arrangements having to be made with regard to the position of the instruments.

The experiments on the 18th May, interrupted by an injury to Wray's compound covered wire, as shown in Figure 1 A; the wire being coiled up in the orifice of the packing box or gland, and pressed against its shoulder, until the covering was cut through, and the conducting copper wire exposed.

The experiments on the 23rd May interrupted by a similar injury to Wray's compound covered wire.

In consequence of the continual fracture of the pipes or press by the pressure of 3 tons on the circular inch in the experiments after the 3d of May the pressure was reduced to 3 tons on the square inch.

APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature and Pressure, &c.—By O. Rowland.—Table VII.—*cont.*

GUTTA PERCHA (SPECIAL).

Pressure, 3 tons on square inch. Diameter of copper wire, $\frac{2}{32}$"; thickness of coating, $\frac{3}{32}$".

INDUCTION.					INSULATION.					
Date.	Hour.	Temperature		Discharge.	Date.	Hour.	Temperature		Full tension.	Loss of Insulation.
		Of Pipes.	Of Hut.				Of Pipes.	Of Hut.		
1860.	*Without Pressure.*	°	°	Mean. °	1860.	*Without Pressure.*	°	°	°	°
May 24	8.45 p.m.	61·5	—	3·4 3·3 3·4 } 3·2 3·5 3·4	May 24	12.39 p.m.	55	—	54	8 in 1 min.
	With Pressure.					*With Pressure.*				
					,,	2.30 a.m.	52·5	—	54	8 in 5 min.
					,,	,,	,,	—	,,	8 in 5 m.
					,,	,,	,,	—	,,	8 in 5 m. 6 s.
May 24	1.45 p.m.	52·5	—	3·2 3·2 } 3·23 3·3	May 25	10 a.m.	50·5	52	54	8 in 8 m.
					,,	3.25 p.m.	53	52	,,	8 in 7 m.
					,,	5 p.m.	63	—	54	8 in 3 m.
					,,	,,	,,	—	,,	8 in 3 m.
May 25	10 a.m.	50·5	52	3·5 3·4 3·5 } 3·5 3·6	,,	10 p.m.	53	—	,,	8 in 4 m.
					,,	10.40 p.m.	52·5	52½	,,	8 in 3 m.
					,,	,,	,,	,,	,,	8 in 3 m.
					,,	12 p.m.	,,	64	,,	8 in 3 m.
					,,	1.30 a.m.	52·5	67	,,	8 in 6 m.
,,	1 p.m.	63	54	4·5 4·6 4·5 } 4·52 4·5	,,	1.50 a.m.	53	67	,,	8 in 6 m.
					,,	2.35 a.m.	53	64	54	2 in 6 m.
					May 26	11.45 a.m.	55·5	63	54	8 in 2 m. 45 s.
					,,	,,	,,	,,	,,	8 in 3 m.
					,,	9 p.m.	55·5	59	54	8 in 9 m.
,,	1.20 p.m.	52·5	68	3·6 3·6 3·3 } 3·5 3·5	May 28	3.25 p.m.	53	52	54	8 in 8 m.
					,,	8.15 p.m.	47·5	48	54	8 in 10 m.
					May 29	10.21 a.m.	53	56	54	8 in 9 m.
							53	62	54	8 in 8 m.
							52	62·5	54	8 in 8 m.
May 26	1 p.m.	55·5	66	3·5 3·5 3·6 } 3·52 3·5	May 30	1 p.m.	53	58	54	8 in 8 m.
					,,	1.9 p.m.	53	59	54	8 in 8 m.
					,,	1.17 p.m.	54	59	54	8 in 8 m.
					,,	2.37 p.m.	54·7	60	54	8 in 7 m.
					,,	2.44 p.m.	55	62·5 to 65	54	8 in 7 m.
May 28	12.30 a.m.	52·25	—	3·0 3·0 3·1 } 3·025 3·0	May 30	2.54 p.m.	56	65 to 67	54	8 in 7 m.
					,,	3.30 p.m.	55·5	71·5 to 72	54	8 in 5 m. 30 s.
					,,	3.36 p.m.	56·5	72	54	8 in 6 m.
					,,	3.43 p.m.	56	72	54	8 in 6 m.
					,,	4.10 p.m.	55·5	67·65	54	8 in 7 m.
June 1	10 a.m.	58	—	2·9 2·9 2·8 } 2·975 2·9	,,	4.18 p.m.	56	65·62½	54	8 in 7 m.
					,,	4.56 p.m.	55·5	59	54	8 in 7 m.
					,,	5.8 p.m.	56	59	54	8 in 7 m.
					,,	7.45 p.m.	51·5	56	54	8 in 7 m.
					,,	8.0 p.m.	51·5	56·5	54	8 in 8 m. 30 s.
June 6	12 a.m.	55	—	3·0 2·9 2·9 } 2·95 3·0	,,	8.25 p.m.	50	56·5	54	8 in 9 m.
					,,	11.25 p.m.	47·5	51·5	54	8 in 14 m.
					,,	12.7 p.m.	47·5	51	54	8 in 18 m.
					,,	12.30 p.m.	47·25	52·5	54	8 in 18 m.
					,,	1.20 a.m.	47·25	60	54	8 in 20 m.
June 7	4 p.m.	51·5	—	3·1 3·2 3·0 } 3·1 3·1	,,	1.41 a.m.	47·5	63·5	54	8 in 22 m.
					,,	2.5 a.m.	47·5	64 to 67·5	54	8 in 22 m.
					,,	3.0 a.m.	47·7	61	54	8 in 18 m.
					,,	3.18 a.m.	48	56	54	8 in 18 m. 30 s.
					,,	10.0 a.m.	53	58	54	8 in 5 m. 30 s.
June 8	2.30 p.m.	61	—	3·1 3·1 2·9 } 3·025 3·0	,,	10.20 a.m.	54	59	54	8 in 5 m. 45 s.
					May 31	10.0 a.m.	51	50	54	8 in 6 m.
					,,	10.8 a.m.	51·25	50	54	8 in 5 m.
					,,	10.13 a.m.	51·25	51	54	8 in 6 m.
					,,	11.30 p.m.	52	60	54	8 in 5 m.
June 11	1 p.m.	62·5	—	2·9 2·9 3·0 } 2·95 3·0	,,	,,	,,	,,	,,	8 in 7 m.
					,,	,,	51·5	61	,,	8 in 7 m.
					,,	12.30 p.m.	51·5	66	,,	8 in 10 m.
						Without Pressure.				
June 12	8 p.m.	56·5	—	3·2 3·3 3·4 } 3·3 3·3	June 1	10.17 p.m.	54·5	56	54	8 in 1 m. 15 s.
					,,		,,	,,	,,	8 in 1 m. 15 s.
					,,		,,	,,	,,	8 in 1 m. 15 s.
						With Pressure.				
June 13	11 p.m.	58·5	—	3·0 3·0 2·9 } 2·975 3·0	June 1	10.0 a.m.	58	62	54	8 in 5 m.
					,,	10.20 a.m.	58	62	,,	8 in 5 m.
					,,	10.25 a.m.	58	62	,,	8 in 5 m.
					,,	12.25 a.m.	60·5	66	,,	8 in 3 m.
June 14	1 p.m.	58·7	—	2·9 3 [illegible] [illegible] } 2·95	,,	9.0 p.m.	55	56	,,	8 in 4 m.
					,,	9.5 p.m.	54·5	56	,,	8 in 5 m.
					,,	9.11 p.m.	54·5	56	,,	8 in 5 m.
					,,	10.10 p.m.	54·5	56	,,	8 in 6 m.
					,,	10.25 p.m.	54·5	58	54	8 in 8 m.
					,,	10.45 p.m.	54·5	58	54	8 in 7 m.
					,,	10.55 p.m.	54·5	58	54	8 in 6 m.
	Without Pressure.				,,	2.30 a.m.	56·25	70	54	8 in 3 m. 30 s.
June 18	12 a.m.	57·5	—	4·[illegible] 3·[illegible] } 3·[illegible] 4·0 3·7	,,		,,	,,	,,	8 in 4 m.
					,,		,,	,,	,,	8 in 4 m.
					,,				,,	8 in 4 m.

APPENDIX No. 6—*continued.* APP. No. 6

Experiments made to determine the Influence of Temperature and Pressure, &c.—By O. Rowland.—Table VII.—*cont.*

Gutta Percha (Special)—*continued.*

INDUCTION.					INSULATION.					
Date.	Hour.	Temperature		Discharge.	Date.	Hour.	Temperature		Full tension.	Loss of Insulation.
		Of Pipes.	Of Hut.				Of Pipes.	Of Hut.		
1860.	*With Pressure.*			Mean.	1860.	*With Pressure.*				
June 20	10 a.m.	57	—	20 } 20 bad	June 4	10.0 p.m.	52	58	50	8 in 8 m. 30 s.
					,,	,,	,,	60	56	8 in 9 m. 30 s.
	Without Pressure.				,,	,,	,,	62	50	8 in 9 m.
June 23	3.30 p.m.	65	68	2·8 } 2·8	,,	,,	49·5	57	50	8 in 9 m. 45 s.
					June 5	11.0 a.m.	54	57	60	8 in 7 m.
	With Pressure.				,,		54	57	50	8 in 7 m.
,,	5 p.m.	64	68	2·7 } 2·65	June 6	12, a.m.	55	61	50	8 in 6 m. 45 s.
				2·6 }	,,	12.15 a.m.	55	61	50	8 in 7 m. 15 s.
,,	10 p.m.	62	66	4·0	,,	1.0 p.m.	57·5	61	50	8 in 5 m.
				3·8	,,	,,	57·5	62	50	8 in 5 m. 30 s.
				3·8 } 3·804	,,	7.25 p.m.	54·5	60	50	8 in 6 m. 20 s.
				3·8	,,	7.45 p.m.	53·5	59	50	8 in 7 m.
				3·8	,,	8.2 p.m.	53·5	58	50	8 in 7 m. 15 s.
	Without Pressure.				,,	8.26 p.m.	52·5	56	50	8 in 6 m. 18 s.
June 26	12 a.m.	60	66	3·	,,	9.20 p.m.	52·25	53	50	8 in 5 m.
				3·	,,	9.25 p.m.	52·5	54	50	8 in 6 m. 30 s.
				3· } 2·95	,,	9.50 p.m.	52·5	54	50	8 in 5 m.
				3·	,,	10 p.m.	52·5	54	50	8 in 7 m. 10 s.
				28	,,	10.8 p.m.	52·25	54	50	8 in 9 m. 25 s.
June 27	12.45 a.m.	63	66	3·	,,	10.19 p.m.	52·25	54	50	8 in 9 m.
				3· } 3·	,,	10.30 p.m.	52·25	54	50	8 in 9 m.
				3·	,,	11 p.m.	52·25	61	50	8 in 9 m.
June 28	3.30 p.m.	59	66	3·	,,	11.48 p.m.	52	66	50	8 in 9 m. 4 s.
				3· } 3·	June 7	3.42 p.m.	51·25	62	50	8 in 8 m.
				3·	,,	,,	,,	63	50	8 in 8 m. 5 s.
	Under Pressure.				,,	,,	,,	63	50	8 in 8 m.
,,	4 p.m.	58	66	3·	,,	8.0 p.m.	52	61	50	8 in 9 m.
				3·1 } 3·02	,,	,,	,,	61	50	8 in 9 m. 14 s.
				3·	,,	8.45 p.m.	,,	60	50	8 in 10 m.
June 29	9.20 a.m.	56	60	3·1	June 8	10 a.m.	57	61·5	50	8 in 4 m. 30 s.
				3·	,,	,,	57	61·5	50	8 in 4 m. 30 s.
				3· } 3·02	,,	10.15 a.m.	57	62	50	8 in 4 m. 15 s.
				3·	,,	12 a.m.	59·7	62	50	8 in 3 m. 15 s.
				3·	,,	,,	59·5	64	50	8 in 3 m. 35 s.
June 30	9.10 a.m.	54·7	58	3·	,,	,,	59	64	50	8 in 3 m. 15 s.
				3· } 3·	June 10	3.25 p.m.	58	62	50	8 in 4 m.
				3·	,,	4 p.m.	57	63	50	8 in 4 m. 30 s.
July 3	9.15 p.m.	63·5	64	3·1	,,	,,	53·7	64	50	8 in 5 m.
				3·	,,	10.30 p.m.	52	62	50	8 in 9 m.
				3·1 } 3·07	,,	,,	51·7	62	50	8 in 9 m. 5 s.
				3·1	,,	,,	51·5	62	50	8 in 9 m. 30 s.
July 4	5 p.m.	66	72	3·	June 11	12 a.m.	61	65	50	8 in 3 m. 15 s.
				3·1	,,	,,	,,	65	50	8 in 3 m. 12 s.
				3·2 } 3·07	,,	,,	,,	65	50	8 in 3 m. 20 s.
				3·	,,	,,	,,	66	50	8 in 3 m. 25 s.
July 5	4.20 a.m.	47·5	56	3·	,,	,,	62·25	65	50	8 in 3 m.
				3·	,,	2 p.m.	61	65	50	8 in 3 m. 30 s.
				3· } 3·	,,	,,	60	65	50	8 in 3 m. 40 s.
				3·	,,	,,	58·25	65	50	8 in 4 m.
July 6	11.20 a.m.	62·5	66	3·	,,	,,	58·7	65	50	8 in 4 m. 20 s.
				3·2 } 3·06	,,	7 p.m.	58	60	50	8 in 3 m.
				3·	,,	,,	58	60	50	8 in 3 m.
July 7	11.8 a.m.	60	64	3·1	June 12	12 a.m.	56·5	60	50	8 in 4 m. 45 s.
				3·1 } 3·12	,,	,,	57	62	50	8 in 4 m. 30 s.
				3·2	,,	1 p.m.	57·25	65	50	8 in 4 m.
				3·1	,,		57·25	65	50	8 in 4 m. 30 s.
July 13	9.30 a.m.	59·5	62	3·	,,			65	50	8 in 4 m. 30 s.
				3·2 } 3·1	,,		,,	65	50	8 in 4 m. 30 s.
				3·1	June 13	10.15 a.m.	58·5	71	50	8 in 4 m.
				3·1	,,	,,	58·5	71	50	8 in 4 m.
July 14	10.30 a.m.	60	62	3·1	,,	12 a.m.	60·5	72	50	8 in 3 m. 45 s.
				3·	,,	12 a.m.	61	72	50	8 in 3 m. 13 s.
				3·1 } 3·07	,,	,,	61	72	50	8 in 3 m. 15 s.
				3·1	,,	,,	62·25	73	50	8 in 3 m. 20 s.
					,,	,,	62·5	73	50	8 in 3 m. 45 s.
					,,	10 p.m.	53	64	50	8 in 7 m. 30 s.
					,,	11 p.m.	52	64	50	8 in 9 m. 15 s.
					,,	,,	52	63	50	8 in 9 m.
					June 14	1 p.m.	57·25	74	50	8 in 3 m. 25 s.
					,,	,,	—	—	—	8 in 3 m. 25 s.
					,,	,,	—	—	—	8 in 3 m. 25 s.
					,,	10 p.m.	57	72	50	Dead earth.
						From this date to the end the wire became evidently defective in insulation.				
						Without Pressure.				
					June 22	6 p.m.	60·5	74	50	8 in 30 s.
					,,	9 p.m.	,,	78	50	8 in 30 s.
					June 23	3.30 p.m.	65	68	50	8 in 25 s.
					,,	,,	,,	,,	,,	8 in 25 s.
					,,	,,	,,	,,	,,	8 in 25 s.

APPENDIX No. 6.—*continued.*

Experiments made to determine the Influence of Temperature and Pressure, &c.—By O. Rowland.—Table VII.—*cont.*

Gutta Percha (Special)—*continued.*

Induction.					Insulation.					
Date.	Hour.	Temperature		Discharge.	Date.	Hour.	Temperature		Full tension.	Loss of Insulation.
		Of Pipes.	Of Hut.				Of Pipes.	Of Hut.		
					1860.		*With Pressure.*			
							°	°	°	
					June 23	4 p.m.	64·5	68	50	8 in 2 m. 30 s.
					,,	,,	,,	,,	,,	8 in 2 m. 30 s.
					,,	9.30 p.m.	62	66	50	Bad.
							Defects made good.			
							Without Pressure.			
					June 27	12 a.m.	63	66	50	8 in 25 s.
					,,					8 in 26 s.
					,,	3.30 p.m.	59	66	50	8 in 18 s.
					,,	,,	,,	,,	,,	8 in 17 s.
							With Pressure.			
					June 27	4 p.m.	58·5	66	50	8 in 45 s.
					,,	,,	,,	,,	,,	8 in 45 s.
					,,	,,	,,	,,	,,	8 in 44 s.
					,,	9 p.m.	54·7	56	50	8 in 45 s.
					,,	,,	,,	,,	,,	8 in 45 s.
					June 29	9 a.m.	55·5	66	50	8 in 25 s.
					,,	,,	,,	,,	,,	8 in 24 s.
					,,	,,	,,	,,	,,	8 in 25 s.
							Pressure let down to re-tighten unions.			
							Without Pressure.			
					,,		56	66	50	8 in 8 s.
					,,					8 in 10 s.
					,,		,,	,,	,,	8 in 12 s.
					,,		,,	,,	,,	8 in 14 s.
					,,		,,	,,	,,	8 in 16 s.
					,,		,,	,,	,,	8 in 14 s.
							With Pressure.			
					June 29		57	66	50	8 in 35 s.
					,,					8 in 35 s.
					June 30	9.30 a.m.	54	60	50	8 in 25 s.
					,,					8 in 25 s.
					July 5	4 a.m.	47·5	56	50	8 in 10 s.
							,,	,,	,,	8 in 11 s.
							,,	,,	,,	8 in 11 s.
						9 a.m.	59	70	50	8 in 12 s.
						4 p.m.	73·5	82	50	8 in 7 s.
							,,	,,	,,	8 in 7 s.
					July 6	11·20 a.m.	62·5	66	,,	8 in 16 s.
					,,		,,	,,	,,	8 in 17 s.
					July 7	11 a.m.	60	64	50	8 in 10 s.
					When the wire was first charged, the instrument showed a total loss of insulation, and the same on the second and third times; the spark which appeared on making contact showed "earth," but on the wire being charged for the fourth time the instrument indicated not only a restored but an improved insulation.					
					July 7	11·8 a.m.	60	64	50	8 in 20 s.
					,,		,,	,,	,,	8 in 19 s.
					July 13	9 a.m.	59	62	50	8 in 15 s.
					,,		,,	,,	,,	8 in 14 s.
					,,		,,	,,	,,	8 in 14 s.
					,,		,,	,,	,,	8 in 15 s.
					July 14	10·30 a.m.	60	62	50	8 in 12 s.
							,,	,,	,,	8 in 13 s.
							,,	,,	,,	8 in 12 s.
							,,	,,	,,	8 in 13 s.

The preceding table of experiments on the induction and insulation of gutta-percha (special) covered wire shows that up to the 14th June, a period of 21 days, the readings of the loss of insulation have been very regular, according to the various degrees of temperatures to which the longitudinal hydraulic pipes were exposed. It will be observed that the highest readings occur on the 30th May, under a temperature below 48°, and upon reference to the table of experiments on the loss of insulation for dynamic electricity, it will be found that with a temperature of 42° the loss of insulation on first experimental wire $\frac{1}{12.32}$, was 4°·9, but when raised up to 52° the loss increased to 15°. At a temperature of 62° the loss was only 15°·16, but at 72° it increased to 27°·8. In the above experiment on the loss of insulation for static electricity the same degrees of temperatures appear to affect the gutta-percha. When the temperature exceeds 50° and 62°, there will be found a marked difference in the readings. There are a few discrepancies, owing to the difficulty of keeping the hut and the instrument in a dry state, in consequence of the heavy and incessant rains which fell during the above period, so that the leakage of the instrument itself, in some instances, enhanced that of the wires under experiment.

At 10 a.m. on the 14th June the instrument indicated "dead earth," or a total loss of insulation on the part of the wire; and on the 18th the pressure having been removed it was examined, when it appeared that it had either by a sudden cessation of pressure, or a gradual suction, been forced or drawn into the T-piece at the near or press end of the pipe, which removed the covering from the copper wire and destroyed the insulation. The wire again being restored and repacked, a few days were allowed for the packing to harden, but upon being again tested on the 21st, it was found "bad." On the 22nd the pressure was withdrawn, and the wire thoroughly examined in

APPENDIX No. 6—*continued.*

App. No. 6. E. 2—7. Experiments on induction and insulation of variously covered wires under pressure.

Experiments made to determine the Influence of Temperature and Pressure, &c.—By O. Rowland.—Table VII.—*cont.*

the presence of Captain Galton and Mr. Latimer Clark. The injury was found in the pipe within six feet of the further end, in the shape of a partly unfolded kink, with a portion of the covering stripped from the wire (see Fig. 4); this being cut off, the end was hermetically sealed, and the wire tested before and under pressure on the 22nd and 23rd, the results being considered satisfactory under the very high degrees of temperatures then existing.

At 9. 30 P.M. on the last mentioned date the wire again had failed.

On the 27th, having removed the pressure, the wire on being tested was found injured at the further end, where it had been sealed. Experiments were resumed on the 28th.

The following experiments on induction were made on the 29th, as comparative tests with Henley's large suspension galvanometer. 512 elements :—

	Discharge.	
	°	
Gutta-percha, special	5·2	
" "	5·3	Mean. 5°·35
" "	5·3	
" "	5·3	
India-rubber	5	
"	5·1	Mean. 5°·02
"	5·	
"	5·	

	Discharge.	
Both wires united, 660 ft. in length	10·2	
" " "	10·2	Mean. 10°·2
" " "	10·2	

The discharge from the 330 feet length of the wire covered with special gutta-percha under pressure was compared with the one mile covered with the same material in the large tank. 256 cells of the battery were employed.

	Discharge.	
	°	
330 ft. special gutta-percha	2·5	
" " "	2·7	Mean. 2°·6
" " "	2·6	

Temp. pipe, 56°.

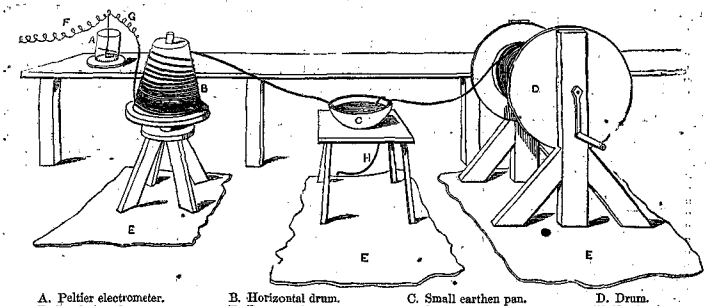
or a discharge of 41·6 per mile.

	Discharge.
	°
1 mile special gutta percha in tank	49·2
Temp. tank 45°.	
Less 1° on leading wire	48·2

July 28th.—The wire was this day taken from the pipe and examined in the presence of Captain Galton and Mr. Latimer Clark. It was found that it had been forced again into the T piece and cut. In external appearance no change was perceptible, but upon being tested in a tub of water at a temperature of 64°, the insulation was bad.

METHOD OF TESTING INSULATED WIRES FOR A FAULT.

Method used in testing insulated wires to discover a fault.



A. Peltier electrometer. B. Horizontal drum. C. Small earthen pan. D. Drum.
E. Insulating sheet. F. Battery wire. G. Leading wire. H. Earth wire.

The wire having been kept in a perfectly dry temperature for a few days, to avoid surface conduction, was again thoroughly tested.

The following were the arrangements adopted :—

The wire was placed in a coiled form on an insulated horizontal drum B, and one end thereof was attached to the stem of the electrometer A, in connexion with the battery (512 cells), the end of the battery wire being covered with dry silk, so as to maintain a constant but subdued charge or tension. A small earthenware basin C, filled with water (having connexion with the earth through a galvanometer H), was placed in an intermediate position between the drum B and the insulated drum D. The wire was then gradually drawn through the water in the basin C, and wound upon the drum D.

A few feet were passed through without any effect upon the instrument; but when a joint made in it, on 22nd June, became immersed, the needle fell instantly to zero, *indicating a defect.* The remainder of the wire in passing through the testing basin showed a tolerably uniform state of insulation, only causing a few slight oscillations of the needle.

The wire was then taken from the drum and replaced in a large earthenware pan, with the exception of the defective joint, one end being, as before, attached to the electrometer, and the other insulated, and an earth wire led into the pan. Water was then gradually poured into the large pan at a temperature of 64°, but the needle showed no variation in the insulation. Upon being tested in the ordinary way, the following were the results:—

Time.	Temperature of Water.	Temperature of Room.	Full Tension.	Loss of Insulation.
	°		°	°
2 p.m.	64	66	50	8 in 27 s.
2 p.m.	64	66	50	8 in 28 s.
2 p.m.	64	66	50	8 in 28 s.
2 p.m.	64	66	50	8 in 30 s.

The defective joint being now immersed, the needle fell to *zero instantly.*

The wire, before being submitted to pressure on the 24th May, tested as follows, viz. :—

Time.	Temperature of Water.	Full Tension.	Loss of Insulation.
	°	°	°
May 24 12 p.m.	55	54	8 in 1 m.
June 1 10·17 p.m.	54·5	54	8 in 1 m. 15 s.

The temperature of the water in the pan having been reduced to 32°·5, the wire was again tested with the following result :—

Time.	Temperature of Water.	Temperature of Room.	Full Tension.	Loss of Insulation.
	°		°	°
7 30 p.m.	32½	68	50	8 in 3 m. 15 s.
" "	32½	68	50	8 in 3 m. 20 s.
"	32½	68	50	8 in 3 m. 17 s.
"	32½	68	50	8 in 3 m. 30 s.
Joint immersed				Zero in 15 s.

App. No. 6.

Appendix No. 6—*continued.*

Experiments made to determine the Influence of Temperature and Pressure, &c.—By O. Rowland.—Table VII.—*cont.*

Temperature of water raised to 55°, corresponding with that of the 24th of May, quoted above. Results :—

Time.	Temperature of Water.	Temperature of Room.	Full Tension.	Loss of Insulation.
	°		°	
9 30 p.m. -	55	66	50	8 in 44 s.
" -	55	66	50	8 in 45 s.
" -	55	66	50	8 in 44 s.
" -	55	66	50	8 in 45 s.

Pressure on from 24th May to 29th July 1860:—

Time.	Temperature of Water.	Temperature of Room.	Full Tension.	Loss of Insulation.
	°		°	
August 23				
10 a.m. -	57	65	50	8 in 47 secs.
" -	"	"	"	8 in 48 secs.
" -	"	"	"	8 in 49 secs.

INDIA RUBBER.

Pressure, 3 tons on square inch.

Diameter of copper wire, $\frac{3}{22}$. Thickness of coating, $\frac{3}{32}$.

Induction.					Insulation.					
Date.	Hour.	Temperature Of Pipes.	Temperature Of Hut.	Discharge.	Date.	Hour.	Temperature Of Pipes.	Temperature Of Hut.	Full tension.	Loss of Insulation.	Loss of Insulation reduced.
	Without Pressure.			Mean.		*Without Pressure.*					
1860.	°	°	°	°	1860.		°	°	°		°
May 24	12·30 p.m.	55	—	4·0	May 24	12.30 p.m.	55	—	54	8 in 45 m. -	2 in 11 m. 15 s.
"				3·8							
"				3·8 } 3·86							
"				3·8							
"				3·9							
"				3·2							
"				3·2 } 3·2							
	With Pressure.					*With Pressure.*					
May 24	1.45 a.m. -	52	—	4·0	"	1.40 a.m.	52	—	54	8 in 22 m. -	2 in 5 m. 30 s.
"				3·8 } 3·83	May 25	5 p.m.	63	—	54	4 in 1 h. -	2 in 30 m.
"				3·8	"	10.22 p.m.	53	—	54	8 in 13 m. -	2 in 3·15 s.
May 25	1.23 p.m.	63	—	4·0	"	10.35 p.m.	"	—	54	8 in 10 m. -	2 in 2 m. 30 s.
"				3·9 } 3·9	"	10.48 p.m.	52·5	—	54	8 in 8 m. -	2 in 2 m.
"				3·8	"	12 p.m.	52	65	54	20 in 18 m. -	2 in 1 m. 48 s.
"				3·9	"	12.35 a.m.	"	67	54	2 in 20 m. -	2 in 20 m.
"	—	—	—	3·0	"	2.10 a.m	53	69	54	2 in 15 m. -	2 in 17 m.
"			●	2·9 } 2·97	"	2.50 a.m.	53	64	54	1 in 5 m. -	2 in 10 m.
"				3·0	"	12.0 a.m.	55	63	54	3 in 30 m. -	2 in 20 m.
"				3·0	"	9.40 p.m.	50	59	54	1·5 in 20 m.	2 in 26 m. 4 s.
"	10 a.m.	50·5	—	3·0	"	10 p.m.	50½	52	54	2·5 in 25 m.	2 in 20 m.
"				3·0 } 3·03	May 28	4.30 p.m.	53	51	54	1·5 in 40 m.	2 in 52 m. 8 s.
"				3·1	"	9.5 p.m.	46·5	48	54	2 in 55 m. -	2 in 55 m.
"				3·0	May 29	12 a.m.	52·25	57	54	1·5 in 30 m.	2 in 40 m.
May 28	4·30 p.m.	52·7	57·5	3·0	"	3.12 p.m.	55·25	70-71·5	54	·6 in 15 m. -	2 in 50 m.
"				2·9 } 3·0	"	4.56 p.m.	55·7	61-59	54	·6 in 15 m. -	2 in 50 m.
"				3·0	"	5.8 p.m.	52·5	59-56	54	10 in 2 h. 20 m.	2 in 28 m.
"				3·1	"	10.26 a.m.	—	52	54	2 in 30 m. -	2 in 30 m.
June 1	10 a.m.	58	62	2·4	May 31	1 p.m.	61	66	54	10 in 2 h. -	2 in 24 m.
"				2·4 } 2·35	June 4	10 a.m.	57·5	61	50	2 in 30 m. -	2 in 30 m.
"				2·3	"	5 p.m.	55	63	50	2 in 34 m. -	2 in 34 m.
"				2·3	"	11.20 a.m.	53·5	57	50	15 in 4 h. -	2 in 32 m.
June 6	12 a.m.	55	61	2·5	June 6	12.40 a.m.	55	61	50	1 in 15 m. -	1 in 30 m.
"				2·6 } 2·55	"	1.30 p.m.	58	62	50	3 in 45 m. -	2 in 30 m.
"				2·6	"	8.10 p.m.	52·5	57	50	1·4 in 15 m. -	2 in 21 m. 24 s.
"				2·5	"	9.32 p.m.	52·5	54	50	1·7 in 15 m. -	2 in 17 m. 39 s.
June 7	4 p.m.	51·5	63	2·6	"	11.20 p.m.	52·25	62	50	1 in 15 m. 3 s.	2 in 30 m. 6 s.
"				2·6 } 2·575	June 7	10 a.m.	49·7	54	50	8 in 15 m. -	2 in 3 m. 45 s.
"				2·5	"	8.45 p.m.	52	60	50	1 in 15 m. 2 s.	2 in 30 m. 4 s.
"				2·6	June 8	11 a.m.	57·5	62	50	1·6 in 15 m. -	2 in 18 m. 45 s.
June 8	2.30 p.m.	61	65	2·7	"	12 a.m.	59·7	65	50	8 in 4 h. -	2 in 60 m.
"				2·6 } 2·6	"	10.30 p.m.	51	62	50	1 in 15 m. 10 s.	2 in 30 m. 20 s.
"				2·6	June 11	12 a.m.	61	66	50	1 in 15 m. -	2 in 30 m.
"				2·5	June 12	12.30 a.m.	57	61	50	1·8 in 15 m. -	2 in 15 m. 40 s.
June 11	1 p.m.	62·25	65	2·6	"	4 p.m.	57·25	66	50	1·6 in 15 m. -	2 in 18 m. 45 s.
"				2·5 } 2·55	June 13	12 a.m.	61	72	50	2 in 15 m. -	2 in 15 m.
"				2·5	"	3 p.m.	59·5	72	50	8 in 47 m. -	2 in 11 m. 45 s.
"				2·6	"	"	59·5	72-62	50	13 in 1 h. 25 m.	2 in 13 m. 20 s.
June 12	8 p.m.	56·5	60	2·6	"	7.40 p.m.	52	62	50	1·5 in 15 m. -	2 in 20 m.
"				2·7 } 2·7	June 14	1 p.m.	57·7	74	50	1·2 in 15 m. -	2 in 25 m.
"				2·7	June 15	2 p.m.	58·7	70	50	2 in 15 m. -	2 in 15 m.
"				2·8	"	9.30 p.m.	57	62	50	1 in 15 m. -	2 in 30 m.
June 13	11 p.m.	58·5	71	2·7	"	11.27 p.m.	62	70	58	2 in 15 m. -	2 in 15 m.
"				2·5 } 2·575	June 18	11.30 a.m.	57·5	68	50	20 in 10 m. 15 s.	2 in 1 m. 1 s.
"				2·6	"	5 p.m.	65	68	50	8 in 14 m. -	2 in 3 m. 30 s.
"				2·5	"	9 p.m.	57	64	50	18 in 16 m. -	2 in 1 m. 47 s.
June 14	1 p.m.	58·25	72	2·5	"	"	"	"	50	in 15 m. -	in 20.
"				2·5 } 2·55	June 19	10 a.m.	57	58	50	20 in 8 m. 15 s.	2 in 45 s.
"				2·7	"	10 a.m.	"	60	50	20 in 7 m. 30 s.	2 in 45 s.
"				2·5	"	10 p.m.	55·5	61	50	20 in 11 m. 4 s.	2 in 1 m. 6 s.
					"	"	55·5	64	50	15 in 15 m. -	2 in 2 m.

APPENDIX No. 6—*continued.*

Experiments made to determine the Influence of Temperature, &c.—By O. Rowland.—Table VII.—*cont.*

India Rubber—*continued.*

INDUCTION.					INSULATION.						
Date.	Hour.	Temperature Of Pipes.	Temperature Of Hut.	Discharge.	Date.	Hour.	Temperature Of Pipes.	Temperature Of Hut.	Full tension.	Loss of Insulation.	Loss of Insulation reduced.
1860.	*With Pressure.*			Mean.	1860.	*With Pressure.*					
June 16	10.30 a.m.	57	64	2·6, 2·6, 2·7 } 2·61	June 20	10 a.m.	60	68	50	20 in 9 m.	2 in 54 s.
,,					,,	3.22 p.m.	60	70	50	15 in 15 m.	2 in 2 m.
,,					,,	3.40 p.m.	,,	,,	50	17 in 15 m.	2 in 1 m. 45 s.
	With Pressure.				June 21	9.30 a.m.	58·7	68	50	18 in 15 m.	2 in 1 m. 40 s.
,,	10.30 a.m.	57	64	2·5, 2·6 } 2·55	June 22	9.20 a.m.	57	68	50	12 in 15 m.	2 in 2 m. 30 s.
,,										Packing bad.	
June 18	10.10 a.m.	55·5	66	2·5, 2·4, 2·5, 2·5 } 2·48		*Without Pressure.*				Renewed.	
,,					,,	12.40 p.m.	59·25	74	50	8 in 7 m.	2 in 1 m. 45 s.
,,					,,	1 p.m.	60·5	74	50	2 in 5 m.	2 in 5 m.
June 19	10 a.m.	57	60	3·1, 3·0, 3·0, 3·0 } 3·025	,,	,,	,,	,,	,,	1·8 in 5 m.	2 in 6 m. 6 s.
,,					,,	6 p.m.	,,	,,	,,	1·7 in 15 m.	2 in 17 m. 38 s.
,,					,,	9 p.m.	,,	78	,,	3 in 15 m.	2 in 10 m.
,,					June 23	3.30 p.m.	65	68	,,	15 in 7 m.	2 in 0 m. 56 s.
June 21	9.30 a.m.	58·7	68	2·5, 2·4, 2·5, 2·5 } 2·47	,,	,,	,,	,,	,,	10 in 5 m.	2 in 1 m.
,,					,,	,,	,,	,,	,,	2 in 5 m.	2 in 5 m.
,,					,,	,,	,,	,,	,,	4·5 in 10 m.	2 in 4 m. 2 s.
,,											
	Without Pressure.					*With Pressure*					
June 23	3.30 p.m.	65	68	2·5, 2·5 } 2·5	,,	5 p.m.	64·5	68	50	2 in 15 m.	2 in 15 m.
,,					,,	9.30 p.m.	62	66	50	30 in 45 s.	2 in 0 m. 34 s.
	With Pressure.									Packing bad.	
,,	5 p.m.	64	68	2·4, 2·5 } 2·45		*Without Pressure.*				Renewed.	
,,					June 26	10 p.m.	56	57	50	1 in 10 m.	2 in 20 m.
,,	10 p.m.	62	66	3·6, 3·5, 3·5, 3·5 } 3·52	June 27	2.25 p.m.	58·5	60	50	½ in 5 m.	2 in 20 m.
,,					,,	,,	58	60	50	½ in 5 m.	2 in 20 m.
,,					,,	,,	,,	,,	,,	½ in 5 m.	2 in 20 m.
,,					,,	,,	,,	,,	,,	½ in 5 m.	2 in 20 m.
,,	12 p.m.	60	66	2·3, 2·4, 2·3, 3·3 } 2·32	June 28	10 a.m.	58	66	50	1·8 in 15 m.	2 in 16 m. 40 s.
,,					,,	,,	,,	,,	,,	1·5 in 15 m.	2 in 20 m.
,,											
	Without Pressure.					*With Pressure.*					
June 28	12 a.m.	63	66	2·5, 2·5, 2·6 } 2·53	,,	4 p.m.	58·5	66	50	2 in 15 m.	2 in 15 m.
,,					,,	9 p.m.	54·7	56	50	½ in 5 m.	2 in 20 m.
,,					June 29	9 a.m.	56	66	50	1 in 15 m.	2 in 30 m.
	With Pressure.										
June 28	4 p.m.	58	66	2·8, 2·9, 2·9 } 2·82	Packing gave way, and the end of the wire hermetically sealed and left in pipe under pressure.						
,,					Pressure on from 24th May to 29th July. Wire taken out and examined in tub—						
,,											
June 29	9.20 am.	56	60	3·0, 2·8, 2·9, 3·0 } 2·92	Time.		Temperature of Water.	Temperature of Hut.	Full Tension.	—	
,,					1 p.m.		55	62	50	1·3 in 15 m.	
,,					,,		,,	,,	,,	1·2 in 15 m.	
,,											
June 30	9·30 am.	54·7	58	3·, 2·9, 3· } 2·96							

The *time* measurements of the loss of insulation in the gutta percha (special) covered wire were taken when the needle or indicator fell 8°, but in consequence of the very slow leakage of the india-rubber covered wire, it was determined to take the time measurements generally of the fall of the needle in 15 minutes.

GENERAL RESULT of the above EXPERIMENTS on HYDROSTATIC PRESSURE.

General results of the above experiments under pressure.

GUTTA PERCHA (SPECIAL), TABLE VII.—This material exhibited a great improvement in its insulating properties under pressure, which was well maintained for a period of 21 days, when the experiment was interrupted by an accident, fully described in note at foot of Table VII. The influence of the variableness in temperature of the pipes, C C, upon its insulation is manifested in a decided manner, the insulation property increasing or diminishing with the increase or diminution of temperature. The pressure was continued both night and day, with the exception of a few intervals, for a period of 11 weeks, during which time the induction discharge appears to have undergone a gradual and appreciable diminution.

On the final examination of the wire on the 23rd August (see woodcut, page 375, and remarks), the insulating qualities of the material seem not to have suffered from the effect of pressure, taking into consideration the difference of temperature between the 24th May, when first tested, and the above date. Its external appearance, with regard to uniformity of gauge, had not, apparently, been altered, but an additional firmness had evidently been imparted thereto.

GUTTA PERCHA (PLAIN), TABLE VII.—The influence of pressure and of temperature upon this material is much the same as on gutta percha (special). Its inductive capacity is but slightly affected. In the first experiment it will be observed that before and under pressure the readings of the loss of insulation and induction discharge remained the same. The insulation being very defective, but not actually destroyed, no improvement took place under pressure. Notes at foot of table explain the interruptions which brought the experiment to a close.

INDIA RUBBER (SILVER & Co.), TABLE VII.—This wire maintained a high degree of insulation both without and under pressure, and appeared not to be affected by the varying temperature of the pipe. A perceptible diminution of the induction discharge is also observable, as in the case of gutta percha (special). Pressure, to which it was subjected for a period of 11 weeks, appears to have improved its insulating properties, as the readings of the final test on the 29th July are higher than those of the 24th May, previously to its being put under pressure.

In the table of experiments it will be observed that several interruptions took place in consequence of the great difficulty experienced in effectually packing the wire in the gland, owing to its softness and elasticity. To these continually occurring interruptions must be attributed the otherwise seeming irregularities in the readings of the loss of insulation, as the wire was found injured in the gland on several occasions during the progress of the experiment. No alteration took place in its external shape. Its colour became rather lighter, and it became much firmer in texture.

WRAY'S COMPOUND, TABLE VII.—The effect of pressure upon this compound was considerably to improve its already superior insulating properties, and perceptibly to diminish its inductive capacity. The influence of the variableness of temperature upon it is exceedingly small. The experiments on the 3rd and 4th May may be taken as giving the true measurement of the loss of insulation, &c. In the subsequent experiments the accidents which occurred by the bursting of the pipes evidently impaired the soundness of the wire, the nature and extent of which injuries are explained in notes appended to Table No. VII., and illustrated by woodcuts, Fig. 1, A.

APP. No. 6.

APPENDIX No. 6—*continued.*

EXPERIMENTS ON INSULATION, &c.

Experiments on insulation, &c.

Impurities, Air Cells, Fissures, and Eccentricity of Conductors.
Gutta-percha Sheets.
Gutta-percha covered Wires.
India-rubber Sheets.
India-rubber covered Wires.

Cores of Submerged Cables:
England and Holland.
The Atlantic.
Subterranean Wires.
Elongation or Stretching of Cores:
Effect of Temperature.

The method adopted for testing sheets consisted of a metallic disc six inches in diameter, placed horizontally upon a plate of glazed porcelain. From the disc a copper wire was passed through a very delicate galvanometer to earth. The sheet was then laid over the disc, extending a few inches beyond its circumference. A copper rod, in contact with a battery of 512 elements, having one end slightly bent, and the other inserted in an insulating handle of ebonite,* was carefully passed over the surface of the sheet, corresponding in size with the metallic disc beneath, so that when the testing wire passed over a fault the passage of the current to earth would be instantly indicated on the instrument.

In the *gutta-percha sheets*, some of about the thickness of common tissue paper, others of about one thirty-second of an inch in thickness, the faults discovered were almost invariably owing to impurities in the shape of dark brownish spots, which occurred sometimes singly, at other times in groups. Examined with the aid of a microscope, these spots had the appearance of a decaying fibre. In a few instances the defective insulation of a sheet was owing to minute fissures, detected by thoroughly wetting the sheet with water, and upon a microscopic examination the edges of the fissures seemed as if cut with a sharp instrument, and not the effect of stretching. With the exception of the impurities or fissures, the insulation tests were generally uniform.

In sheets of *masticated india-rubber*, of about one thirty-second of an inch in thickness, on two occasions the insulation was deteriorated by impurities, but not by fissures.

GUTTA-PERCHA COVERED WIRES.—The method adopted for testing short pieces of covered wires consisted of a small porcelain vessel filled with water, and an earth-connecting wire. The short piece of covered wire was bent horse-shoe shape, and inserted in the water, leaving out about four inches of both ends, one end being insulated, the other brought into contact with a Peltier electrometer. The loss of insulation was then ascertained in the usual manner, as fully described in the experiments under pressure, 512 elements being employed.

In the very numerous pieces tested, inferior or faulty insulation was found to arise from either of the following causes:—impurities or extraneous matter, air cells, fissures, and the eccentric position of conductor. The tests otherwise exhibited an uniform insulating quality.

INDIA-RUBBER COVERED WIRES.—No defective insulation was, however, found arising from impurities, air cells, cavities, or eccentricity of conductor, in india-rubber covered wires.

Masticated rubber cores become much firmer by prolonged submersion in water, and in colour and structure much resemble the material in its virgin state.

CORES OF SUBMERGED WIRES.—About a dozen kinks were examined which had been formed in submerging the cable laid between England and Holland, and which had been submerged for about five years. The core was stripped of its iron and hemp sheathing, and it was found that the gutta-percha had been so compressed by the kinking as to reduce its thickness from four thirty-seconds of an inch to less than two thirty-seconds of an inch. The insulation equalled that of other pieces cut from the same cable which had not been affected by kinks, or otherwise, and was far superior to that of new plain gutta-percha.

A piece of the Atlantic core submerged near the coast of Newfoundland for upwards of two years was also tested against other lengths of new gutta-percha cores, and found far superior in its insulating capacities.

Gutta-percha when long submerged in sea water seems to assume a clearer, tougher, and denser appearance.

SUBTERRANEAN WIRES.—It was always found that the insulation of the gutta-percha was very deficient, *even in the parts best preserved*, when brought to the company's works from different parts of the town and country, many portions having absolutely decayed and become worthless for the purposes of insulation.

ELONGATION OR STRETCHING OF CORES.—In the manipulation of cores such as in drawing cables along subterranean pipes, or in paying out of vessels, the greatest precautions should also be taken to avoid violent stretching or elongation. The composition or structure of gutta-percha is materially deteriorated thereby, and the insulation often permanently impaired. Sometimes it has been found that a very slight elongation caused a very great diminution of insulation, owing probably to the distension of air-cells or cavities, or defective material. India-rubber, from its tenacity, is not so liable to be similarly affected, but in the course of experiments it has been found that violent stretching has an injurious effect upon its insulating properties, both in the shape of sheets and cores.

EFFECT OF TEMPERATURE.—In Table No. V. of experiments on the insulation for dynamic electricity, experiments 6 and 9, the loss and return of insulation under the influence of temperatures is shown, and from numerous additional tests made during the duration of the experiments, with the view of determining the highest degree of temperature to which gutta-percha (plain) and india-rubber covered wires might be subjected with safety, it is evident that gutta-percha should not exceed 95 degrees and india-rubber 140 degrees.

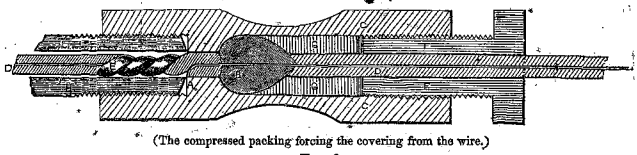
The general result of the above tests clearly demonstrates that superior insulation depends upon the purity and the homogeneousness of gutta-percha and india-rubber coatings respectively, and that to secure an uniform and permanent insulation, air cells, fissures, eccentricity of conductor, and violent stretching or elongation should be avoided in the manufacture and manipulation of telegraphic cores.

OWEN ROWLAND.

The following Woodcut explains the manner in which defects occurred in the Wires during the Experiments under Pressure.

Diagrams explanatory of the defects occurring in the experiments.

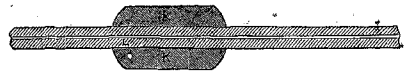
SECTIONS OF GLAND AND BURR OR PACKING.—FIG. 1.



(The compressed packing forcing the covering from the wire.)

FIG. 2.

C. Gland of gun metal.
D. Covered wire.
F. Packing screw.
G. Packing brass cylinder.
H. Iron longitudinal pipe.
I. Copper conducting wire.



K. Packing when placed in chamber.
L. Covered wire in centre of burr.
Wray's wire was injured at A.
Gutta percha wire was injured in packing box at B.
Wray's wire was coiled as at E.

FIG. 3.—Showing way in which wire was forced through pipe when pipe burst.

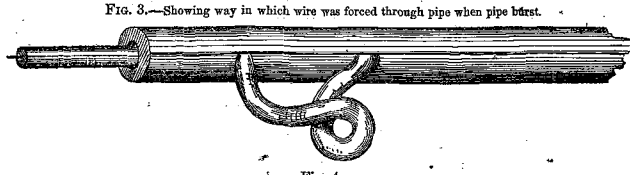
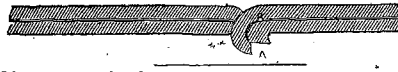


Fig. 4.



A. Injury.
B. Kink.

Description of machinery used in making pressure experiments.

DESCRIPTION of the MACHINERY employed in making the EXPERIMENTS to determine the INFLUENCE of HYDROSTATIC PRESSURE on various INSULATING MATERIALS.

PLATE, No. 2.

(A) The scale-board upon which were placed the weights regulating the amount of pressure imparted to the water in the longitudinal pipes (CC) by the hydraulic pump (B).

(B) The hydraulic pump. The water being forced through the ascending pipe into the cylinder (D), in iron frame, drove upwards the spindle or piston (E), which lifted up the regulating weights (A).

CC Iron pipes, ¼-inch bore, and 330 feet each in length, containing the wires under experiment, each wire having one end hermetically sealed, and the other led into testing box (F), containing a Peltier electrometer with suitable arrangements for communicating the full battery charge or tension to the wires, the box being furnished with a glass lid to observe the fall of the index of the electrometer, as it indicated in degrees the loss of the static charge or insulation in the wires.

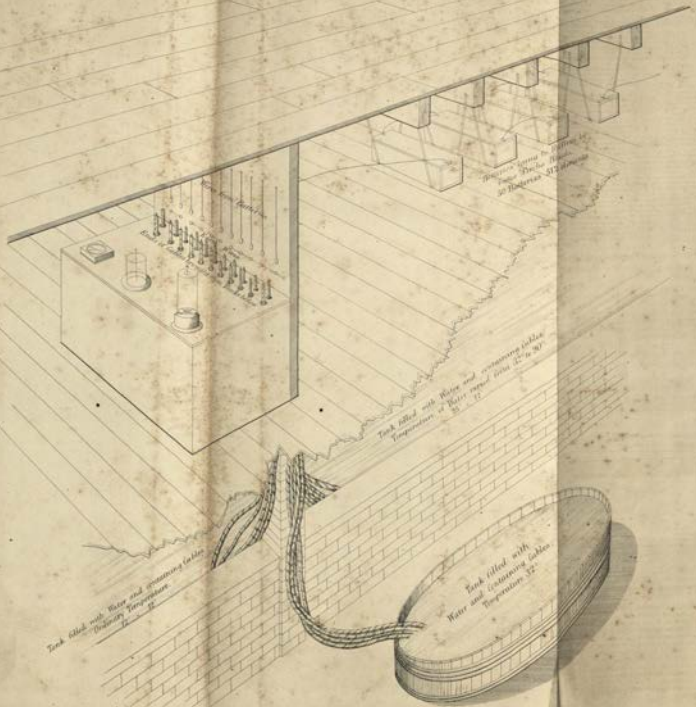
The measurements of the loss of insulation and the induction discharge were taken in the first instance in each experiment immediately before and after the pressure being put on.

Thermometers were fixed to the longitudinal pipes, and the degrees of temperature ascertained and noted at the commencement of each experiment.

OWEN ROWLAND.

* A material possessing superior insulating qualities.

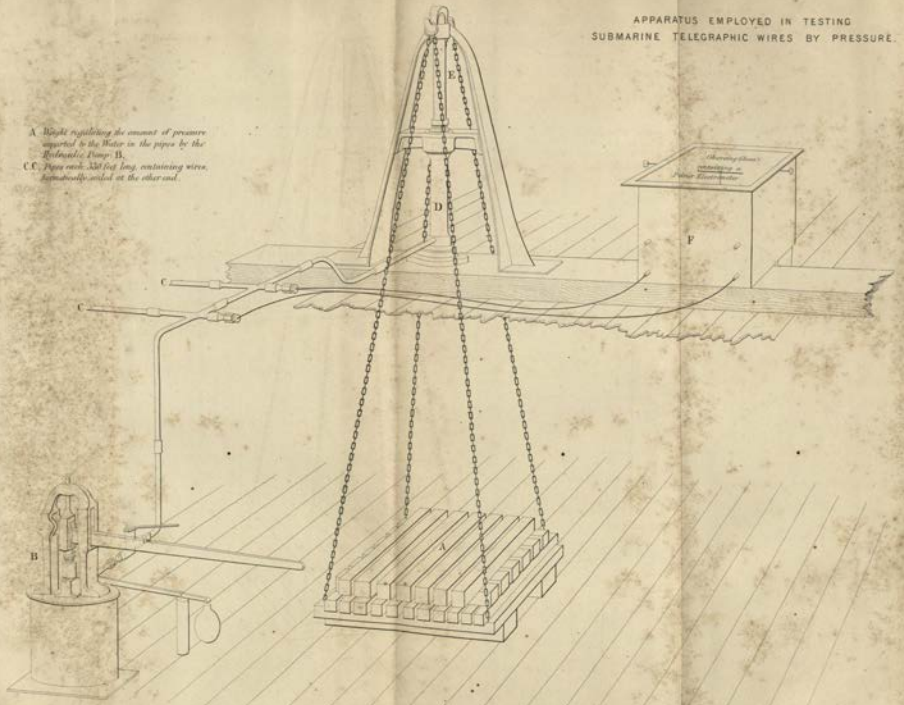
DESCRIPTION OF APPARATUS EMPLOYED IN TESTING SUBMARINE TELEGRAPHIC CABLES AT DIFFERENT DEGREES OF TEMPERATURE.


Tank filled with Water and submerging tables Temperature 32°
Tank filled with Water and submerging tables Temperature of Water raised from 32° to 80°
Tank filled with Water and submerging tables Temperature 32°

APPARATUS EMPLOYED IN TESTING SUBMARINE TELEGRAPHIC WIRES BY PRESSURE.

A. Weight regulating the amount of pressure exported by the Water in the pipes by the Hydraulic Pump: B.

C C. Pipes each 330 feet long, containing wires, hermetically sealed at the other end.



App. No. 7.

APPENDIX No. 7.

App. No. 7.

NOTES AND EXPERIMENTS BY DR. WERNER SIEMENS AND MR. WILLIAM SIEMENS.

Notes and experiments by Dr. Werner Siemens and Mr. William Siemens.

No. 1.—*Resistance of Short Cables.*

Notes and experiments by Mr. Werner Siemens and Mr. William Siemens.

Resistance of short cables.

One pole of a battery of n elements is joined to the cable while the other pole is to earth; then, if O represents the angle through which the galvanometer is turned to bring the needle again to zero, the following equation is established:—

$$\sin \phi = \frac{n\,E}{x + W_1}$$

E representing the electromotive power of one element and x the unknown resistance of the cable, and W_1 the resistance of the galvanometer.

In order to arrive at the actual value of the insulation resistances, a known resistance, say of 10,000 units, is introduced into the circuit instead of the cable, the sensibility of the instrument weakened (to $\frac{1}{100}$) by a branch resistance, W_2, and the number of cells reduced to one. Another equation is then obtained, in which I may represent the force of the current in the whole circuit to be

$$I = \frac{E}{10{,}000 + \frac{W_1 \cdot W_2}{W_1 + W_2}}$$ and the strength of the current

passing through the galvanometer will be

$$i_1 = \sin \phi_1 = \frac{W_2 \cdot E}{W_1 + W_2} \cdot \frac{1}{10{,}000 + \frac{W_1 \cdot W_2}{W_1 + W_2}}$$

and because $W_1 = 99\ W_2$

$$i_1 = \sin \phi_1 = \frac{E}{100} \cdot \frac{1}{10{,}000 + \frac{99}{100} W_2}$$

W_2 being with our instruments equal to 70 units, we obtain by introducing instead of 10,000 units 9,930,

$$\sin \phi_1 = \frac{E}{100} \cdot \frac{1}{10{,}000}$$

Estimating E from the first formula, and combining it with the second one, we obtain

$$x = n \frac{\sin \phi_1}{\sin \phi}$$

in which formula x is given in millions of units.

No. 2.—*Specific Resistance of Insulating Materials.*

Derivation of the formula for calculating the *specific* insulation resistance.—Mr. Werner Siemens obtained the same formula which Professor William Thomson arrived at in a very elegant manner in a more simple way.

If $d\,x$ represents the thickness of a differential cylinder at the distance x along the length axis of the cable, its resistance will be

$$d\,w = \frac{d\,x}{2\,\pi\,l\,\lambda\,x}$$

and the whole resistance equal to

$$W = \frac{1}{2\,\pi\,l\,\lambda} \int_r^R \frac{d\,x}{x} = \frac{l\,n\,\frac{R}{r}}{2\,\pi\,l\,\lambda}$$

No. 3.—*Table of Specific Induction.*

Table of specific induction.

In the tables containing the results of the experiments of specific induction, the following is the explanation of the different columns:—

Column No. 2 contains the descriptions of the cables subjected to the test.

No. 3 the lengths thereof.

Nos. 4 and 5 the outer and inner radius of the coverings.

No. 7 gives the temperature of the water, in which the cables are submerged.

No. 8 the number of elements employed for the test.

No. 9 the angle through which the instrument had to be turned to bring the needle again to zero.

No. 10 the constant of the instrument.

No. 11 the charge observed by the instrument reduced to one element and to the unit of length, and

No. 12 the specific induction of the different materials, the mean specific induction of gutta-percha taken for unit.

TABLE of SPECIFIC INDUCTION of DIFFERENT MATERIALS.

Cable.			Radius.			Temperature.	Number of Cells Tension.	Deflection ϕ.	Constant of the Instrument.	$\frac{\sin \phi}{l\,n}$	Specific Induction. (Gu. P. = 1.) $\frac{l\,n\,\frac{R}{r}}{2\,\pi\,l} C$
No.	Material.	Length.	Inner r.	Outer R.	$\frac{R}{r}$						
1	Gutta Percha	A. $\frac{1}{2}$ m.	4	16	4	51°	7	20'3	1'	9'92	1'06
„	„	„	„	„		72	5	25'	1'7	9'82	1'07
2	„	B. $\frac{1}{2}$ m.	„	„	„	51	7	20'1	1	9'9	1'06
3	„	A + B	„	„	„	„	7	43'4	1	9'8	1'05
—	„	„	„	„	„	„	5	29'1	1	9'7	1'04
—	„	„	„	„	„	„	3	16'5	1	9'5	1'02
4	„	A. $\frac{1}{2}$ m.	2	14	7	„	7	14'5	1	7'14	1'07
5	„	B. $\frac{1}{2}$ m.	„	„	„	„	„	14'2	„	7'0	1'05
6	„	A. + B.	„	„	„	„	„	28'5	„	6'8	1'048
7	„	A. $\frac{1}{2}$ m.	4	10	2·5	„	3	12'3	„	14'2	1
8	„	B. $\frac{1}{2}$ m.	„	„	„	„	„	12'2	„	14'2	1
9	„	C. $\frac{1}{2}$ m.	„	„	„	„	„	12'5	„	14'4	1'002
—	„	„	„	„	„	72	2	15'4	1'7	14'5	1'003
10	„	D.	„	„	„	51	3	12'5	1	14'4	1'002
—	„	„	„	„	„	72	2	15'4	1'7	14'5	1'003
11	„	A. + B.	„	„	„	51	3	25'6	1	14'4	1'0
12	„	A. + B. + C.	„	„	„	„	„	40'3	„	„	1'0
13	„	A. + B. + C. + D.	„	„	„	„	„	61'0	„	14'38	0'999
14	„	A. 1 m.	2	8	4	„	„	16'2	1	9'3	1'
—	„	„	„	„	„	72	2	17'8	1'7	9'4	1'2
15	„	B.	„	„	„	51	3	16'6	1	9'5	1'208
—	„	„	„	„	„	72	„	31'1	1'7	9'4	0'99
—	„	„	„	„	„	92	„	25'9	1'45	—	0'99

App. No. 7. Notes and experiments by Dr. Werner Siemens and Mr. William Siemens.

Appendix No. 7—*continued.*

Table of Specific Induction of different Materials—*continued.*

Cable. No.	Material.	Length.	Radius. Inner r.	Outer R.	$\frac{R}{r}$	Temperature.	Number of Cells Tension.	Deflection ϕ.	Constant of the Instrument.	$\frac{\sin \phi}{l n}$	Specific Induction. (Gu. $\frac{R}{r} = 1$.) $\frac{l n \frac{R}{r}}{2 \pi l} C$
16	Gutta Percha	A. + B.	,,	,,	,,	51	,,	34·6	1	9·38	1·003
17	,,	A 1 m.	1	7	7	51	3	11·4	1	6·58	0·99
—	,,	,,	,,	,,	,,	72	,,	20·6	1·7	6·4	0·99
—	,,	,,	,,	,,	,,	92	,,	17·7	1·45	6·5	1·2
18	,,	B. 1 m.	,,	,,	,,	51	,,	11·4	1	6·58	0·99
—	,,	,,	,,	,,	,,	72	5	31·8	1·7	6·49	1
19	,,	A. + B.	,,	,,	,,	51	3	22·8	1	6·47	0·98
20	,,	A. 1 m.	1	4	4	51	,,	16	1	9·18	0·99
—	,,	,,	,,	,,	,,	72	2	19·4	1·7	9·2	1·02
21	,,	B.	,,	,,	,,	51	3	16·	1	9·18	0·99
—	,,	,,	,,	,,	,,	72	5	28·4	1·7	9·2	0·99
—	,,	,,	,,	,,	,,	92	3	24·6	1·45	9·19	0·89
22	,,	A. + B.	,,	,,	,,	51	3	32·8	1	9·03	0·97
23	,,	A. ½ m.	1	13	13	,,	,,	4·8	,,	5·6	1·12
—	,,	,,	,,	,,	,,	72	5	16·2	1·7	5·5	1·09
—	,,	,,	,,	,,	,,	92	5	14·9	1·45	5·5	1·09
24	,,	B. ½ m.	,,	,,	,,	51	3	4·8	1	5·6	1·12
25	,,	C. ,,	,,	,,	,,	,,	,,	4·8	1	5·6	1·12
26	,,	D. ,,	,,	,,	,,	,,	,,	4·8	,,	5·6	1·22
27	,,	A. + B.	,,	,,	,,	,,	,,	9·2	,,	5·3	1·10
28	,,	A. + B. + C.	,,	,,	,,	,,	,,	13·	,,	5·2	1·
29	,,	A. + B. + C. + D.	,,	,,	,,	,,	,,	17·9	,,	5·12	1·1
30	Hearder	450 yards	2	6	3	,,	,,	5	,,	13·	1·24
31	Wray	A. 1 m.	1	7	7	,,	5	15	,,	5·17	0·77
—	,,	,,	,,	,,	,,	72	3	16·1	1·7	5·2	0·625
—	,,	,,	,,	,,	,,	92	3	,,	1·45	5·4	0·96
—	,,	,,	,,	,,	,,	51	,,	8·8	1	5·1	0·77
32	Silver	B. 1 m.	1	6·2	6·2	51	5	13·9	1	4·8	0·67
—	,,	,,	,,	,,	,,	72	3	14·5	1·7	4·9	0·696
—	,,	,,	,,	,,	,,	92	,,	12·6	1·45	5·	0·705
—	,,	,,	,,	,,	,,	51	5	14·2	1	4·8	0·68
—	,,	,,	,,	,,	,,	72	,,	18·1	1·7	4·8	0·619
33	,,	C. 1 m.	1	7·2	7·2	51	,,	12·8	1	4·45	0·66
—	,,	,,	,,	,,	,,	72	3	12·9	1·7	4·3	0·518
33	Silver	C. 1 m.	1	7·2	7·2	92	3	11·6	1·45	4·7	0·696
34	,,	,,	1	,,	,,	51	5	13	1	4·5	0·69
—	,,	C. + B. 2 m.	,,	,,	,,	,,	,,	27·6	1	4·65	0·69
—	,,	,,	,,	,,	,,	,,	,,	27·6	,,	,,	,,
35	Hall & Wells	A. 1 m.	,,	4	4	51	5	34·6	1	11·3	1·21
—	,,	,,	,,	,,	,,	72	1½	18·6	1·6	11·6	1·38
—	,,	,,	,,	,,	,,	92	3	32·9	1·6	11·6	1·32
—	,,	,,	,,	,,	,,	51	5	35·2	1·	11·5	1·21
36	,,	B. 1 m.	,,	,,	,,	,,	,,	·25	1·	8·45	0·92
—	,,	,,	,,	,,	,,	72	3	26·8	1·7	8·5	0·91
—	,,	,,	,,	,,	,,	92	3	23·8	1·45	8·5	0·997
—	,,	,,	,,	,,	,,	51	5	24·2	1·	8·2	0·92
37	Hughes	A. 1 m.	1	6	6	51	5	16·4	1·	5·66	0·83
—	,,	,,	,,	,,	,,	72	3	17·8	1·7	5·67	0·84
—	,,	,,	,,	,,	,,	92	3	16·2	1·45	5·65	0·88
—	,,	,,	,,	,,	,,	51	5	16·6	1·	5·66	0·83
38	Many coatings.	A. 1 m.	1	7	7	51	5	19·3	1·	6·61	1·0
—	,,	,,	,,	,,		,,	,,	,,	1·	6·61	1·0
—	,,	,,	,,	,,	,,	72	3	20·2	1·7	6·62	1·03
—	,,	,,	,,	,,	,,	92	,,	17·7	1·45	6·62	1·04

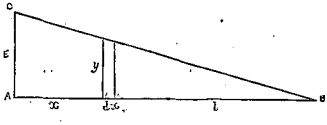
APPENDIX No. 7.—*continued.*

Table of Specific Induction of different Materials—*concluded.*

Cable. No.	Material.	Length.	Radius. Inner r.	Outer R.	$\frac{R}{r}$	Temperature.	Number of Cells Tension.	Deflection ϕ.	Constant of the Instrument.	$\frac{S u \phi}{l n}$	Specific Induction. (Gu. P. = 1.) $\frac{l n \frac{R}{r}}{2 \pi l}$ C
39	Gutta Percha special.	A. 1 m.	1	4	4	51	5	21·2	1'	7·23	0·74
—	,,	,,	,,	,,	,,	72	3	20·6	1·7	7·23	0·74
—	,,	,,	,,	,,	,,	92	,,	18·1	1·45	7·3	0·766
—	,,	,,	,,	,,	,,	51	5	21·4	1'	7·29	0·745
40	,,	B. 1 m.	,,	,,	,,	,,	,,	20·1	1	6·9	0·74
—	,,	,,	,,	,,	,,	72	3	20·9	1·7	6·93	0·754
—	,,	,,	,,	,,	,,	92	,,	18·4	1·45	6·9	0·75
—	,,	,,	,,	,,	,,	51	5	20·3	1	6·95	0·74
41	,,	C. 1 m.	1	7	7	51	,,	20·4	,,	,,	0·74
42	Radcliff	A. 1 m.	1	7	7	51	5	21·2	1'	7·23	1·09
—	,,	,,	,,	,,	,,	72	3	22·1	1·7	7·3	1·1
—	,,	,,	,,	,,	,,	92	3	18·8	1·45	7·3	1·1
—	,,	,,	,,	,,	,,	51	5	21·4	1	7·23	1·1
43	,,	B. 1 m.	1	7	7	51	5	22·4	1	7·62	1·16
44	,,	,,	,,	,,	,,	72	3	22·9	1·7	7·63	1·15
—	,,	,,	,,	,,	,,	92	,,	19·4	1·45	7·63	1·15
—	,,	,,	,,	,,	,,	51	5	22	1	7·59	1·14
45	Godefroy	1 m.	1	4	4	51	,,	30·6	1	10·18	1·08
—	,,	,,	,,	,,	,,	51	5	31	1	10·3	1·1
—	,,	,,	,,	,,	,,	72	1½	15·4	1·7	10·3	1·12
—	,,	,,	,,	,,	,,	92	3	28·8	1·45	10·6	1·12
46	Gutta Percha	1 m.	,,	,,	,,	51	5	28·6	1	9·57	1·1
—	,,	,,	,,	,,	,,	,,	,,	31·0	1	9 57	1·06
47	Siemens	500 yards	,,	,,	,,	51	5	8·1	1·7	7·3	0·77
—	,,	390 yards	,,	,,	,,	72	5	6·5	1·45	7·4	0·78

Charge and distribution along the wire.

No. 4.—*Charge and Distribution along the Wire.*



Let A B represent a given length l of uncoiled cable, the end B of which is to earth, and A C the electro-motive force E of a battery, one pole of which is in connexion with A, the other pole being to earth. Then, according to the laws of Ohm, supposing the cable to be of equal section and conductivity throughout, the curve of the electro-motive force at any point along the line is indicated by C B.

In 1849 (see Poggendorff's Annalen), Werner Siemens proved that when a current is sent through a submerged cable, a quantity of electricity is retained in charge along the whole surface, being distributed proportional to the tension of each point. Thus, the tension of the electricity on any small intermediate given length, dx of the conductor at the distance x from a may be represented by y, the quantity of electricity dq, by which the outside cylinder dx is charged according to the formula given previously for induction in cables:—

$$dq = yk = y\,2\,\frac{I\pi dx}{ln\frac{R}{r}}$$

This quantity of electricity dq has to pass through the resistance of section x, in order to arrive at dx.

The resulting current developes dq in the time dt, and we have accordingly the equation—

$$dq = \frac{\frac{Edt}{dx}}{r^2\pi\lambda} = \frac{Er^2\pi\lambda}{x}\,dt$$

By combining these formulæ, a differential equation is obtained:— Charge and distribution along the wire.

$$\frac{y\,.\,2\,I\pi\,dx}{ln\frac{R}{r}} = \frac{Er^2\pi\lambda}{x}\,dt$$

by elimating y and substituting its equivalent:—

$$E : y = l : l-x$$
$$y = \frac{E(l-x)}{l}$$

into the differential equation, we obtain:—

$$dt = \frac{2\,I}{l\,.\,r^2\,ln\,.\,\frac{R}{r}\,.\,\lambda}\,.\,x\,(l-x)\,dx$$

$$t = \frac{2\,I}{l\,.\,r^2\,ln\,\frac{R}{r}\lambda}\cdot\int_0^l x\,(l-x)\,dx$$

$$= \frac{2\,I}{l\,.\,r^2\,ln\,\frac{R}{r}\lambda}\cdot\left(\frac{l^3}{2}-\frac{l^3}{3}\right)$$

$$t = \frac{I\,.\,l^2}{3\,r^2\lambda : ln\,\frac{R}{r}}$$

No. 5.—*Table of Insulation of different Materials.*

Table of insulation of different materials.

The annexed table gives specific resistances. The figures are arrived at by means of the formula given in Appendix 1 and 2, from deflections due to a given number of cells, and a certain constant of sensibility to be multiplied by 10^{10}.

These figures, therefore, represent, in trillions of units, the specific resistances as compared to that of mercury. Certain variations may be partly due to the application of the high battery power, which the cables had been previously subjected to, and to insufficient time allowed for the heat to penetrate through the coiled cables, and to various other causes.

Notwithstanding these irregularities, however, the wide differences between gutta-percha and india-rubber and Wray's mixture are very apparent.

APPENDIX No. 7—*continued.*

App. No. 7.

Notes and experiments by Dr. Werner Siemens and Mr. William Siemens.

TABLE OF SPECIFIC RESISTANCES OF INSULATED MATERIALS.

MATERIALS.	B.	r.	32° F. Number of Elements.	32° F. Deflection.	32° F. Specific Resistances.	52° F. Number of Elements.	52° F. Deflections.	52° F. Specific Resistances.	72° F. Number of Elements.	72° F. Deflections.	72° F. Specific Resistances.	92° F. Number of Elements.	92° F. Deflections.	92° F. Specific Resistances.
Gutta-Percha -	B.	13	1	—	—	—	256	5·6	0·916	64	16	0·5	64	19·2	0·325
	A.	„	„	—	—	—	„	3·6	1·45	„	12·5	0·637	„	16·9	0·368
	A.	7	1	512	2·8	4·40	128	2·6	2·63	„	21·9	0·969	32	20·2	0·407
	B.	4	1	—	—	—	—	—	—	„	36·7	0·85	16	26·5	0·221
	B.	8	2	—	—	—	64	2·4	1·89	„	70·8	0·538	„	21·6	0·264
	A.	10	4	—	—	—	256	13·2	1·09	„	30·6	0·996	„	11·7	0·393
	B.	„	„	—	—	—	256	6·4	2·23	„	21·9	1·03	„	9·6	0·447
Silver - -	B.	7·2	1	—	—	—	256	1·4	48·9	512	4·4	41·3	512	4·6	31·0
	C.	6·2	1	—	—	—	„	1·2	52·4	„	3·4	49·0	„	5·3	24·3
Wray - - -		7	1	—	—	—	„	2·6	23·6	„	6·4	26·0	„	15·7	38·4
Siemens - -		4	1	—	—	—	„	0·5	38·4	„	1·1	49·55	„	0·9	38·4
Gutta-Percha Special -	A.	4	1	—	—	—	128	21·4	2·25	64	21·1	8·25			
	B.	4	1	—	—	—	„	4·7	6·44	—	6	4·86	32	1·3	8·88
	C.	7	1	—	—	—	512	5·5	22·9	—	—	—	„	8·6	1·55
Hughes - -		6	1	—	—	—	64	16·7	1·05	8	18·9	0·153	8	11·6	0·191
Many coatings -		7	1	—	—	—	256	8·	7·86	256	21·4	3·98	32	12·8	0·696
Hall and Wells	A.	4	1	—	—	—	64	faulty.							
	B.	„	1	—	—	—	128	—	—	128	8·3	5·53	256	27·5	3·41
Radcliff's -	A.	7	1	—	—	—	Faulty.			2	15·9	0·641	2	11·2	0·657
	B.	„	1	—	—	—	16	8·	0·48	16	15·0	0·350	16	8·6	0·642
Godefroy - -		„	1	—	—	—	—	—	—	16	14·9	0·496	16	13·2	0·432

The absorption of water by gutta percha, India rubber, &c.

No. 5.—*The Absorption of Water by Gutta Percha, India-Rubber, and Wray's Mixture under various circumstances and when immersed in Distilled Water, Sea Water, and Brine.*—See Lithographed Tables.

We have made a series of observations, extending over several months, on the rapidity and extent to which caoutchouc (india-rubber), Wray's mixture, and gutta percha absorb water under various circumstances. The following diagrams give the results of these observations.

The horizontal distances represent the time of immersion in days.

The vertical distances give the increase of weight observed, each division representing 10 grains, and the original weight of each piece of material being=1000 grains.

The *black lines* give the increase of weight when the material was immersed in *pure distilled water* of the temperatures marked upon the diagrams; viz., 39°, 65°, and 120° Fahr. respectively.

The *blue lines* give the increase of weight when a similar piece of the same material was steeped into *sea water containing 3 per cent. of salt.*

The *red lines* represent the increase of weight when the same material was, under similar circumstances, immersed in *concentrated brine.*

When no pressure is mentioned, it is to be understood that the immersion took place under atmospheric pressure (or 15 lbs. indicated). When 50 *lbs. pressure* per square inch was used, the result is represented by *broken lines*, and when under *vacuum dotted lines* are made to represent the rate of absorption.

When no temperature is marked, the temperature of the room, which was regulated to 65° Fahr. is implied.

Whenever the increase of weight exceeded the limits of the scale of 100 grains, the curve representing the increase is set down again to the zero line. Therefore 100 grains must be added to the values expressed by the second ascending curve, and 200 grains to those expressed by the curve when set down twice.

The material was used in the form of sheets of the thicknesses given upon the diagrams.

The following is an enumeration of the principal results obtained.

1. All the materials examined absorb water more rapidly from *pure water* than from *sea water*, and more rapidly from *sea water* than from *brine*.

In gutta percha this difference is very decided, but the least remarkable, the absorption being about *three* times greater in sea water, and *five* times greater in pure water than in brine.

Caoutchouc absorbs water somewhat less rapidly than gutta percha at 39° Fahr.; which being the point of maximum density of water may be taken for the temperature of the deep sea.

Wray's mixture absorbs, on the contrary, at the same rate from *sea water* as from *pure water*, that rate being nearly equal to that of gutta percha, at the lower temperature of 39° Fahr.

2. Increase of temperature affects the rate of absorption by gutta percha only very moderately, that rate being little more than doubled for an increase from 39° to 120° Fahr.

Caoutchouc absorbs from brine at about the same rate at 39° and at 120° Fahr. From sea water it absorbs at double the rate at 120°, and in pure water the rate of absorption is as much as eight times greater at 120° than at 39° Fahr.

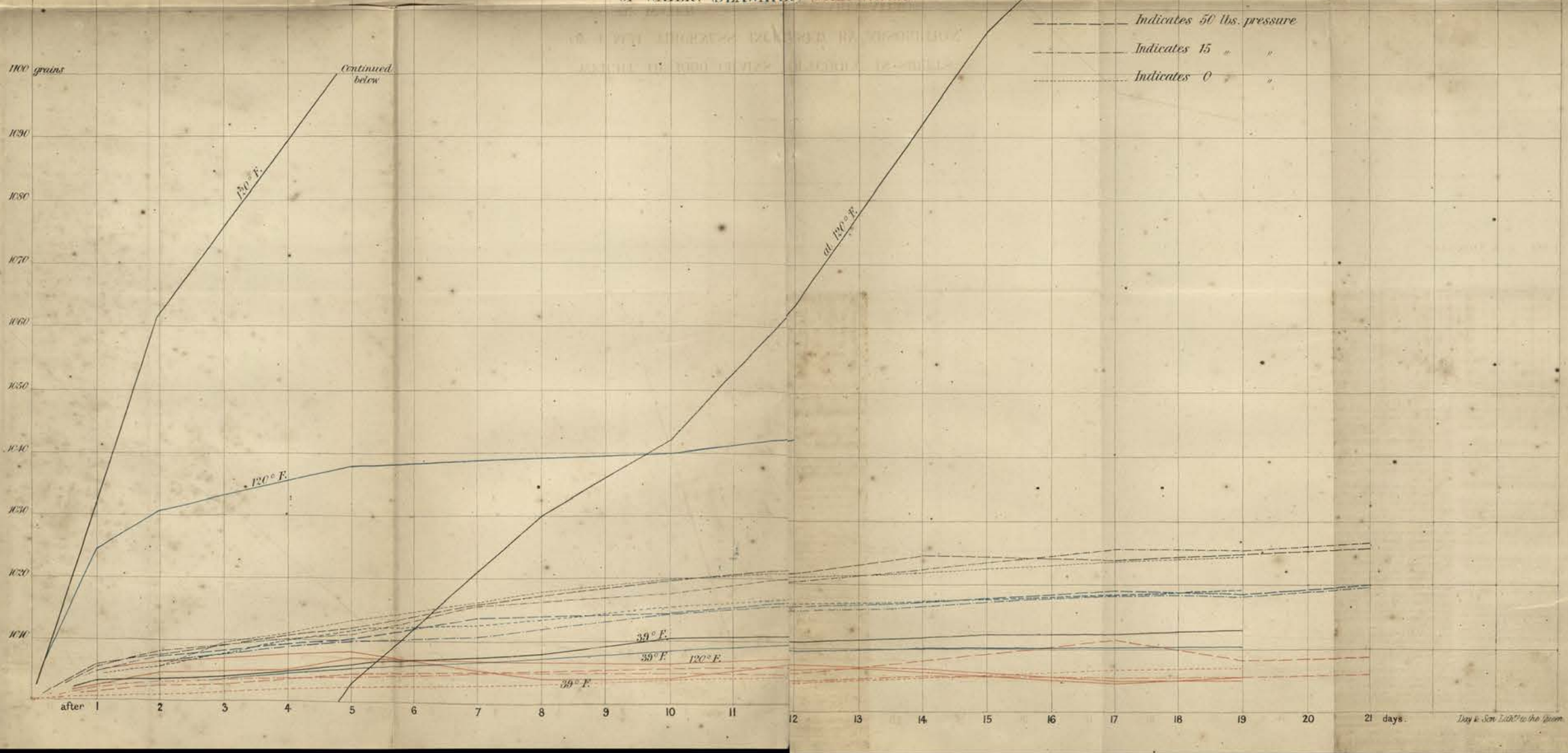
In regard of Wray's mixture, the rate of absorption from sea water is not materially influenced by temperature; but in pure water the rate of absorption increases in a very extraordinary ratio with increase of temperature, being about 16 times greater at 120° than at 39° Fahr. Both caoutchouc and Wray's mixture change colour and lose firmness when exposed for many days to pure water of 120° Fahr.

3. Increase of pressure within the limits of 50 lbs. per square inch does not affect the rate of absorption materially. The rate is somewhat higher under a vacuum, owing probably to the absence of condensed air upon the surface.

4. The thickness of the sheets of material employed affect the absorption in a much higher ratio than can be accounted for by the simple increase of surface. The absorption of water by the thicker sheets appears to stop short at a limit which is rapidly exceeded by the thin sheets.

5. The conductivity of these materials appears not to be materially affected by the absorption of two to three per cent. of water which may be taken for a limit that will not be exceeded in sea water and at ordinary temperatures. We intend, however, to extend our inquiry in this direction.

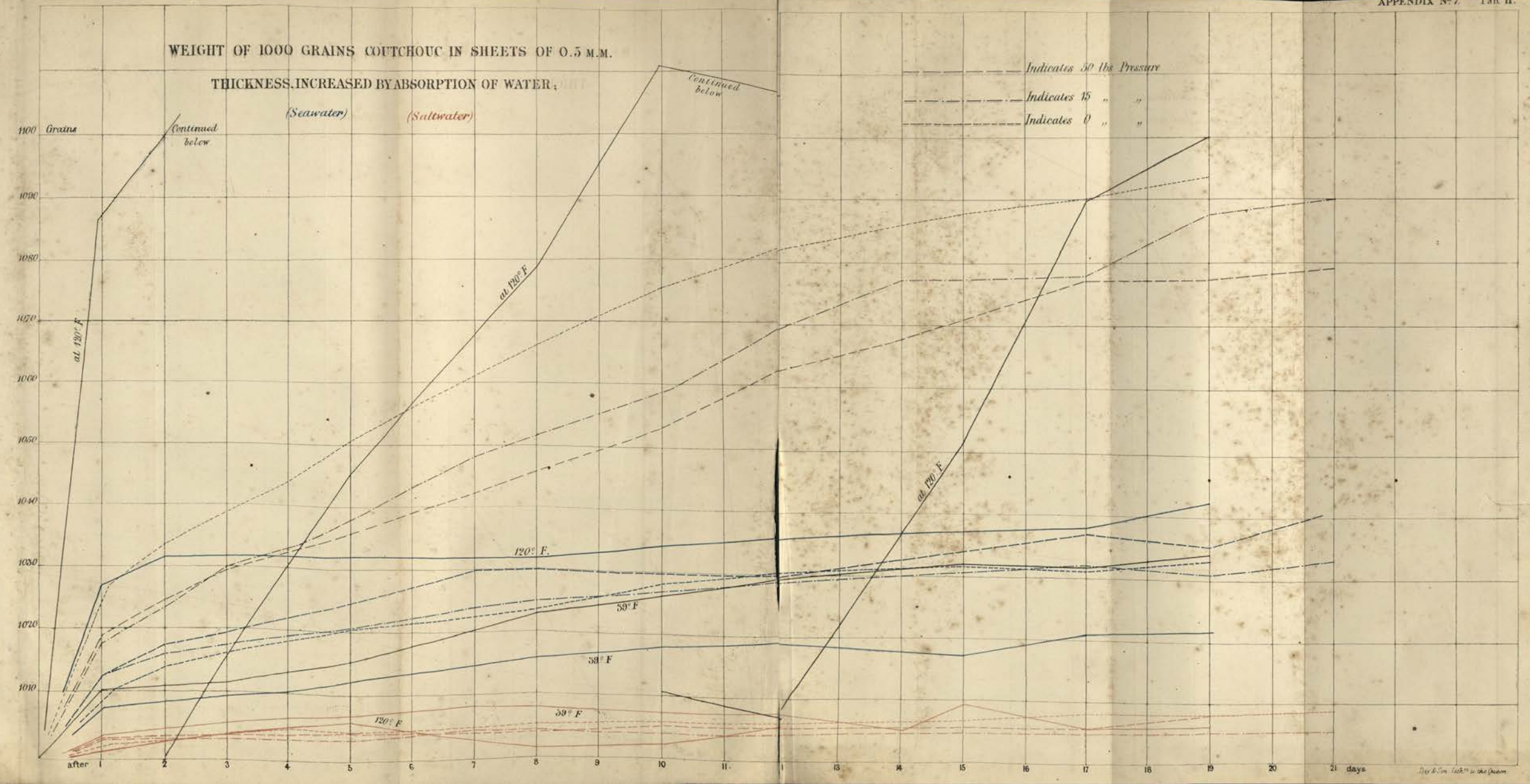
WEIGHT OF 1000 GRAINS CAOUTCHOUC IN SHEETS OF 1 M.M. THICKNESS INCREASED BY ABSORPTION OF WATER. (SEAWATER) (SALTWATER)



Day & Son Lith.rs to the Queen

WEIGHT OF 1000 GRAINS COUTCHOUC IN SHEETS OF 0.5 M.M. THICKNESS, INCREASED BY ABSORPTION OF WATER.

(Seawater) (Saltwater)



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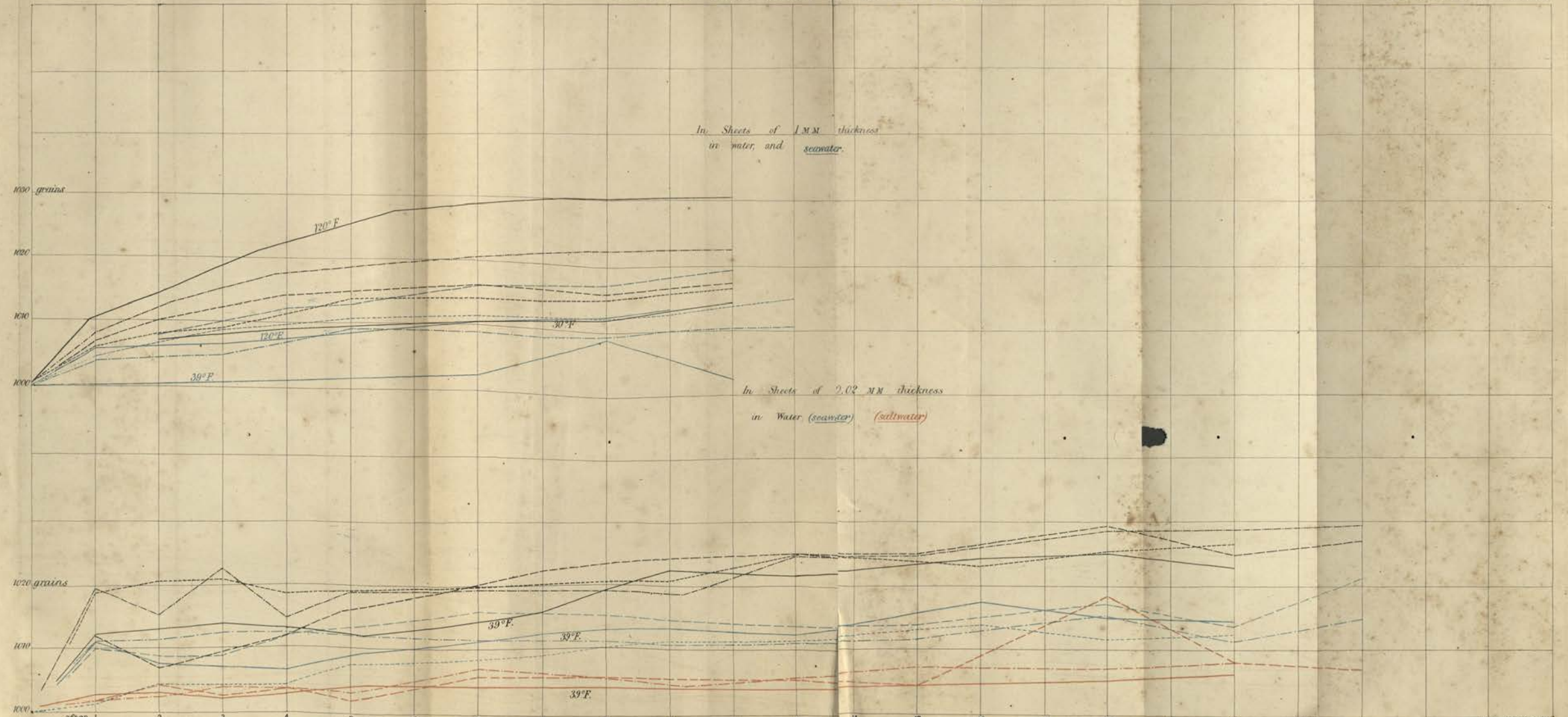
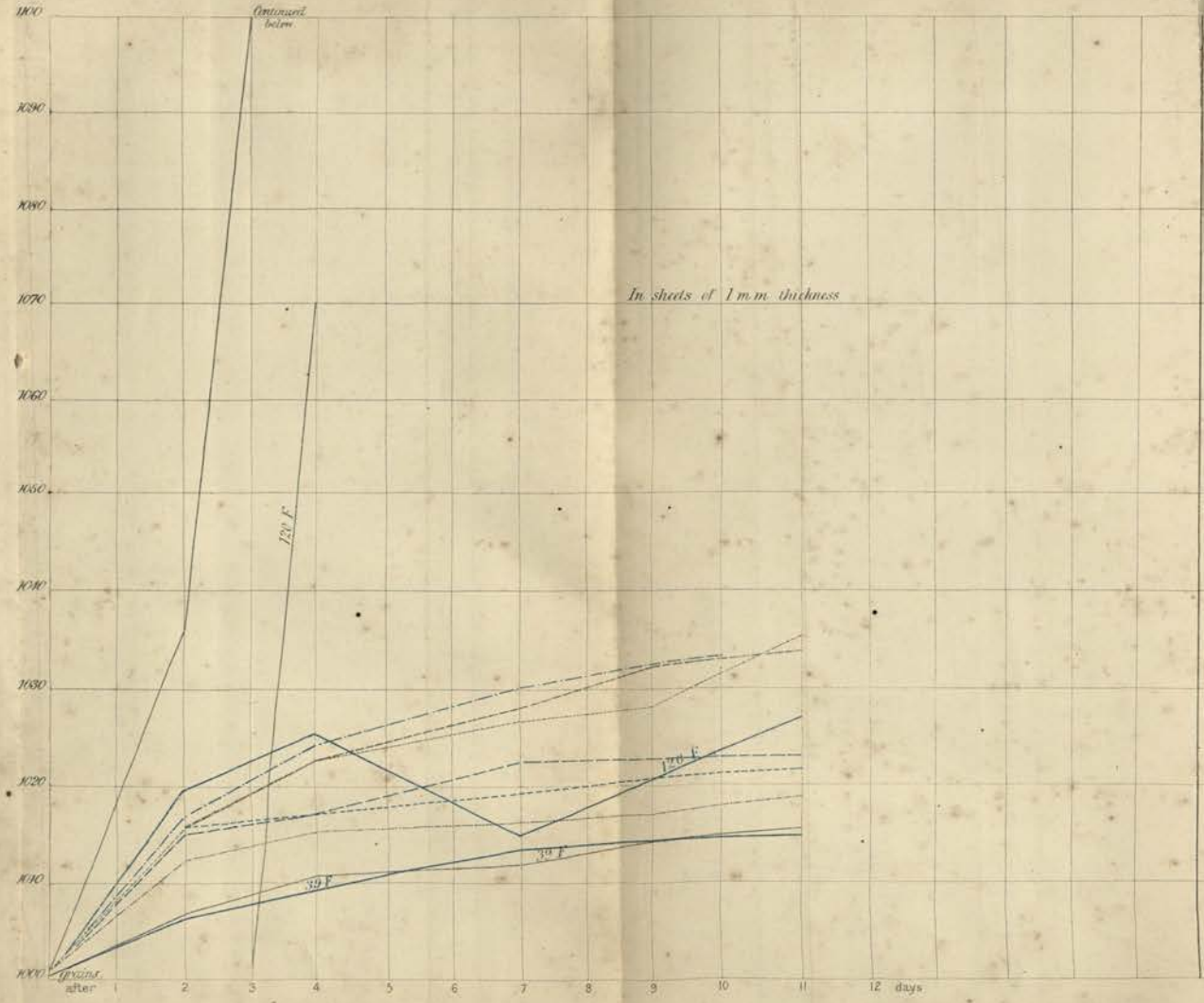
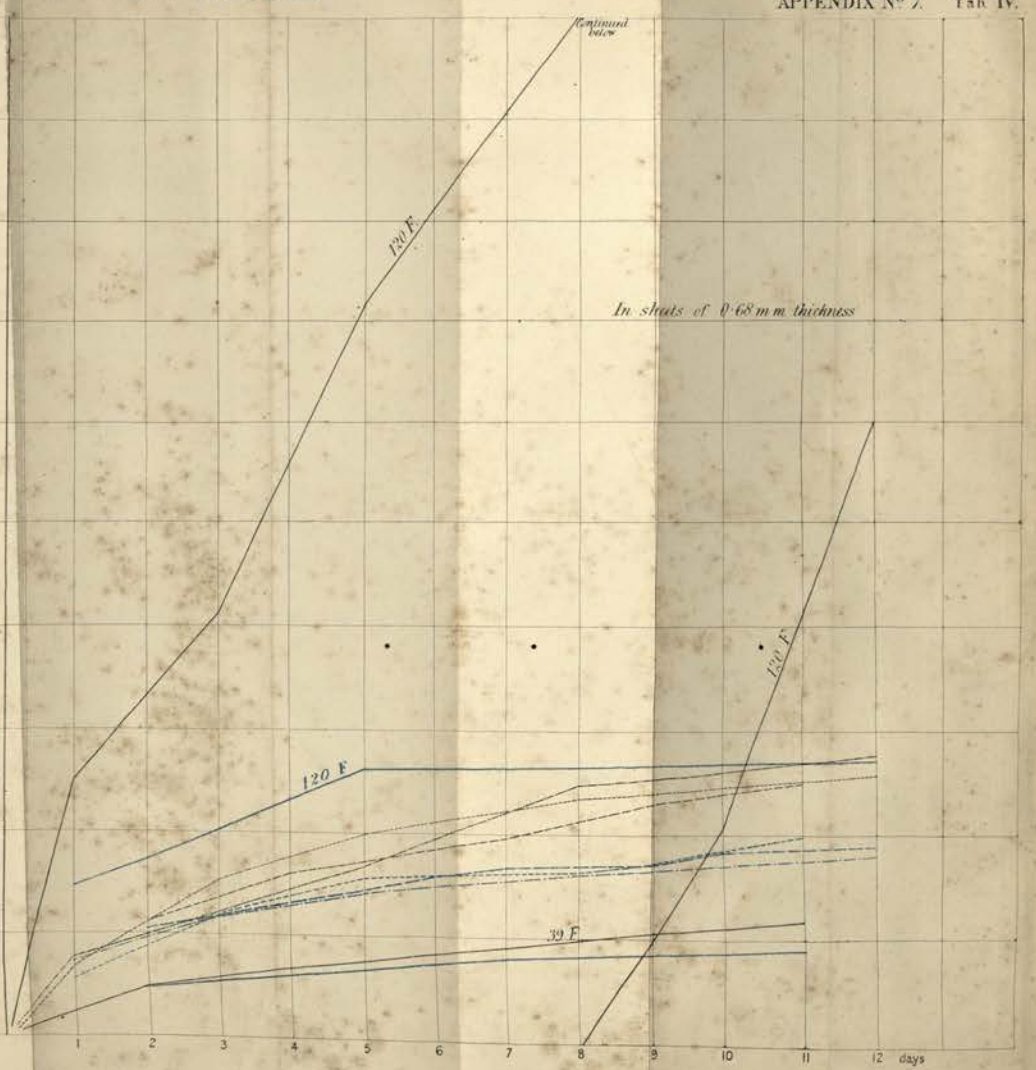
WEIGHT OF 1000 GRAINS OF GUTTA PERCHA.



WEIGHT OF 1000 GRAINS OF WRAYS MIXTURE.

IN WATER AND SEAWATER.





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APPENDIX No. 8.

EXPERIMENTS upon GUTTA-PERCHA as an INSULATOR for SUBMARINE ELECTRIC CONDUCTORS, by JOHN CHATTERTON and WILLOUGHBY SMITH.

Gutta Percha Works,
18, Wharf Road, City Road,
London, January 11th, 1861.

SIR,

SINCE we had the honour to be examined before the Committee, some important facts have been developed bearing upon gutta-percha as an insulator for submarine electric conductors, which we think will be interesting to the Committee. We may add that improvements have recently been made in the preparation of the materials employed, and also in the machinery for putting on the several coatings of gutta-percha; and we can with the greatest confidence state, that any number of miles can now be produced with certainty of uniform electrical resistance, and we think practically perfect.

We have also removed the *only* objection urged against the use of strand conductors, which is their want of solidity. To accomplish this, we cause the centre wire of the strand to pass through and be coated with compound, into which the six outer wires become embedded in the process of "twisting." The compound, which is forced through the interstices of the outer wires, becomes firmly united to the first coating of insulating materials, and thus the whole is rendered perfectly solid, as proved by the fact that a few inches of this strand will prevent the passage of water at a pressure of 600lb. to the square inch. We believe this to be the best form of conductor, as it can be made in any length, without a joint in all seven wires at once.

We wish we could feel equally confident that in the subsequent processes of manufacturing and laying a cable, the core could be preserved from all injury; but knowing that cables have worked well for a time, and then, without any apparent cause, failed, and knowing also, that gutta-percha itself undergoes no change whatever in sea water, we have endeavoured to ascertain how these failures have arisen, and from a series of experiments (particulars of which are appended) we feel perfectly satisfied that accidental mechanical injuries in the core, whereby the insulation has been affected, have been temporarily repaired by the yarn serving being saturated with tar, and have stood the test and passed into the cable without detection. The result is obvious, for, according to the nature of the injury, so is it a question of time only to the failure of the line. Even a wilful injury, such as the introduction of a nail, or other sharp instrument, into the core, might pass unobserved until too late to obviate the result. To illustrate these cases we beg to refer you to experiments marked Nos. 1, 2, and 3. To avoid the possibility of such casualties, we would suggest that the yarn or serving material be saturated with a preserving and *conducting* fluid, which would with certainty show any injury to the insulation at the time of the accident; the core being under a continual electrical test throughout the whole process of manufacturing and laying. If this plan were fully carried out, and the insulation of the conductor entirely and exclusively dependent upon the compound and gutta-percha, as applied under the present system, we have no doubt of the permanent success of submarine cables.

We will say a few words as to the effect of heat upon gutta-percha. The experiments marked 4 and 5 will show that up to 100 deg. Fah. it is not sensibly affected (except in its electrical resistance); there is, therefore, no fear of the conductor being disturbed in the gutta-percha under that temperature. Notwithstanding this we strongly recommend that in no instance should the core be subjected to any hot process, where there is the slightest chance of uncertainty as to the degree of heat employed. We will only add further that the core manufactured for Her Majesty's Government, tested by Messrs. Siemens in Mr. Reid's pressure tanks, has proved unquestionably that the electrical resistance of gutta-percha is very materially increased by pressure; and that as an insulator it is absolutely impermeable. The core manufactured for the Toulon and Algiers line for Messrs. Glass, Elliot and Co., further illustrated this valuable property. The testing of the 700 miles is fully described in the accompanying diagram, to which we beg to call your especial attention.

We must apologize for troubling you with this long letter, but the great interest we take in the all important subject of universal communication by telegraph, and our unbounded confidence in the durability of gutta-percha covered wires, if laid in the sea *uninjured*, must plead our excuse.

We have, &c.
JOHN CHATTERTON (Engineer).
WILLOUGHBY SMITH, Electrician.

Capt. Galton, R.E.,
Board of Trade.

EXPERIMENT THE FIRST.

A length (4 yards) of core, having the conductor exposed in four different places, "served" with hemp, saturated with Stockholm tar and tallow, was immersed in water, and the insulation remained perfect for three days, the test being applied frequently from a sand battery of 504 pairs of plates, 4 × 3½. It was then joined to a line of eight miles, the insulation of the whole, when immersed, using the same battery power, being,

	Zinc to line.		Copper to line.
One minute	8·5°	One minute	8°

Commenced charging with reversals from 100 Daniell's, at the rate of 200 per minute.

After 48 Hours.

	Zinc to line.		Copper to line.
One minute	11·8°	Stationary for half a minute at 11·8°, then rose to 80°.	

Same battery power.

After 72 Hours.

	Zinc to line.		Copper to line.
One minute	11·8°	One minute	11°

Same battery power.

Increased reversals to 400 per minute.

After 74 Hours.

	Zinc to line.		Copper to line.
One minute	12°	One minute	11°

Same battery power.

After 96 Hours.

	Zinc to line.		Copper to line.
One minute	13°	One minute	12·5°

Same battery power.

After 100 Hours.

	Zinc to line.		Copper to line.
One minute	14°	One minute	11°

Same battery power.

App. No. 8. Experiments upon gutta percha as an insulator for submarine electric conductors, by J. Chatterton and Wil. loughby Smith.

Appendix No. 8—*continued.*

After 124 *Hours.*

	Zinc to line.	Copper to line.
One minute	12°	12·2

Same battery power.

After 172 *Hours, the last* 48 *without being charged.*

	Zinc to line.	Copper to line.
One minute	13·3°	End of the first minute needle vibrating between 50° and 60°
		2nd minute 40 „ 45
		3rd „ 58 „ 60
		4th „ 50 „ 60
		5th „ 40 „ 70

Recommenced reversals at 400 per minute.

After 196 *Hours.*

Zinc to line.		Copper to line.
One minute 9·8°	One minute	11·8°

Same battery power.

After 220 *Hours.*

Zinc to line.		Copper to line.
One minute 17·5°	One minute	45·5°
	Two „	44·
	Three „	41·5
	Four „	36·
	Five „	41·

Same battery power.

Reduced reversals to 200 per minute.

After 244 *Hours.*

Zinc to line.	Copper to line.
1st minute needle vibrating between 54° and 68°	1st minute needle vibrating between 56° and 65°
2nd „ „ „ 61 „ 64	2nd „ „ „ 55 „ 60
3rd „ „ „ 50 „ 65	3rd „ „ „ 50 „ 60
4th „ stationary „ 59	4th „ „ „ 48 „ 55
5th „ vibrating between 50 „ 70	5th „ „ „ 53 „ 55

Same battery power used, but by reducing the power to 108 pairs, the following reading was obtained.

Zinc to line.	Copper to line.
3 minutes stationary 1·3°	3 minutes stationary at ·8°

After 272 *hours, the last* 24 *without being charged.*

Zinc to line.	Copper to line.
1st minute needle vibrating between 60° and 90°	1st minute needle vibrating between 58° and 61°
2nd „ „ „ 50 „ 80	2nd „ „ „ 58 „ 61
3rd „ „ „ 60 „ 70	3rd „ „ „ 60 „ 70
4th „ „ „ 55 „ 65	4th „ „ „ 85 „ 90
5th „ „ „ 60 „ 70	5th „ „ „ 55 „ 63

Battery power 504 pairs; recommenced reversals at 300 per minute.

After 296 *Hours.*

Zinc to line.	Copper to line.
One minute 9°	One minute 11°

Battery power 504 pairs.

After 344 *hours, the last* 48 *without being charged.*

Zinc to line.	Copper to line.
1st minute needle vibrating between 20° and 30°	1st minute needle vibrating between 35° and 40°
2nd „ „ „ 25 „ 60	2nd „ „ „ 25 „ 33
3rd „ „ „ 55 „ 60	3rd „ stationary 25
4th „ „ „ 35 „ 45	4th „ „ 22
5th „ „ „ 45 „ 55	5th „ „ 23

Battery power 504 pairs. Recommenced reversals at 300 per minute.

After 372 *Hours.*

Zinc to line.	Copper to line.
One minute 7·8°	One minute 7·°

Battery power 504 pairs.

After 396 *Hours.*

Zinc to line.	Copper to line.
One minute 8·5°	One minute 8·°

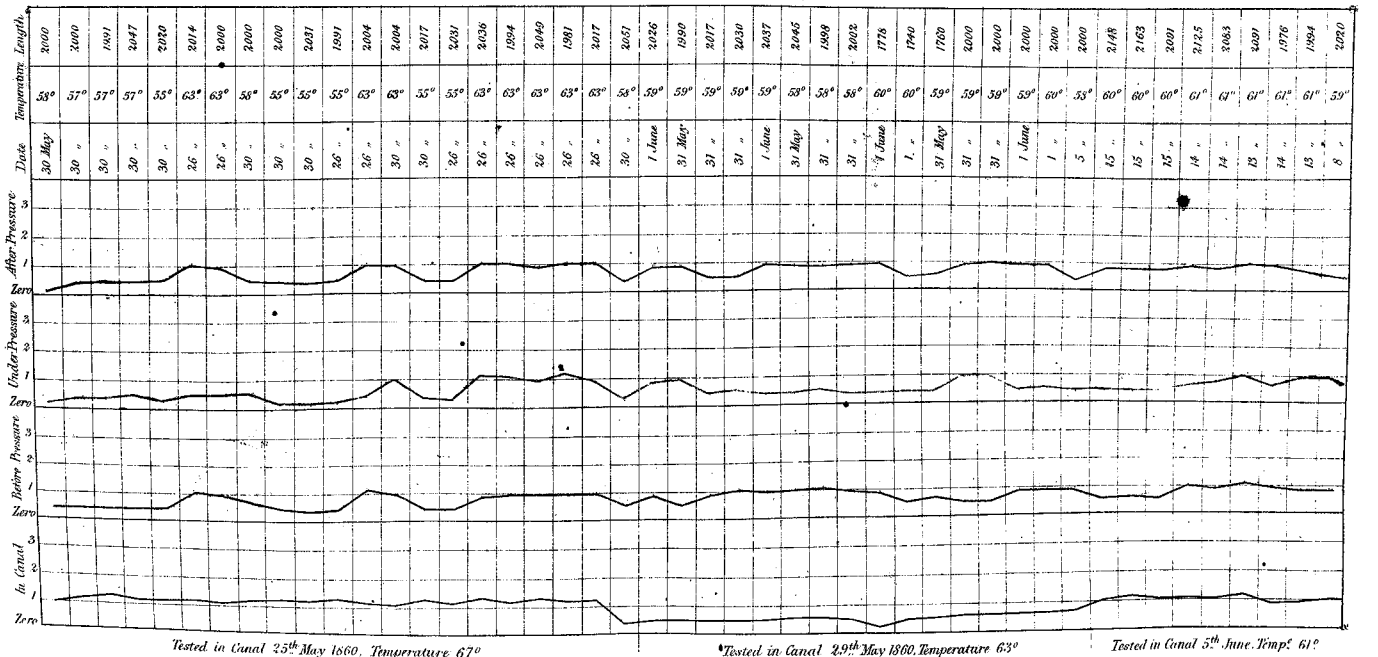
Battery powers 504 pairs.

After 420 *Hours.*

Zinc to line.	Copper to line.
One minute 7·8°	Stationary at 7·5° for 45 seconds, then rose very slowly to 45°

Battery power 504 pairs.

DIAGRAM ILLUSTRATING THE TESTINGS OF EACH COIL OF THE INSULATED CORE OF THE TOULON AND ALGIERS CABLE. Prepared by Willoughby Smith.



APPENDIX No. 8—*continued.*

After 444 Hours.

	Zinc to line.		Copper to line.
One minute	6·°	One minute	5·4°

Battery power 504 pairs.

After 468 Hours.

	Zinc to line.		Copper to line.
First minute needle vibrating between	40 and 65°	First minute stationary	14·5°
2 " " "	50 " 60	2 " " "	12·
3 " " "	60 " 62	3 " " "	12·5
4 " " "	58 " 60	4 " " "	14·
5 " " "	15 " 25	5 " " "	8·

Battery power 504 pairs.

After 640 hours, the last 172 without being charged. This defect is now offering so little resistance that the battery power had to be reduced to 108 pairs to get the following reading:

	Zinc to line.		Copper to line.
One minute	75°	One minute	75°

EXPERIMENT THE SECOND.

Five nails were forced through the gutta percha (passing through the strand conductor) in a length of core, about 4 yards, which was then "served" with hemp saturated with tar and tallow. This was then immersed, and remained perfect in insulation for 168 hours, the same battery power and instrument used as in the first experiment.

	Zinc to line.	Copper to line.
After 192 hours' immersion	·8°	·5°
" 216 "	2·	1·2
" 240 "	3·3	2·
" 288 "	10·	6·
" 360 "	28·	23·5
" 384 "	36·	30·5
" 504 "	74·	35·

Reduced Battery Power to 108 Pairs.

	Zinc to line.	Copper to line.
After 528 hours' immersion	30°	-
" 556 "	87	70°
" 580 "	86	73
" 652 "	90	72

Each reading taken after one minute's electrification.

This has now become quite bad, so much so that were it to be charged with reversals for 24 hours from a strong battery, continuity would be destroyed.

EXPERIMENT THE THIRD.

In another length of core prepared same as the above, the nails were withdrawn previously to being "served." This length has now been immersed 724 hours, and the insulation is still perfect.

EXPERIMENT THE FOURTH (Temperature).

Two lengths of insulated wire same size as the core used in the Toulon and Algiers cable, 800 yards each, immersed in water at 100° Far., and tested with the same battery and instrument as used when testing the core of the Toulon cable. The following are the readings taken after one minute's electrification:—

	No. 1 coil. Zinc to line.	No. 2 coil. Zinc to line.		No. 1 coil. Zinc to line.	No. 2 coil. Zinc to line.
After one hour's immersion.	9·	7·1	After 444 hours immersion.	3·8	4·2
" 2 "	7·	6·1	" 468 "	3·1	3·8
" 50 "	5·3	5·2·	" 492 "	3·2	3·4
" 66 "	5·8	5·4	" 516 "	3·2	3·3
" 106 "	5·5	4·9	" 540 "	3·	3·1
" 130 "	5·	4·7	" 564 "	3·	3·
" 154 "	4·5	4·4	" 588 "	2·9	2·9
" 180 "	3·5	4·2	" 636 "	3·2	3·4
" 202 "	3·8	4·	" 660 "	3·	3·
" 226 "	4·2	4·9	" 684 "	3·3	3·5
" 250 "	4·2	4·5	" 718 "	3·	3·2
" 274 "	4·1	4·4	" 742 "	2·9	3·
" 298 "	4·2	4·4	" 766 "	3·2	3·4
" 322 "	4·	4·3	" 1030 "	3·	2·8
" 346 "	4·	4·5	" 1054 "	3·	2·9
" 368 "	3·7	4·3	" 1078 "	3·	3·2
" 396 "	3·8	4·5	" 1102 "	2·8	3·
" 420 "	3·8	4·3			

EXPERIMENT THE FIFTH (Temperature).

A length of the same sized core as that used in the cable recently manufactured for Her Majesty's government was placed on an iron cylinder 12 inches in diameter, and immersed in water at 100° Fah. for two months. The result was that the conductor was not in the slightest degree disturbed, nor was there any change perceptible.

The annexed Diagram illustrates the testing of the core manufactured by the Gutta Percha Company for Messrs. Glass, Elliot, and Co. for the Toulon and Algiers line.

The conductor is a seven wire strand insulated by four alternate coatings of compound and gutta-percha to diameter ·34, and was tested in coils average length of one knot. The battery power consisted of 504 pairs of plates 4 × 3½, and the galvanometer used known as Reid's sensitive horizontal. The *black* line shows the test in the canal.

The *blue* line in the pressure tank before pressure.

The *red* line under a pressure of 600lbs. per square inch, and the *green* line taken after pressure.

The reading of the instrument was in each case taken after one minute's electrification.

App. No. 9.

APPENDIX No. 9.

ABSTRACT of EXPERIMENTS made for determining the RELATIVE STRENGTH of the OUTER COVERING of SUBMARINE TELEGRAPHIC CABLES. *See* PLATE No. 1.

Name of Cable.	Description of Cable.	Weight per Mile, in Air.	Specific Gravity (in Water).	Mode in which Experiments were made.	Length experimented on.	Length of Cable in Water equivalent to 1 per cent. of Elongation.	Length of Cable in Water equivalent to 1 per Cent. of Elongation.	Length of Cable equivalent to Breaking Strain.	Remarks.
		lbs.			Feet.	Fms.	Fms.	Fms.	
Gibraltar cable (No. 2.)	Core of 7 strands of copper, weighing 400 lbs. per mile, covered with 3 coats of gutta-percha, alternated with 3 coats of Chatterton's compound, weighing 400 lbs. per mile. The core covered with 12 strands of steel wire and hemp, each strand consisting of 1 wire, No. 17 gauge, with 6 yarns of tarred hemp, each about No. 16 gauge, with 1 turn in half-inch; cable, 1 twist in 9 inches; cable served closely with 1 strong string. External diameter, $\frac{13}{16}$". See Fig. No. 2, Plate No. 2).	2,409	1·9	Cable suspended from triangle (see Plate No. 1), the upper end being fixed, and the lower end, to which the weight was suspended, being allowed to revolve.	20	815	1,232	Not broken.	This cable was weighted to 3,596 lbs. The extension before removing the weight was = 1·927 per cent. The permanent extension, after removing the weight, was = 1·197 per cent.; the return = ·73 per cent. The number of revolutions, 18.
Ditto - -	Ditto, not served with string. (See Fig. No. 3, Plate No. 2).	2,409	1·9	Ditto - -	20	654	1,154	Not broken.	Ditto. The extension before removing the weight was = 1·875 per cent. The permanent extension was equal to 1·3541 per cent; the return = ·52 per cent. The number of revolutions, 18½.
Ditto - -	*Ditto - - - -*	*2,409*	*1·9*	*Cable fixed between clamps.*	*8 ft. 4 in.*	*2,768*	*3,868*	*5,936*	*Average of experiments made by Messrs. Gisborne, Forde, and C. W. Siemens.*
Ditto (No. 4)	Same core, served with 9 tarred yarns about No. 8 gauge, and covered with 12 strands of steel and hemp, each strand consisting of 1 steel wire, No. 14, with 6 yarns of tarred hemp, each about No. 11 gauge. External diameter, 1 $\frac{4}{16}$". (See Fig. No. 4, Plate No. 2).	4,398	1·9	Cable suspended from triangle (see Plate No. 1), the upper end being fixed, and the lower end, to which the weight was suspended, being allowed to revolve.	20	350	546	Not broken.	This cable was weighted to 2,591 lbs. The extension before removing the weight was = 1·666 per cent. The permanent extension was = 1·328 per cent. The return = ·38 per cent. The number of revolutions, 2¾.
Ditto (No. 4)	Ditto - - - -	4,398	1·9	Cable tested in horizontal machine (see Plate No. 1), both ends being fixed.	65½	1,176	1,947	Not broken.	This cable was weighted to 4·467 lbs. The extension before removing the weight was = ·946 per cent. The permanent extension was = ·866 per cent. The return = ·08 per cent.
Ditto (No. 4)	Ditto - - - -	4,398	1·9	Ditto - -	65½	2,440	3,924	—	Cable did not break with the weight of 13,043 lbs. = 5,683 fathoms in length of its weight in water.
Ditto (No. 4)	*Ditto - - - -*	*4,398*	*1·9*	*Cable fixed between clamps.*	*8 ft. 4 in.*	*3,644*	*5,806*	*9,299*	*Average of experiments made by Messrs. Gisborne, Forde, and C. W. Siemens.*
Ditto (No. 3)	Ditto, with 12 strands of *iron* wire. External diameter, 1 $\frac{4}{16}$" inch.	4,330	1·85	Cable suspended from triangle (see Plate No. 1), the upper end being fixed, and the lower end, to which the weight was suspended, being allowed to revolve.	10	419	794	Not broken.	This cable was weighted to 1,787 lbs. The extension before removing the weight was = ·988. The permanent extension was = ·416 The return = ·572. The number of revolutions, 1¾.
Ditto (No. 3)	Ditto - - - -	4,330	1·85	Ditto - -	10	198	748	4,989	The splicing at top gave way, and the core was forced out between the steel and hempen strands on their return (see Plate No. 9). The cable was then re-spliced, and broke with 11,311 lbs. at the place where the core had been forced out.

APPENDIX No. 9—*continued.*

Name of Cable.	Description of Cable.	Weight per Mile, in Air.	Specific Gravity (in Water).	Mode in which Experiments were made.	Length experimented on.	Length of Cable in Water equivalent to 0° per Cent. of Elongation.	Length of Cable in Water equivalent to 1 per Cent. of Elongation.	Length of Cable in Water equivalent to Breaking Strain.	Remarks.
		lbs.			Feet.	Fms.	Fms.	Fms.	
Godefroy's -	Core of 7 strands of copper wire No. 22, insulated with 2 coatings of gutta-percha; outside of which 22 steel wires No. 21, in close parallel lines, insulated with india-rubber, and covered with india-rubber waterproof canvass. External diameter $\frac{6}{16}$". (See Fig. No. 9, Plate No. 3).	1,211	1·9	Cable suspended from triangle (see Plate No. 1), the upper end being fixed, and the lower end, to which the weight was suspended, being allowed to revolve.	20	2,553	—	3,424	The number of revolutions $= \frac{1}{4}$.
Ditto - -	Core of 7 strands of copper No. 22, insulated with india-rubber; outside of which 22 steel wires No. 21, in close parallel lines; afterwards insulated with patent cocoa-nut gutta-percha, and protected with 2 coats of india-rubber waterproof canvass. External diameter $\frac{13}{32}$". (See Fig. No. 8, Plate No. 3).	1,903	1·4	Ditto - -	20	2,596	3,500	Not broken.	The cable was weighted to 1,787 lbs. The extension before removing the weight was = ·520 per cent. The permanent extension was = ·182 per cent. The return = ·338 per cent. The number of revolutions = 0.
Ditto - -	Ditto - - - -	1,903	1·4	Ditto - -	20	2,596	—	3,309	The number of revolutions = 0.
Silver and Co.'s.	Copper wire No. 16, insulated with india-rubber, then served with tarred yarn, outside of which steel wires, gauge 21, in close parallel lines, and covered with platted hemp. External diameter, $\frac{1}{2}$". (See Fig. No. 1, Plate No. 2.)	2,150	2·8	Ditto - -	20	1,642	2,260	Not broken.	The number of revolutions = 0.
Ditto - -	Ditto - - - -	2,150	2·8	Ditto - -	20	1,832	—	3,213	The number of revolutions = 0.
Ditto - -	Ditto - - - -	2,150	2·8	Cable tested in Horizontal Machine (see Plate No. 1), ends being fixed.	65½	961	2,145	2,675	The number of revolutions = 0.
Hall & Wells, No. 1.	Core of 7 strands of copper wires, No. 22, covered with tape and insulated with 3 coatings of india-rubber, having between the 2d and 3d coatings a layer of vulcanized thread covered with cotton, the insulator served with thin hempen twine, lapped spirally, with 4 tarred yarns and again 12 longitudinal tarred yarns or marlin lines. The outward covering made of hemp platting, with 12 longitudinal strands or marlin lines. External diameter, $1\frac{1}{16}$". (See Fig. No. 10, Plate No. 4).	1,907	1·35	Cable suspended from triangle (see Plate No. 1), the upper end being fixed, and the lower end, to which the weight was suspended, being allowed to revolve.	20	—	897	Not broken.	This cable was weighted to 2,256 lbs. The extension before removing the weight was = 3·80 per cent. The permanent extension of the cable, after removing the weight, was = 2·76 per cent. The return = 1·04 per cent. The number of revolutions = $3\frac{1}{2}$.
Ditto, No. 1 -	Ditto - - - -	1,907	1·35	Ditto - -	20	760	1,126	4,420	This cable had been immersed in water during the night. The number of revolutions = $\frac{1}{2}$.
Ditto No. 2 -	Same core closely served with thin tarred hempen twine, and 19 tarred longitudinal strands or marlin lines, and 8 longitudinal steel wires, No. 16. The outer covering made of hemp platting. External diameter, $1\frac{1}{16}$". (See Fig. No. 11, Plate No. 4).	2,577	1·6	Ditto - -	20	1,830	—	Not broken.	The cable was weighted to 2,859 lbs. The extension before removing the weight was = ·73 per cent. The permanent extension of the cable after removing the weight was = ·36 per cent. The return = ·37 per cent. The number of revolutions = $1\frac{1}{4}$.
Ditto No. 2 -	Ditto - - - -	2,577	1·6	Ditto - -	20	2,499	—	2,729	The number of revolutions = $\frac{1}{2}$.

App. No. 2.

APPENDIX No. 9—*continued.*

Name of Cable.	Description of Cable.	Weight per Mile, in Air.	Specific Gravity (in Water).	Mode in which Experiments were taken.	Length experimented on.	Length of Cable in Water equivalent to 0·5 per Cent. of Elongation.	Length of Cable in Water equivalent to 1 per Cent. of Elongation.	Length of Cable in Water sufficient to Breaking Strain.	Remarks.
		lbs.			Feet.	Fms.	Fms.	Fms.	
Hall & Wells, No. 3.	Core covered with india-rubber, closely served with 2 tarred thin yarns and 12 longitudinal hemp lines, and 8 longitudinal steel wires (4 No. 15 and 4 No. 14) in a sheath of hemp platting. External diameter, $\frac{27}{32}$".	1,568	1·9	Cable suspended from triangle (see Plate No. 1), the upper end being fixed, and the lower end, to which the weight was suspended, being allowed to revolve.	20	2,120	—	Not broken.	The cable was weighted to 1,787 lbs. The extension before removing the weight was = ·416 per cent. The permanent extension, after removing the weight, = ·208 per cent. The return = ·208 per cent. The number of revolutions = 0.
Ditto No. 3	Ditto - - - -	1,568	1·9	Ditto - -	20	2,437	4,525	5,597	The number of revolutions = 0.
Siemen's -	Gutta-percha core, covered spirally with 56 strands of hemp, and lapped with copper. External diameter, $1\frac{1}{16}$". (See Fig. No. 12, Plate 4).	1,705	1·5	Ditto - -	9 ft. 6 in.	—	1,760	Not broken.	The cable was weighted to 2,256 lbs. The extension before removing the weight was = 1·75 per cent. The permanent extension after removing the weight, = 1·15 per cent. The return, = ·60 per cent. The number of revolutions = $1\frac{1}{2}$.
Ditto - -	Ditto - - - -	1,705	1·5	Ditto - -	9 ft. 6 in.	1,342	2,552	Ditto.	The splicing of the lower loop gave way. The number of revolutions = $\frac{1}{4}$.
Ditto - -	Ditto - - - -	1,705	1·5	Ditto - -	9 ft. 6 in.	2,046	3,672	Ditto.	The splicing again gave way. The number of revolutions = $\frac{1}{4}$.
Ditto - -	Ditto - - - -	1,705	1·5	Ditto - -	9 ft. 6 in.	1,694	3,284	Ditto.	The splicing gave way. Further experiments were stayed on finding that 2 strands in the upper loop had broken. The cable was weighted to 2,591 lbs. The number of revolutions = 0.
Allen's No. 1	Core consisting of No. 10 copper wire, covered spirally with steel wires, gauge 25, and coated with 3 thicknesses of gutta-percha, and finished in covering of platted jute, saturated with marine paint. (See Fig. No. 5, Plate No. 3). External diameter, $\frac{13}{16}$".	2,122	1·6	Ditto - -	20 ft.	1,519	—	Not broken.	The cable was weighted to 1,787 lbs. The extension before removing the weight was = ·703 per cent. The permanent extension of the cable, after removing the weight, was = ·282 per cent. The return = ·42 per cent. The number of revolutions = 2.
Ditto - -	Ditto - - - -	2,123	1·6	Ditto - -	20 ft.	1,283	2,258	2,936	The number of revolutions = $\frac{1}{2}$.
Ditto (No. 2)	Ditto, but finished with two coats of india-rubber waterproof canvass instead of the platted jute. External diameter $\frac{13}{16}$". (See Fig. No. 6, Plate No. 3).	1,961	1·38	Ditto - -	20	2,408	—	Not broken.	The cable was weighted to 1,787 lbs. The extension before removing the weight was = ·67 per cent. The permanent extension of the cable was = ·234 per cent. The return = ·443 per cent. The number of revolutions = $\frac{3}{4}$.
Ditto - -	Ditto - - - -	1,961	1·38	Ditto - -	20	2,022	3,373	4,698	The number of revolutions = $\frac{1}{4}$.
Ditto - -	Ditto - - - -	—	1·38	Cable tested in a horizontal machine, both ends being fixed. (See Plate No. 1.)	65 ft. 6 in.	3,403	5,555	7,484	The number of revolutions = 0.
Allen's No. 1	Core of No. 12 copper wire, covered with steel wires No. 25, and coated with 4 thicknesses of gutta-percha, and finished in a coat of Godefroy's compound. External diameter, $\frac{17}{32}$". (See Fig. No. 7, Plate No. 3)	1,351	1·3	Cable suspended from triangle. (See Plate No. 1), the upper end being fixed, and the lower end, to which the weight was suspended, being allowed to revolve.	20	3,335	6,340	6,348	The number of revolutions = 0.

APPENDIX No. 9.—*continued.* APP. No. 9.

Name of Cable.	Description of Cable.	Weight per mile, in Air.	Specific Gravity (in Water).	Mode in which Experiments were taken.	Length experimented on.	Length of Cable in Water equivalent to 0·5 per Cent. of Elongation.	Length of Cable in Water equivalent to Breaking Point of Elongation.	Length of Cable in Water equivalent to Breaking Strain.	Remarks.
		lbs.			Fect.	Fms.	Fms.	Fms.	
Sinnock's No. 1.	Core of 7 strands of copper wire, covered with gutta-percha, served with waterproof tape; then 11 longitudinal thick marlin lines, each consisting of 9 tarred yarns, served spirally with hempen string; the whole served with 2 turned iron wires and 3 tarred strings. External diameter, 1". (See Fig. No. 14, Plate No. 4).	2,695	1·4	Cable suspended from triangle (See Plate No. 1), the upper end being fixed, and the lower end, to which the weight was suspended, being allowed to revolve.	20	622	1,088	Not broken.	The cable was weighted to 1,787 lbs. The extension before removing the weight was = 1·664 per cent. The permanent extension was = ·936 per cent. The return = ·728 per cent. The number of revolutions = 1½.
Ditto -	Ditto - - - - -	2,695	1·4	Ditto - -	20	—	463	2,855	The number of revolutions = 0.
Ditto No. 2	Core of 7 strands of copper wire, covered with gutta-percha, and served with waterproof tape; next 15 longitudinal tarred hempen lines; the whole served spirally with 3 twined iron wires ($\frac{3}{16}$") and 3 tarred hempen strings. External diameter $1\frac{1}{16}$". (See Fig. No. 13, Plate No. 4).	1,430	1·7	Ditto - -	20	997	1,782	Not broken.	The cable was weighted to 1,787 lbs. The extension before removing the weight was = 1·3 per cent. The permanent extension was = 1·04 per cent. The return = ·26 per cent. The number of revolutions = 1½.
Ditto - -	Ditto - - - - -	1,430	1·7	Ditto - -	20	—	636	7,132	The number of revolutions = 0.
De Bergues' -	Core of 7 strands of copper wire covered with gutta-percha; next 17 longitudinal tarred hempen lines served with similar sized cord of hemp. External diameter, $\frac{8}{16}$". (See Fig. No. 15, Plate No. 5).	509	1·26	Ditto - -	20	5,300	8,124	Not broken	The cable was weighted to 1,251 lbs. The extension before removing the weight was = 1·404 per cent. The permanent extension was = ·832 per cent. The return = ·572 per cent. The number of revolutions = 0.
Ditto - -	Ditto - - - - -	509	1·26	Ditto - -	20	—	5,908	16,982	The number of revolutions = 0.
Red Sea Cable	- - - - - -	*21 cwt.*	—	*Ditto* - -	*8 ft. 4 in.*	*4,245*	—	*5,114*	*Experiments made by Messrs. Gisborne, Forde, and C. W. Siemens.*
Ditto - -	- - - - - -	*21 cwt.*	—	*Ditto* - -	*8 ft. 4 in.*	*3,871*	*4,800*	*4,677*	
Clark's (Latimer).	Gibraltar core served with waterproof tape, and 52 steel wires, gauge (No. 22), having a slight spiral direction, and covered with tape and a composition of shellac and marine glue. External diameter, $\frac{12}{16}$". (See Fig. 20, Plate No. 6).	2,029	1·92	Ditto - -	10	1,610	—	Not broken.	The cable was weighted to 1,787 lbs. The extension before removing the weight was = ·468 per cent. The permanent extension was ·364. The return = ·104 per cent. The number of revolutions = 0.
Ditto - -	Ditto - - - - -	2,024	1·92	Ditto - -	10	1,119	—	3,376	The number of revolutions = 0.
Hall & Wells, Specimen No. 1.	*Core of 1 strand of No. 16 copper wires weighing 64 lbs. per mile, insulated with india-rubber weighing 160 lbs. per mile. The core covered with 20 longitudinal strands of hemp, steeped in pitch and cork dust, and 8 steel wires, No. 14 gauge, braided together with 24 strands of hemp saturated with best Stockholm tar. External diameter, $\frac{7}{8}$".*	*1,838*	*1·4*	*Cable fixed between clamps.*	*20*	*The elongation of the cable in the experiment did not amount to 0·5 per cent.*		*10,810*	*Experiments made by Messrs Hall and Wells.*
Ditto, Specimen No. 2.	*Core of 7 strands of No. 21 copper wire weighing 103 lbs. per mile, insulated with india-rubber weighing 200 lbs. per mile. The core covered with 20 longitudinal strands of hemp steeped in pitch and cork dust, and 8 steel wires No. 15 gauge, braided together with 24 strands of hemp saturated with best Stockholm tar. External diameter, $\frac{3}{4}$".*	*1,649*	*1·25*	*Ditto* - -	*20*	*Ditto* - -		*11,635*	

App. No. 9. Experiments on the elasticity of steel, iron, and copper wires, and tarred hempen strands.

Appendix No. 9—*continued.*

Experiment on the Elasticity of Copper Wire.

Diameter of Wire, 1/32 inch. Length, 100 inches. (On Triangle.)

Weight.	Elongation.	Return.	Permanent Elongation.
lbs.	inch.	inch.	
1	·0156	·0156	·0
2	·03125	·03125	·0
3	·0625	·0625	·0
4	·0625	·0625	·0
5	·0625	·0625	·0
6	·0625	·0625	·0
7	·0625	·0625	·0
8	·0625	·0625	·0
9	·0625	·0625	·0
10	·0625	·0625	·0
11	·125	·03125	·09375
12	·156	·0625	·0935
13	·4375	·0625	·375
14	1·	·0625	·9375
15	1·75	·0625	1·6875
16	2·0625	·125	1·9375
17	4·	·0625	3·9375
18	5·	·125	4·875
19	6·5	·09375	6·406
20	8·	·09375	7·906
21	9·75	·0625	9·687
22	15·0625	·156	14·907
23	15·187	·156	15·031
24	broke down.		

Experiments on the Elasticity of Steel, Iron, and Copper Wires, and Tarred Hemp Strands.

STEEL.*

Diameter of Wire, 2 25″/32. Length, 100 inches.

Weight.	Elongation.	Return.	Permanent Elongation.
lbs.	inch.	inch.	
7	·0	·0	·0
14	·0	·0	·0
28	·0	·0	·0
56	·03125	·03125	·0
112	·0625	·0625	·0
168	·25	·09375	·03125
224	·1875	·1406	·04657
336	·25	·1875	·0625
448	·3125	·21875	·0977
616	·375	·2719	·1031

* The steel taken from Gibraltar cable No. 4.

IRON.†

Diameter of Wire, 2·25/32″. Length, 100 inches.

Weight.	Elongation.	Return.	Permanent Elongation.
lbs.	inch. ″	inch. ″	″
7	·0	·4	·0
14	·0	·0	·0
28	·0625	·0625	·0
56	·0625	·0625	·0
112	·125	·125	·0
168	·1563	·125	·0315
224	·1875	·125	·0625
336	·2815	·1724	·1091

† The iron wire was taken from Gibraltar cable No. 3.

COPPER.

Diameter of Wire, 4 25″/32. Length, 100 inches.

Weight.	Elongation.	Return.	Permanent Elongation.
lbs.	inch.	inch.	
7	·0	·0	·0
14	·03125	·03125	·0
28	·0625	·0625	·0
56	·09375	·09375	·0
112	·125	·125	·0
168	·1875	·125	·0625
224	·875	·0625	·8125
336	7·75	·0625	7·6875

HEMP.‡

Diameter of Strand, 7″/32. Length, 100 inches. Number of Yarns, 6. Diameter of yarn, 2/32.

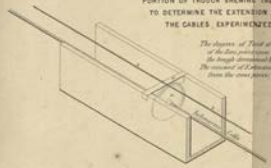
Weight.	Elongation.	Return.	Permanent Elongation.
lbs.	inch.	inch.	
7	·03125	·03125	·0
14	·25	·25	·0
28	·75	·625	·125
56	1·5	·563	·937
112	2·0625	·563	1·502
168	3·25	—	—
224	3·5	1·	2·5
336	4·25	1·218	3·032

‡ The hemp taken from Gibraltar cable No. 3, and had been tarred and steeped in water before experiment.

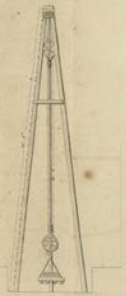
Comparison of the Elongation, Return, and Permanent Elongation of the Materials used to form the Conductor and the outer Covering of Cables.

		Elongation.	Return.	Permanent Elongation.
Weight 336 lbs.	Iron	·2815	·1724	·1091
	Copper	7·75	·0625	7·6875
	Hemp	4·25	1·218	3·032

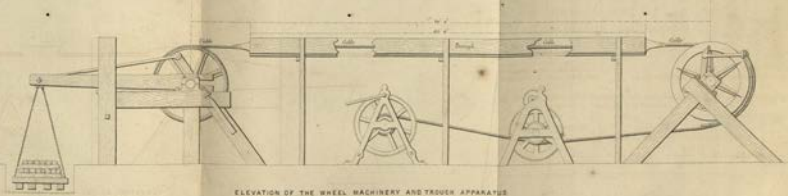
PORTION OF TROUGH SHEWING THE MEANS ADOPTED TO DETERMINE THE EXTENSION AND TWIST OF THE CABLES EXPERIMENTED UPON.



The diagram of Twist of the Cable indicated by the divergence of the Iron pointers from the Dial to from the center of the top of the trough determined by the Star Side.
The measure of Extension indicated by the distance of the Iron from the cross pieces of Hand of Mother spoon.



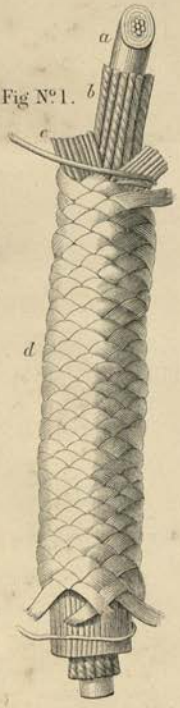
TRIPOD APPARATUS USED IN TESTING THE STRENGTH OF SUBMARINE CABLES.



ELEVATION OF THE WHEEL MACHINERY AND TROUGH APPARATUS USED IN TESTING THE STRENGTH OF SUBMARINE CABLES.

SILVER & Cos.

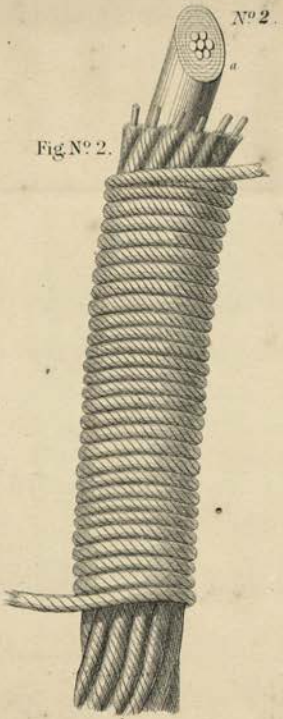
Fig Nº 1.



a Copper wires Nº16 covered with india rubber.
b Tarred Yarns.
c Steel wires.
d Plaited hemp covering.

Nº 2.

Fig. Nº 2.

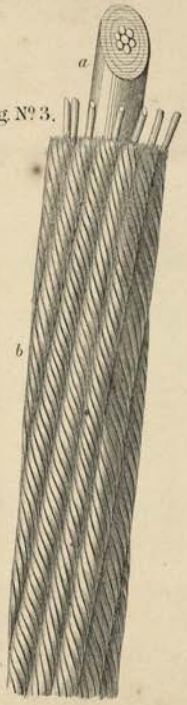


a Gibraltar Core of 7 copper wire covered with 3 coatings of Gutta percha and 3 alternate coats of Chatterton's compound.
b 12 Strands of steel wire and hemp, each Strand consisting of 1 Nº17 wire and 6 Yarns of tarred hemp.
c Serving of 1 strong string.

GIBRALTAR.

Nº 2.

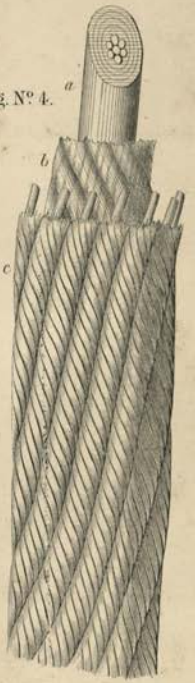
Fig. Nº 3.



a — Gibraltar Core. (See preceding figure)
b — Covering consisting of 12 strands of steel wire and hemp, each strand consisting of 1 Nº17 wire and 6 Yarns of tarred hemp.

Nº 4.

Fig. Nº 4.



a — Gibraltar Core. (See preceding figure)
b — 9 tarred Yarns about 8 gauge.
c — Outside covering of 12 strands of steel wire and hemp, each strand consisting of 1 wire Nº14 with 6 Yarns of tarred hemp of Nº11 gauge.

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ALLANS.

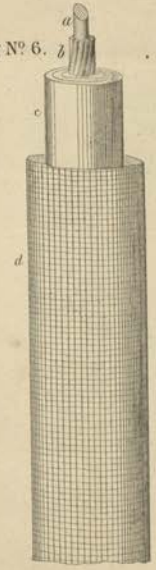
Nº 1

Fig Nº 5.

a — Copper wire Nº 10.
b — Steel wires (laid spirally)
c — Three thicknesses of gutta percha.
d — Covering of platted jute laid spirally.

Nº 2

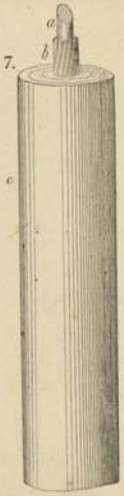
Fig Nº 6.



a — Copper wire Nº 10.
b — Steel wires.
c — Three thickness of gutta percha.
d — Two coats of india rubber canvass.

Nº 3

Fig. Nº 7.

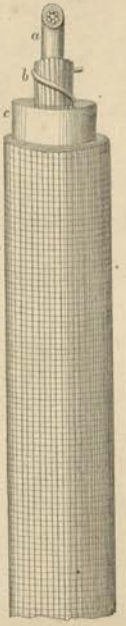


a — Copper wire Nº 10
b — Steel wires
c — Three thicknesses of gutta percha.

CODEFROY'S.

Nº 1

Fig. Nº 8.



a — 7 copper wires Nº 22 covered with india rubber.
b — 22 steel wires.
c — Patent cocoa nut fibre compound.
d — India rubber canvass.

Nº 2

Fig. Nº 9.

a — 7 copper wires Nº 22 covered with two coats gutta percha.
b — 22 steel wires with canvass covering.
c — India rubber coating.
d — India rubber canvass.

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HALL & WELLS.

Nº 1

Fig. Nº 10

Core of 7 Copper wires Nº 22, covered with tape and insulated with 3 coatings of india rubber(a) having between 2nd & 3rd coatings a layer of vulcanized thread covered with cotton.
b — Serving of thin hempen twine
c — 4 tarred yarns lapped spirally
d 12 longitudinal hempen yarns
e — Sheath of hemp plaiting with 12 longitudinal strands.

Nº 2

Fig Nº 11.

Core of 7 Copper wires Nº 22 covered with tape and insulated with 3 coatings of india rubber(a) having between 2nd & 3rd coatings a layer of vulcanized thread covered with cotton.
b — Serving of thin hempen twine.
c — 19 tarred longitudinal strands.
d — 8 longitudinal steel wires Nº 16.
e — Sheath of hemp plaiting.

SIEMENS'

Fig Nº 12.

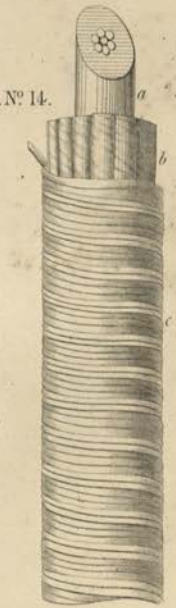
Core of 7 copper wires
a — Gutta percha insulator
b — Spiral strands of hemp
c — Metal sheath wound spirally.

SINNOCK'S

Fig. Nº 13.

Core of 7 Copper wires
a — Gutta percha insulator served with water proof tape
b — 15 Tarred hempen lines
c — Spiral serving of alternate iron wires and hempen string.

Fig. Nº 14.



Core of 7 copper wires
a — Gutta percha insulator served with water proof tape
b — 11 Marlin line spirally served with hempen string
c — Spiral serving of alternate iron wires and hempen string

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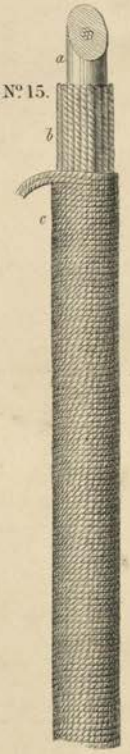
DE BERGUES'

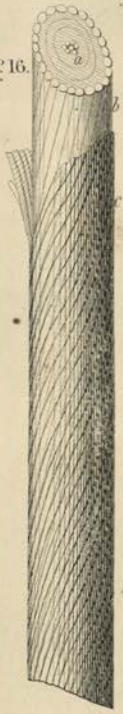
Fig Nº 15.



a. Core of 7 strands of copper wire covered with Gutta percha.
b. 17 Tarred longitudinal hempen lines.
c. Serving of hemp.

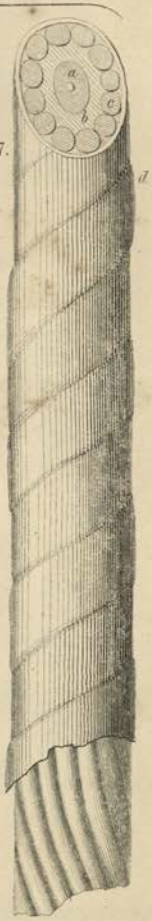
L. CLARK'S

Fig. Nº 16.



a. Core of 7 strands of copper wire covered with with Gutta percha.
b. Steel wires.
c. covering of tarred hempen lines.

Fig Nº 17.



a. Core covered with Gutta percha.
b. Serving of Tape and hemp
c. Steel wires
d. Covering of canvas and marine glue

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HEARDER'S

Fig. Nº 18.

a Copper wires.
b. Hempen strands.
c. Gutta percha.

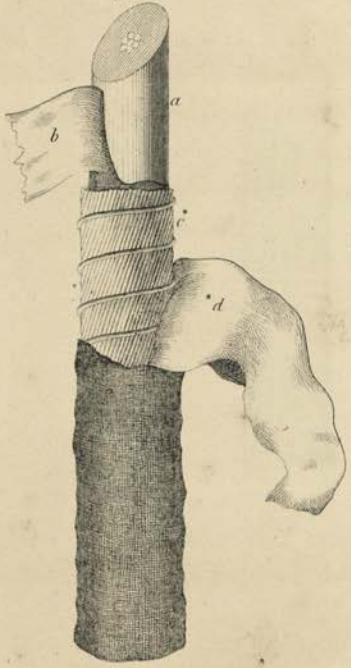
GORDON'S

Fig. Nº 19.

a Copper wire.
b. Steel wires.
c. Gutta percha.

L. CLARK'S

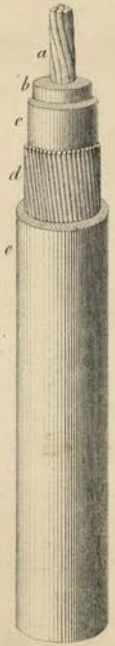
Fig. Nº 20.



a Gibraltar Core
b Sewing of tape
c 52 Steel wires (Nº 22 gauge)
d Covering of tape & a composition of Shellac & Marine Glue

HOOPER'S

Fig. Nº 21.



a Copper wires
b India rubber
c Vulcanized rubber
d Steel wires
e Outer covering of Vulcanized rubber

ROGER'S

Fig. Nº 22.

a Gutta percha Core with 4 wires
b India rubber cloth
c Hempen strands
d Platted hemp covering

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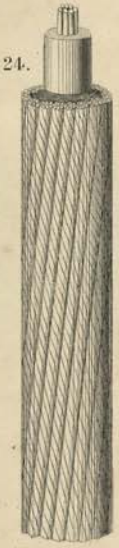
RED SEA.

Fig Nº 23.

ATLANTIC.

Fig. Nº 24.



DARDANELLES – ALEXANDRIA.
CANDIA–ALEXANDRIA SECTION.

Fig Nº 25.

a — *7 Copper wires covered with Gutta Percha.*
b — *Covering 8 hempen strands.*

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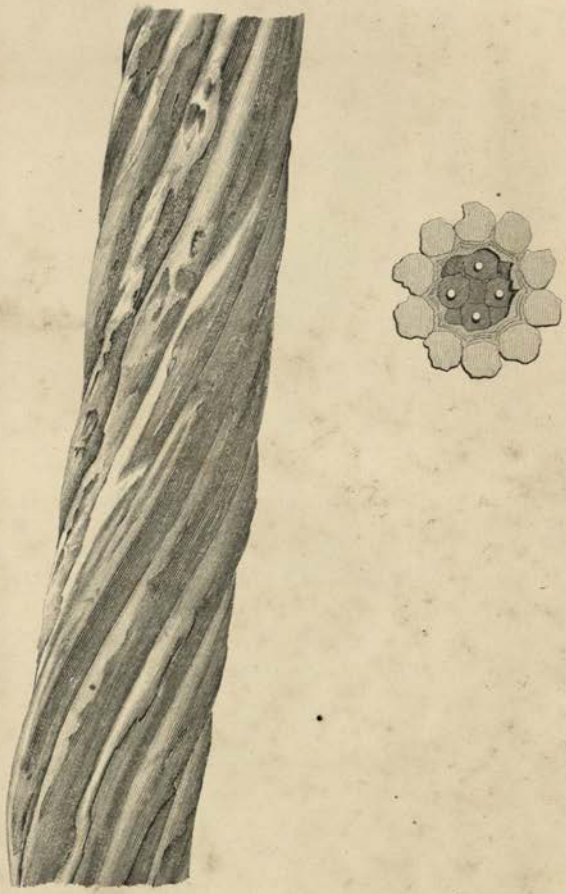


DOVER & CALAIS CABLE.

Laid 17th October 1851.
Taken up November 1859.
Electrical Condition perfect.

Day & Son, Lith.rs to the Queen

GIBRALTAR CABLE

Nº 3.

In the experiments for testing the strength of this cable the splicing at the top gave way and the consequent contraction in the length of the cable caused the core to be forced out between the strands of outer covering. The cable after readjustment subsequently broke at the place where the core had been forced out.

Appendix No. 10.

Abstract of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, for determining the relative Strength, &c. of Outer Covering of Submarine Telegraphic Cables.

No. of Specimen.	Specification of Sample for 1 Knot. Weight of Core. Copper.	Gutta Percha and Compound.	Outside Coat of Compound.	Weight of Outer Covering. Hemp. Serving.	Strands.	Wire. Iron.	Steel.	Number of Wires.	Strands.	Gauges of Wires.	Total Weight. In Air.	In Water.	Strength of Cable. ½ the Breaking Strain.	⅔ the Breaking Strain.	Breaking Strain.	Per-centage of Elongation. ½ the Breaking Strain.	⅔ the Breaking Strain.	Breaking Strain.	Equivalent in Depth. ½ the Breaking Strain.	⅔ the Breaking Strain.	Breaking Strain.	Size of Cable. Outside Diameter of Core.	Outside Diameter of Cable.	Number of Sheet.	Remarks.
	Cwts.	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*No.*	*No.*	*No.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*				*Fms.*	*Fms.*	*Fms.*	*Inches.*	*Inches.*	*No.*	
1	3·57	3·57	–	–	3·91	10·59	–	12	12	14	21·64	12·63	35·00	46·67	70·00	·25	·45	1·75	2,801	3,734	5,601	·50	·75	1	
				Same as above. 2d Sample tested.							,,	,,	42·50	56·67	85·00	·27	·41	1·07	3,402	4,532	6,802	,,	,,	2 to 5	
2	3·57	3·57	–	–	5·68	5·31	–	12	12	17	18·13	8·01	35·25	41·67	62·50	·39	1·21	2·90	3,952	5,270	7,908	·50	·75	6	
				Same as above. 2d Sample tested.							,,	,,	32·50	43·34	65·00	·41	·86	2·77	4,110	5,480	8,219	,,	,,	7	
3	3·57	3·57	–	–	4·91	9·20	–	10	10	14	21·25	11·41	30·00	40·00	60·00	·35	·60	2·50	3,663	3,551	5,327	·50	·75	8	Elongation doubtful.
				Same as above. 2d Sample tested.							,,	,,	30·00	40·00	58·75	·28	·51	1·71	2,663	3,551	5,216	,,	,,	9	
3a	3·57	3·57	–	–	5·07	–	8·44	10	10	14	20·65	10·75	40·00	53·34	80·00	·40	·62	2·90	3,767	5,026	7,539	·50	·75	10	Elongation doubtful.
				Same as above. 2d Sample tested.							,,	,,	47·50	63·33	95·00	·75	1·25	3·30	4,476	5,969	8,953	,,	,,	11	
4	3·57	3·57	–	–	4·56 G.F.	–	–	–	16	–	11·70	3·36	8·75	11·75	17·62	·12	·40	1·00	2,638	3,543	5,653	·50	·63	12	
5	3·57	3·57	2·40	–	3·92	10·34	–	12	12	14·50	23·80	11·80	27·50	37·67	55·00	·23	·42	·96	2,863	3,151	4,726	·50	·875	13	
				Same as above. 2d Sample tested.							,,	,,	25·00	33·33	50·00	·16	·27	·65	2,148	2,865	4,297	,,	,,	14	
6a	3·57	3·57	–	–	8·78	–	10·88	39	13	19	26·80	13·80	56·25	75·00	112·50	·71	1·01	1·61	4,225	5,511	8,450	·50	·87	15	
7	3·57	3·57	–	–	8·78	10·88	–	39	13	19	–	–	37·50	50·00	75·00	2·20	2·65	?	2,755	3,674	5,511	·50	·87	16 & 17	Elongation at breaking doubtful.
8	3·57	3·57	–	1·78	6·00	11·37	–	12	12	14	26·29	13·75	Sample not broken.						Not known.			·50	·875	18	Sample not broken.
				Same as above. 2d Sample tested.									46·25	61·67	92·50	·71	1·18	2·80?	3,410	4,547	6,821	,,	,,	19	Elongation at breaking doubtful.
					Do.	3d	do.						37·50	50·00	75·00	·55	·85	2·62	2,765	3,687	5,531	,,	,,	20	12 welded joints in 100 inches and 1 gutta percha joint.
					Do.	4th	do.						37·00	50·00	75·00	·57	·82	2·63	2,765	3,687	5,531	,,	,,	21	
					Do.	5th	do.						40·00	53·33	80·00	·69	1·03	3·20	2,950	3,933	5,900	,,	,,	22	6 welded joints in 100 inches and 1 gutta percha joint.
					Do.	6th	do.						37·25	48·33	72·50	·46	·72	2·75	2,673	3,565	5,347	,,	,,	23	
					Do.	7th	do.						43·75	58·33	87·50	·62	1·08	3·32	3,226	4,302	6,453	,,	,,	24	
					Do.	8th	do.						40·00	53·33	80·00	·61	·94	2·54	2,950	3,933	5,900	,,	,,	25	
					Do.	9th	do.						37·50	50·00	75·00	·50	·77	2·17	2,765	3,687	5,531	,,	,,	*26	Without welds or joints.
					Do.	10th	do.						42·50	56·67	85·00	·55	·87	3·15	3,133	4,179	6,269	,,	,,	27	
					Do.	11th	do.						41·25	55·00	82·50	·52	·83	2·87	3,041	4,055	6,083	,,	,,	28	
9a	3·57	3·57	–	1·78	6·00	–	10·55	12	12¹	15	25·47	13·11	50·00	66·66	102·50	·86	1·17	?	3,867	5,285	7,928	,,	,,	29 & 30	Elongation at breaking doubtful.
				Same as above. 2d Sample tested.									58·75	78·33	117·50	1·24	1·82	3·90	4,543	6,068	9,087	,,	,,	31	
					Do.	3d	do.						50·00	67·00	100·00	·90	1·18	1·93	3,867	5,157	7,735	,,	,,	32	12 welded joints in 100 inches and 1 gutta percha joint.
					Do.	4th	do.						51·25	68·33	102·50	·75	1·01	1·87	3,964	5,286	7,928	,,	,,	33	6 welded joints in 100 inches and 1 gutta percha joint.
					Do.	5th	do.						43·75	58·33	87·50	·56	·77	1·92	3,384	4,512	6,768	,,	,,	34	

APPENDIX No. 10—*continued.*

Abstract of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, for determining the relative Strength, &c. of Outer Covering of Submarine Telegraphic Cables—*continued.*

No. of Specimen.	Specification of Sample for 1 Knot.											Strength of Cable.			Per-centage of Elongation.			Equivalent in Depth.			Size of Cable.		Number of Sheet.	Remarks.
	Weight of							Number of		Gauge of Wires.	Total Weight.													
	Core.			Outer Covering.																				
				Hemp.		Wire.																		
	Copper.	Gutta Percha and Compound.	Outside Coat of Compound.	Serving.	Strands.	Iron.	Steel.	Wires.	Strands.		In Air.	In Water.	½ the Breaking Strain.	⅔ the Breaking Strain.	Breaking Strain.	½ the Breaking Strain.	⅔ the Breaking Strain.	Breaking Strain.	½ the Breaking Strain.	⅔ the Breaking Strain.	Breaking Strain.	Outside Diameter of Core.	Outside Diameter of Cable.		
9*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*No.*	*No.*	*No.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*				*Fms.*	*Fms.*	*Fms.*	*Inches.*	*Inches.*	*No.*	
					Same as above.		6th Sample tested.						70·00	93·33	140·00	·62	·84	3·22	5,414	7,219	10,828	50	·875	35 & 36	Without welds or joints.
					Do.		7th	do.					65·00	86·67	130·00	·87	1·25	3·46	5,027	6,703	10,054	,,	,,	37	Do. do.
					Do.		8th	do.					66·25	88·33	132·50	·79	1·18	?	5,124	6,832	10,248	,,	,,	38 & 39	Elongation at breaking doubtful.
					Do.		9th	do.					66·25	88·33	132·50	·86	1·30	3·40	5,124	6,832	10,248	,,	,,	40	Without welds or joints.
					Do.		10th	do.					70·00	93·33	140·00	·92	1·43	3·66	5,414	7,219	10,828	,,	,,	41	Do. do.
					Do.		11th	do.					70·00	93·34	140·00	1·04	1·52	3·91	5,414	7,219	10,828	,,	,,	42	
10		Glass, Elliott, and Co.			Quantity not known.			15	15	14	Not known.		Not known.		50·00	–	–	·60		Not known.		Not measured.		43	Sample too short for testing; elongation very doubtful.
11	3·57	3·57	–	6·60	8·10	22·90	–	14	14	12	44·74	23·40	63·75	85·00	127·50	·65	·90	2·36	2,760	3,679	5,519	·50	1·125	44	
					Same as above.		2d Sample tested.						63·75	85·00	127·50	·77	1·02	2·92	2,760	3,679	5,519	,,	,,	45 & 46	Sir C. Bright's specification; elongation at breaking doubtful.
12	1·61	1·90	–	1·50	–	16·00	–	18	18	14	21·00	17·30	32·50	42·50	65·00	·27	·42	·67	1,903	2,488	3,806	·30	·562	47	
					Same as above.		2d Sample tested.						41·25	55·00	82.50	·27	·35	·81	2,410	3,220	4,821	,,	,,	48	Elongation doubtful
					Do.		3d	do.					38·75	51·67	87·50	·17	·29	·56	2,629	3,026	4,538	,,	,,	49	
					Do.		4th	do.					41·25	55·00	82·50	·26	·33	·90	2,410	3,220	4,821	,,	,,	50	Elongation doubtful — Red Sea Cable.
					Do.		5th	do.					42·50	56·66	85·00	·34	·49	1·16	2,484	3,312	4,968	,,	,,	51	
					Do.		6th	do.					40·00	53·33	80·00	·28	·38	·55	2,342	3,116	4,674	,,	,,	52 & 53	
					Do.		7th	do.					43·75	58·33	87·50	·19	·31	1·00	2,536	3,408	5,112	,,	,,	54 & 55	
13	·83	2·23	–	3·00	–	15·64	–	126	18	20	21·70	16·30	40·00	53·34	80·00	·24	·38	·80	2,489	3,351	4,979	·30	·62	56	Atlantic cable.
14	3·57	3·57	–	2·00	–	33·56	–	18	18	11	42·70	32·73	71·25	95·00	142·50	·51	·63	1·01	2,212	2,940	5,565	·50	·77	57 & 58	Elongation doubtful. Gibraltar shore end No. 3.
					Same as above.		2d Sample tested.						78·75	105·00	157·50	40	·76	1·10	2,437	3,249	4,874	,,	,,	59 to 62	
15	1·61	1·90	–	–	7·69	13·60	–	84	28	19	24·80	13·80	26·25	35·00	52·50	1·23	1·90	3·30	1,929	2,572	3,858	·30	·75	63	Edwin Wright's double lay, iron and hemp.
					Same as above.		2d Sample tested.						23·75	31·67	47·50	·99	1·72	2·93	1,745	2,327	3,490	,,	,,	,,	
16	3·57	3·57	–	7·15	6·53	–	–	–	–	–	21·42	4·50	17·50	23·33	35·00	1·76	2·84	4·06	3,941	5,255	7,882	·50	1·00	64	Tweed and Rogers' patent. 1st Sample tested.
17	3·57	3·57	–	1·78	6·00	9·95	–	12	12	14·5	24·87	12·65	37·50	50·00	75·00	·26	·42	1·85	3,000	4,000	6,000	·50	·844	65	
					Same as above.		2d Sample tested.						35·00	46·67	70·00	·77	1·05	3·06	2,800	3,733	5,600	,,	,,	66	
					Do.		3d	do.					37·50	50·00	75·00	·52	·85	3·00	3,000	4,000	6,000	,,	,,	67	

Appendix No. 10—*continued.*

Abstract of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, for determining the relative Strength, &c. of Outer Covering of Submarine Telegraphic Cables—*continued.*

No. of Specimen.	Specification of Sample for 1 Knot.												Strength of Cable.			Per-centage of Elongation.			Equivalent in Depth.			Size of Cable.		Number of Sheet.	Remarks.
	Weight of							Number of			Total Weight.														
	Core.			Outer Covering.																					
				Hemp.		Wire.																			
	Copper.	Gutta Percha and Compound.	Outside Coat of Compound.	Serving.	Strands.	Iron.	Steel.	Wires.	Strands.	Gauges of Wires.	In Air.	In Water.	½ the Breaking Strain.	⅓ the Breaking Strain.	Breaking Strain.	½ the Breaking Strain.	⅓ the Breaking Strain.	Breaking Strain.	½ the Breaking Strain.	⅓ the Breaking Strain.	Breaking Strain.	Outside Diameter of Core.	Outside Diameter of Cable.		
	Cwts.	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*No*	*No.*	*No.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*	*Cwts.*				*Fms.*	*Fms.*	*Fms.*	*Inches.*	*Inches.*	*No.*	
				Same as above.			4th Sample tested.						37·50	50·00	75·00	·47	·75	3·10	3,000	4,000	6,000	·50	8·44	68 & 69	
				Do.			5th do.						33·75	45·00	67·50	·31	·54	1·80	2,700	3,600	5,400	„	„	70	
				Do.			6th do.						37·50	50·00	75·00	·29	·46	2·13	3,000	4,000	6,000	„	„	71 & 72	
				Do.			7th do.						35·00	46·67	70·00	·30	·50	1·91	2,800	3,733	5,600	„	„	73	
				Do.			8th do.						35·00	46·67	70·00	·41	·64	2·23	2,800	3,733	5,600	„	„	74	
18	3·57	3·57	2·68	–	6·00	9·93	–	12	12	14·5	25·77	12·80	33·75	45·00	67·50	·26	·40	2·00	2,673	3,564	5,346	·50	·815	75	
19	3·57	3·57		Simply Core without outside Compound.							7·15	3·36	4·70	6·30	9·40	·09	·15	25·50?	1,413	1,884	2,826	·50	·50	76	Elongation very doubtful.
				Same as above.			2d Sample tested.						–	–	67·50	–	–	14·00		Not calculated.		„	„	77 & 78	Core coiled up in 4 loops.
				Do.			3d do.						3·75	5·00	7·50	·28	2·60	21·74	1,130	1,507	2,260	„	„	79	Weights hung on direct.
				Do.			4th do.						3·72	5·00	7·4375	·28	2·30	25·00	1,121	1,507	2,240	„	„	80	
20	3·57	–	–	–	–	–	–	–	–	–	3·57	3·125	2·87	3·83	5·750	·14	·43	?		Not calculated.		–	16	81	Copper strand only.
16	3·57	3·57	–	7·15	6·58	–	–	–	–	–	21·62	5·58	18·75	25·00	37·56	·75	1·42	2·46	3,382	4,510	6,765	·50	1·00	82	Tweed and Rogers' patent 2d sample tested.
22	3·57	3·57		Not known.			Specimen too short.				–	–	27·50	36·67	55·00	1·28	1·72	2·93		Not known.		·50	·750	83	
23	1·61	1·90		Do.			do.				–	–	20·00	26·67	40·00	·50	1·37	2·46		Do.		·562	·625	84	
24	3·57	3·57	2·00	–	–	32·08	–	18	18	12	41·22	30·60	82·50	87·00	130·00	·22	·33	·73	2,224	2,811	4,447	·50	·750	85	
25	3·57	3·57	6·09	–	–	46·28	–	18	18	10	58·51	41·70	82·50	110·00	165·00	·41	·52	1·25	2,006	2,675	4,012	·50	·925	86	
26	3·57	3·57	6·09	–	11·49	11·71	–	12	12	14	36·43	15·35	42·50?	54·67?	85·00?	·91?	2·40?	3·90?	2,809	3,613	5,618	·50	1·125	87	One wire broke at 50 cwt.
27				Particulars not known.							Not known.		10·00	13·33	20·00	Broke immediately.				Not known.		–	·719	88	Tweed and Rogers' patent.
				Do.			2d Sample tested.				Do.		8·75	11·47	17·50	Sample too short.				Do.		–	·656	88	Tweed and Rogers' patent.
				Do.			3d do.				Do.		7·50	10·00	15·00	·50	2·44	4·47		Do.		–	·75	„	No particulars given.
28	3·57	3·57	–	4·26	4·52	10·40	–	54	18	20	26·32	13·67	31·25	41·67	62·50	·51	·78	1·56	2,325	3,094	4,641	·50	·875	89	Henley.
				Same as above.			2d Sample tested.				„	„	33·50	44·67	67·00	·50	·77	1·80	2,505	3,339	5,009	„	„	90	
9				Do.			12th do.				25·47	13·11	73·75	98·38	147·50	·99	1·54	4·06	5,704	7,605	11,407	·50	·875	91 & 92	
				Do.			13th do.				„	„	71·25	95·00	142·50	·72	1·11	2·70?	5,510	7,346	11,019	„	„	93	Elongation at breaking too low.
12				Do.			8th do.				21·00	17·30	43·75	58·34	87·50	·25	·35	·81	2,557	3,409	5,114	·50	·562	94	Red Sea Cable.

A
APP. No. 10.

APPENDIX No. 10.—*continued.*

RESULTS of EXPERIMENTS made by Messrs. GISBORNE and FORDE and C. W. SIEMENS for the purpose of arriving at the best form for Outer Covering of SUBMARINE TELEGRAPHIC CABLES.

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P. 115	Continuity. P. P. 115	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°		°	*Cwts.*	*Cwts.*	*Fms.*	
1859: June 17	No. 1. (Iron and Hemp.) Outside diameter ·75 inch. 1st Sample tested.	1	2 27	1·00	—	10·00	100·00	0·00	—	65·0			21·64	12·63	800	
		2	28	1·00	—	20·00		·10	·10	—			—	—	1,600	
		3	32	1·00	—	30·00		·25	·15	—			—	—	2,401	
		4	32	·50	—	35·00		·25	·00	—			—	—	2,801	=½ Breaking strain.
		5	34	1·00	—	45·00		·40	·15	—			—	—	3,601	
						46·67		·45	—	—			—	—	3,734	=¾ Breaking strain.
		6	36	·25	—	47·50		·50	·10	—			—	—	3,802	
		7	38	·25	—	50·00		·65	·15	—			—	—	4,001	
		8	40	—	—	—		·70	·05	—			—	—	—	
		9	40	·25	—	52·50		·77	·07	—			—	—	4,201	
		10	42	·25	—	55·00		·85	·08	—			—	—	4,401	
		11	44	·25	—	57·50	101·00	·13	·28	—			—	—	4,601	
		12	46	·25	—	60·00		·20	·20	—			—	—	4,802	
		13	53	·25	—	62·50		·40	·20	—			—	—	5,001	
		14	55	·25	—	63·00		·55	·15	—			—	—	5,201	
		15	57	·25	—	67·50		·75	·20	—			—	—	5,401	
		16	3 00	·25	—	70·00		·75	·00	—			—	—	5,601	Broke at clamp; three strands broke. Hemp appeared to be slightly injured previous to testing.
July 1	No. 2. (12 Iron Wires yarned.) Outside diameter ·76 inch. 2d Sample tested.	1	11 30	1·00	—	10·00	100·00	·00	—				21·64	12·63	800	
		2		1·00	—	20·00		·08	·08	—			—	—	1,600	
		3		1·00	—	30·00		·17	·09	—			—	—	2,400	
		4		1·00	—	40·00		·30	·13	—			—	—	3,200	
		5	Weights put on at intervals of about one minute.	·50	—	45·00		—	—	—			—	—	3,601	Drew in clamps.
		1		1·00	—	10·00	100·00	·00	—				21·64	12·63	800	Continued July 1st.
		2		1·00	—	20·00		·07	·07	—			—	—	1,600	
		3		1·00	—	30·00		·18	·11	—			—	—	2,400	
		4		1·00	—	40·00		·29	·11	—			—	—	3,200	
		5		1·00	—	50·00		·43	·14	—			—	—	4,000	
		6		·50	—	55·00		·54	·11	—			—	—	4,401	
		7		·25	—	57·50		·60	·06	—			—	—	4,612	
		8		·25	—	—		—	—	—			—	—	—	Drew in clamps.
		1		1·00	—	10·00	98·00	·00	—				21·64	12·63	800	Continued same day.
		2		1·00	—	20·00		·05	·05	—			—	—	1,600	
		3		1·00	—	30·00		·11	·06	—			—	—	2,400	
		4		1·00	—	40·00		·18	·07	—			—	—	3,200	
		5		1·00	—	50·00		·27	·09	—			—	—	4,000	
		6		·50	—	55·00		·30	·03	—			—	—	4,401	
		7		·50	—	60·00		·36	·06	—			—	—	4,801	Continued.
		8		·25	—	62·50		·45	·09	—			—	—	5,001	
		9		·25	—	65·00		·61	·16	—			—	—	5,201	Continued same day.
		10		·25	—	67·50		·80	·19	—			—	—	5,401	
		11		·25	—	70·00	99·07	·27		—			—	—	5,601	
		12		·25	—	72·50		·31	·24	—			—	—	2,801	
		13		·25	—	75·00		·56	·24	—			—	—	3,001	
		14		·25	—	77·50		—	—	—			—	—	3,201	Drew in clamps.
		1		1·00	—	10·00	100·00	·00	—				21·64	12·63	800	Continued same day.
		2		1·00	—	20·00		·04	·04	—			—	—	1,600	
		3		1·00	—	30·00		·16	·12	—			—	—	2,400	
		4		1·00	—	40·00		·23	·07	—			—	—	3,200	
		5		1·00	—	50·00		·34	·11	—			—	—	4,000	
		6		1·00	—	60·00		·41	·07	—			—	—	4,801	
		7		·50	—	65·00		·48	·07	—			—	—	5,201	
		8		·50	—	70·00		·55	·07	—			—	—	5,601	
		9		·25	—	72·00		—	—	—			—	—	—	Drew in clamps.
July 5		1		1·00	—	10·00	70·00	·00	—				21·64	12·63	800	Continued. Clamps refitted and test continued from 1st inst.
		2		1·00	—	20·00		·04	·04	—			—	—	1,600	
		3		1·00	—	30·00		·10	·06	—			—	—	2,400	
		4		1·00	—	40·00		·17	·07	—			—	—	3,200	
		5		·50	—	45·00		·20	·03	—			—	—	3,601	
		6		·25	—	50·00		·25	·05	—			—	—	4,001	
		7		·25	—	52·50		·27	·02	—			—	—	4,201	
		8		·25	—	55·00		·28	·01	—			—	—	4,401	
		9		·25	—	57·50		·31	·03	—			—	—	4,601	
		10		·25	—	60·00		·33	·02	—			—	—	4,801	
		11		·25	—	62·50		·34	·01	—			—	—	5,001	
		12		·25	—	65·00		·36	·02	—			—	—	5,201	
		13		·25	—	67·50		·40	·04	—			—	—	5,401	
		14		·25	—	70·00		·44	·04	—			—	—	5,601	
		15		·25	—	72·50		·50	·06	—			—	—	5,801	
		16		·25	—	75·00		·59	·09	—			—	—	6,002	Slipped in clamps.
		1		1·00	—	10·00	70·00	·00	—				21·64	12·63	800	Continued same day.
		2		1·00	—	20·00		·05	·05	—			—	—	1,600	Clamps refitted and tests continued same day.
		3		1·00	—	30·00		·10	·05	—			—	—	2,400	
		4		1·00	—	40·00		·17	·07	—			—	—	3,200	
						42·50		·19	—	—			—	—	3,402	=¼ Breaking strain.
		5		1·00	—	50·00		·25	·08	—			—	—	4,001	
						58·67		·29	—	—			—	—	4,532	=⅔ Breaking strain.
		6		1·00	—	60·00		·31	·06	—			—	—	4,801	
		7		·50	—	65·00		·36	·05	—			—	—	5,201	
		8		·50	—	70·00		·40	·04	—			—	—	5,601	
		9		·50	—	75·00		·49	·09	—			—	—	6,002	
		10		·25	—	77·50		·58	·09	—			—	—	6,203	
		11		·25	—	80·00		·65	·07	—			—	—	6,402	
		12		·25	—	82·50		·75	·10	—			—	—	6,602	
		13		·25	—	85·00		—	—	—			—	—	6,802	Broke fair. This specimen clamped six times. Eight strands broke. Per-centage of Elongation: Safe strain = ·27 per cent. Ma. do. = ·41 " Breaking do. = 1·07 " Memo.—Elongation doubtful, specimen being clamped six times.

Tested in Air.

Weights put on at intervals of about one minute.

Appendix No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*				*Cwts.*	*Cwts.*	*Fms.*	
1859. June 20	No. 2. (Hemp and Iron.) Outside diameter ·75 inch. 1st Sample tested.	1	About one minute between each additional weight.	1·00	—	10·00	100·00	—	—	Tested in Air.			18·13	8·01	1,265	
		2		1·00	—	20·00	·13	·15	—				—	—	2,529	
		3		1·00	—	30·00	·35	·20	—				—	—	3,794	
						31·25	·39	—	—				—	—	3,952	½ Breaking strain.
		4		·50	—	35·00	·63	·18	—				—	—	4,426	
		5		·50	—	40·00	101·01	·48	—				—	—	5,059	
						41·67	·21	—	—				—	—	5,270	½ Breaking strain.
		6		·25	—	42·50	·40	·39	—				—	—	5,374	
		7		·25	—	45·00	·63	·23	—				—	—	5,691	
		8		·25	—	47·50	·80	·17	—				—	—	6,007	
		9		·25	—	50·00	102·00	·20	—				—	—	6,323	
		10		·25	—	52·50	·20	·20	—				—	—	6,639	
		11		·25	—	55·00	·40	·20	—				—	—	6,956	
		12		·25	—	57·50	·58	·18	—				—	—	7,272	
		13		·25	—	60·00	·79	·12	—				—	—	7,588	
		14		·25	—	62·50	·90	·20	—				—	—	7,903	Broke very fairly seven strands in different places. Core not much indented.
July 4	No. 3. (Iron and Hemp.) Outside diameter ·75 inch. 2d Sample tested.	1	—	1·00	—	10·00	100·00	·00	—	Tested in Air.			18·13	8·01	1,265	
		2	—	1·00	—	20·00	·16	·16	—				—	—	2,529	
		3	—	1·00	—	30·00	·35	·19	—				—	—	3,794	
						32·50	·41	—	—				—	—	4,110	=½ Breaking strain.
		4	—	1·00	—	40·00	·66	·31	—				—	—	5,059	
						43·34	·86	—	—				—	—	5,480	=¾ Breaking strain.
		5	—	·00	—	45·00	·90	·24	—				—	—	5,691	
		6	—	·00	—	50·00	101·30	·40	—				—	—	6,323	
		7	—	·25	—	52·50	·95	·65	—				—	—	6,639	
		8	—	·25	—	55·00	102·15	·20	—				—	—	6,956	
		9	—	·25	—	57·50	·32	·17	—				—	—	7,272	
		10	—	·25	—	60·00	·47	·15	—				—	—	7,588	
		11	—	·25	—	62·50	·77	·30	—				—	—	7,903	
		12	—	·25	—	65·00	—	—	—				—	—	8,219	Broke fair; eight strands broken.
June 18	No. 3. (Hemp and Iron.) Outside diameter ·75 inch. 1st Sample tested.	1	2 46	1·00	—	10·00	100·00	·00	—	Tested in Water.	No deflection of Galvanometer.	No deflection of Galvanometer.	21·25	11·41	888	Lay of cable might be straighter.
		2	48	1·00	—	28·00	·35	·35	—				—	—	1,776	
		3	58	—	1·00	10·00	—	—	—				—	—	888	
		4	3 00	—	1·00	0·00	·25	—	·10				—	—	—	Scale not properly fixed; elongation not certain.
				0·00	—	0·00	100·00	·00	—				21·25	11·41	—	Recommenced.
		1	3 12	1·00	—	10·00	·10	·10	—				—	—	888	
		2	13	1·00	—	20·00	·20	·10	—				—	—	1,776	
		3	14	1·00	—	30·00	·35	·15	—				—	—	2,663	=¼ Breaking strain.
		4	15	·50	—	35·00	·45	·10	—				—	—	3,107	
		5	17	·50	—	40·00	·60	·15	—				—	—	3,551	=½ Breaking strain.
		6	19	·50	—	45·00	·75	·15	—				—	—	3,995	
		7	21	·25	—	47·50	·85	·10	—				—	—	4,217	
		8	23	·25	—	50·00	101·15	·30	—				—	—	4,439	
		9	25	·25	—	52·50	·32	·17	—				—	—	4,661	
		10	27	·25	—	55·00	·75	·43	—				—	—	4,883	
		11	31	·25	—	57·50	102·20	·45	—				—	—	5,105	
		12	33	·25	—	60·00	·50	·30	—				—	—	5,327	Stretched rapidly and broke near centre of trough; four strands broke. Core twisted apparently in consequence of short lay. Elongation doubtful; specimen clamped twice.
June 24	No. 3. (Iron and Hemp.) Outside diameter ·75 inch. 2d Sample tested.	1	3 10	1·00	—	10·00	100·00	·00	—	Tested in Air.			21·25	11·41	888	
		2	—	1·00	—	20·00	·08	·08	—				—	—	1,776	
		3	—	1·00	—	30·00	·28	·20	—				—	—	2,663	=¼ Breaking strain.
		4	—	·50	—	35·00	·38	·10	—				—	—	3,107	
		5	—	·50	—	40·00	·51	·13	—				—	—	3,551	=½ Breaking strain.
		6	—	·25	—	42·50	·60	·09	—				—	—	3,773	
		7	—	·25	—	45·00	·68	·08	—				—	—	3,995	
		8	—	·25	—	47·50	·80	·13	—				—	—	4,217	
		9	—	·25	—	50·00	·96	·16	—				—	—	4,439	
		10	—	·125	—	52·50	101·18	·22	—				—	—	4,661	
		11	—	·125	—	55·00	·47	·29	—				—	—	4,883	
		12	—	·125	—	56·25	·58	·11	—				—	—	4,994	
		13	—	·125	—	57·50	·71	·13	—				—	—	5,105	
		14	—	·125	—	58·75	—	—	—				—	—	5,216	Fairly broken; five strands parted at different places.
June 18	No. 3. (Hemp and Steel.) Outside diameter ·75 inch. 1st Sample tested.	1	3 35	1·00	—	10·00	100·00	·00	—	Tested in Air.	—	—	20·65	10·75	942	
		2	38	1·00	—	20·00	·15	·15	—		—	—	—	—	1,885	
		3	4 00	1·00	—	30·00	·33	·18	—		—	—	—	—	2,827	
		4	1	1·00	—	40·00	·56	·23	—		—	—	—	—	3,767	
		5	3	1·00	—	50·00	·77	·21	—		—	—	—	—	4,719	
		6	5	1·00	—	60·00	101·05	·28	—		—	—	—	—	5,653	
		7	8	1·00	—	70·00	·41	·36	—		—	—	—	—	6,596	Gave in clamps; specimen uninjured; to be continued.
																Continued 20th June.
		1	About one minute between each additional weight.	1·00	—	10·00	100·00	0·00	—	Tested in Air.	—	—	20·65	10·75	942	Test continued from Saturday 18th June 1859.
		2		1·00	—	20·00	·10	·10	—		—	—	—	—	1,885	
		3		1·00	—	30·00	·28	·18	—		—	—	—	—	2,827	
		4		1·00	—	40·00	·40	·12	—		—	—	—	—	3,767	=½ Breaking strain.
		5		1·00	—	50·00	·56	·16	—		—	—	—	—	4,712	
						53·34	·62	—	—		—	—	—	—	5,026	=⅔ Breaking strain.
		6		1·00	—	60·00	·73	·17	—		—	—	—	—	5,654	
		7		·50	—	65·00	·83	·10	—		—	—	—	—	6,125	
		8		·50	—	70·00	·95	·12	—		—	—	—	—	6,597	
		9		·50	—	75·00	101·10	·15	—		—	—	—	—	7,068	
		10		·50	—	80·00	·50	·40	—		—	—	—	—	7,539	
		—		—	—	—	·66	·06	—		—	—	—	—	—	

APPENDIX No. 10.—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
1859: June 18	No. 3. (Hemp and Steel.) Outside diameter ·75 inch. 1st Sample tested.	11	About one minute to each additional weight.	·50	—	85·00	102·95	·49	—	Tested in Air.			20·65	10·75	8,010	
		12		·59	—	90·00	·69	·55	—				—	—	8,481	
		13		·25	—	92·50	·70	·10	—				—	—	8,717	Broke; two strands near clamp, and one in middle. Core very much twisted.
		14		—	1·25	86·00	·90	·20	—				—	—	7,539	Three more strands broke. Core more twisted and indented. Core to be tested under pressure.
																Memo.—Elongation doubtful, specimen being twice clamped.
July 19	No. 3. (Steel and Hemp.) Outside diameter ·75 inch. 2d Sample tested.	1	5 27	1·00	—	10·00	100·00	·00	—	Tested in Air.			20·65	10·75	942	
		2	32	2·00	—	30·00	·44	·44	—				—	—	2,827	
						47·50	·75	—	—				—	—	4,476	= ½ Breaking strain.
		3	34	2·00	—	50·00	·86	·22	—				—	—	4,712	
		4	36	1·00	—	60·00	101·13	·27	—				—	—	5,654	
						65·33	·25	—	—				—	—	5,969	Breaking strain.
		5	38	·50	—	68·00	·38	·13	—				—	—	9,132	
		6	40	·50	—	70·00	·43	·14	—				—	—	6,597	
		7	42	·50	—	75·00	·66	·24	—				—	—	7,068	
		8	44	·25	—	77·50	·77	·11	—				—	—	7,304	
		9	46	·25	—	80·00	·97	·20	—				—	—	7,539	
		10	48	·25	—	82·50	102·15	·18	—				—	—	7,775	
		11	50	·25	—	85·00	·37	·22	—				—	—	8,010	
		12	52	·25	—	87·50	·65	·28	—				—	—	8,246	
		13	54	·25	—	90·00	103·00	·35	—				—	—	8,481	
		14	56	·25	—	92·50	·30	·30	—				—	—	8,717	
		15	58	·25	—	95·00	—	—	—				—	—	8,953	Six strands broke. Very fair.
June 17	No. 4. (Hemp only.) Outside diameter ·63 inch. 1st Sample tested.	1	Weights added at intervals of about two minutes.	·25	—	2·50	100·00	—	—	Water about 65° Fahrenheit.	Not affected except where core was pricked. (See No. 10.)	Not affected.	11·70	3·36	754	Not a fair test, two or three of the strands being previously cut near clamp.
		2		·25	—	5·00	99·55	—	·05				—	—	1,508	Contraction of length of sample consequent on contraction of hemp in water.
		3		·25	—	7·50	100·00	·05	—				—	—	2,262	
						8·75	·12	·40	—				—	—	2,638	= ½ Breaking strain.
		4		·50	—	12·50	·45	·45	—				—	—	3,768	11·75 cwts. = 338 fathoms = ¾ Breaking strain.
		5		·25	—	15·00	·60	·15	—				—	—	4,522	
		6		·25	—	17·50	·72	·12	—				—	—	5,276	
		7		·06	—	17·56	·90	·18	—				—	—	5,464	
		8		·06	—	17·62	101·00	·10	—				—	—	5,623	
		9		—	·25	13·12	—	—	—				—	—	—	Broke several strands near clamp. Core not broken, and apparently perfect.
		10		—	·25	12·62	102·00	1·00	—				—	—	—	Core pricked with piece of iron, and connected with galvanometer, which showed deflection.
		11		—	2·56	10·06	—	—	—				—	—	—	As core stretched insulation became perfect.
		12		—	2·56	7·50	—	—	—				—	—	—	
		13		·06	—	8·10	103·10	3·10	—				—	—	—	
		14		·06	—	8·70	106·85	1·75	—				—	—	—	
		15		—	·06	8·10	107·50	·70	—				—	—	—	
		16		—	—	8·10	108·00	·50	—				—	—	—	Core not broken. Continuity and insulation perfect.
July 7	No. 5. (Outside iron wires covered with gutta percha.) Outside diameter ·875 inch. 1st Sample tested.	1		1·00	—	10·00	100·00	—	—	Tested in Air.			23·80	11·80	859	
		2		·50	—	15·00	·94	·04	—				—	—	1,289	
		3		·50	—	20·00	·10	·06	—				—	—	1,719	
						27·50	·23	—	—				—	—	2,363	= ½ Breaking strain.
		4		1·00	—	30·00	·28	·18	—				—	—	3,578	
						37·67	·42	—	—				—	—	3,151	= ¾ Breaking strain.
		5		1·00	—	40·00	·50	·22	—				—	—	3,437	
		6		·50	—	45·00	·65	·15	—				—	—	3,867	
		7		·50	—	50·00	·96	·31	—				—	—	4,297	
		8		·50	—	55·00	—	—	—				—	—	4,726	Broke fairly near clamp; all 12 wires. Core apparently uninjured, except at break.
July 15	No. 5. (Outside iron wires covered with gutta percha.) Outside diameter ·875 inch. 2d Sample tested.	1	3 25	1·00	—	10·00	100·00	·00	—	78	108	1	23·80	11·80	859	
		2	29	1·00	—	20·00	·10	·10	—				—	—	1,719	Tested in water and electrically.
						25·00	·16	—	—				—	—	—	
		3	31	1·00	—	30·00	·22	·12	—				—	—	2,578	= ¼ Breaking strain.
						33·33	·27	—	—				—	—	—	
		4	33	·50	—	35·00	·30	·08	—				—	—	3,008	= ¾ Breaking strain.
		5	35	·50	—	40·00	·49	·10	—				—	—	3,437	
		6	37	—	·50	35·00	·37	—	·03				—	—	—	
		7	39	—	·50	30·00	·34	—	·03				—	—	—	
		8	41	—	1·00	20·00	·28	—	·06				—	—	—	
		9	43	—	1·00	10·00	·20	—	·08				—	—	—	
		10	47	1·00	—	20·00	·25	·05	—				—	—	1,709	
		11	49	1·00	—	30·00	·34	·09	—				—	—	2,578	
		12	51	·50	—	35·00	·37	·03	—				—	—	3,008	
		13	53	·50	—	40·00	·42	·05	—				—	—	3,437	
		14	55	·50	—	45·00	·55	·13	—				—	—	3,867	
		15	57	·25	—	47·50	·65	·10	—				—	—	4,082	
		16	59	·25	—	50·00	—	—	—				—	—	4,297	All broke, core included.
June 28	No. 6. (Steel and Hemp.) Wright and Co. Outside diameter ·87 inch. 1st Sample tested.	1	2 35	1·00	—	10·00	99·00	·00	—	Tested in Air.			26·80	13·80	735	
		2		1·00	—	20·00	·13	·13	—				—	—	1,470	Specimen laid up by hand.
		3		1·00	—	30·00	·27	·14	—				—	—	2,204	
		4		1·00	—	40·00	·45	·18	—				—	—	2,939	
		5		1·00	—	50·00	·58	·13	—				—	—	3,674	
						56·25	·68	—	—				—	—	4,228	= ½ Breaking strain.
		6		1·00	—	60·00	·74	·16	—				—	—	4,408	
		7		·50	—	65·00	·83	·09	—				—	—	4,776	
		8		·50	—	70·00	·90	·07	—				—	—	5,144	
		9		·50	—	75·00	·97	·07	—				—	—	5,511	= ¾ Breaking strain.
		10		·50	—	80·00	97·03	·06	—				—	—	5,878	
		11		·25	—	82·50	·09	·06	—				—	—	6,062	
		12		·25	—	85·00	·11	·02	—				—	—	6,246	Per-centage of elongation:
		13		·25	—	87·50	·14	·03	—				—	—	6,429	¼ Breaking strain = ·71
		14		·25	—	90·00	·20	·06	—				—	—	6,613	½ Do. do. = 1·01
			Weights added at intervals of about one minute.	·25	—	92·50	·25	·05	—				—	—	6,797	¾ Breaking do. = 1·01

APPENDIX No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
1859: June 28	No. 6. (Steel and Hemp.) Wright and Co. Outside diameter ·87 inch. 1st Sample tested.	16	Weights put on at intervals of about one minute.	·25	—	93·00	·30	·05	—	Tested in Air			26·80	13·80	6,982	
		17		·25	—	97·50	·32	·02	—				—	—	7,164	
		18		·25	—	100·00	·37	·03	—				—	—	7,348	
		19		·25	—	102·50	·40	·03	—				—	—	7,715	
		20		·25	—	105·00	·44	·04	—				—	—	7,899	
		21		·25	—	107·50	·49	·05	—				—	—	8,083	
		22		·25	—	110·00	·55	·06	—				—	—	8,266	
		23		·25	—	112·50	—	—	—				—	—	8,450	Broke in clamp from excess of tar. (Specimen very badly laid up) by hand.
June 17	No. 7. (Iron and Hemp.) Mr. Wright, covered by hand. Outside diameter ·87 inch. 1st Sample tested.	1	3 42	1·00	—	10·00	100·00	0·00	—	About 60 degrees Fahrenheit.	No deflection of the needle.	No deduction of the needle.	26·80	13·80	735	
			44	—	—	—	—	—	—				—	—	—	Specimen laid up by hand.
		2	46	1·00	—	20·00	101·53	·70	—				—	—	1,470	
		3	48	·50	—	25·00	·85	·20	—				—	—	1,837	
		4	50	·50	—	30·00	102·00	·15	—				—	—	2,204	
		5	53	·50	—	35·00	·10	·10	—				—	—	2,571	
						37·50	·20	—	—				—	—	2,755	=½ Breaking strain.
		6	54	·50	—	40·00	·25	·10	—				—	—	2,939	
		7	56	·50	—	45·00	·50	·25	—				—	—	3,306	
		8	58	·25	—	47·50	—	—	—				—	—	3,490	
		9	4 00	·25	—	50·00	·65	·15	—				—	—	3,674	=¾ Breaking strain.
		10	2	·25	—	52·50	—	—	—				—	—	3,858	Drew in clamp (to be continued).
June 30		1		1·00	—	10·00	100·33	0·00	—	Tested in Air.			6·80	13·80	735	Continued 30th June.
		2		1·00	—	20·00	·75	·42	—				—	—	1,470	
		3	About one minute between each addition.	1·00	—	30·00	101·02	·27	—				—	—	2,204	=½ Breaking strain.
						37·50	·15	—	—				—	—	—	
		4		1·00	—	40·00	·20	·18	—				—	—	2,939	
		5		1·00	—	80·00	·59	·39	—				—	—	3,674	=½ Breaking strain.
		6		·25	—	52·50	·67	·08	—				—	—	3,858	
		7		·25	—	55·00	·89	·13	—				—	—	4,041	
		8		·25	—	57·50	·92	·13	—				—	—	4,225	
		9		·25	—	60·00	102·06	·14	—				—	—	4,408	
		10		·25	—	62·50	·20	·14	—				—	—	4,592	
		11		·25	—	65·00	·28	·08	—				—	—	4,776	
		12		·25	—	67·50	·38	·10	—				—	—	4,960	
		13		·25	—	70·00	·50	·12	—				—	—	5,144	
		14		·25	—	72·50	·65	·15	—				—	—	5,327	
		15		·25	—	75·00	—	—	—				—	—	5,511	Broke ten strands, three inches from clamp; outer covering only unravelled about two feet back after break. Core not so much indented as with single strands steel wire. Elongation very doubtful, being twice clamped.
June 17	No. 8. (Iron and Hemp.) Outside diameter ·875 inch. 1st Sample tested.	1	Weights added at intervals of about 1½ minutes.	·50	—	5·00	100·00	—	—	Specimen tested in Air	Not tested electrically.	Not tested electrically.	26·29	13·75	369	Specimen merely straightened.
		2		·25	—	7·50	—	—	—				—	—	553	
		3		·25	—	10·00	—	—	—				—	—	737	
		4		·50	—	15·00	—	—	—				—	—	1,106	
		5		·50	—	20·00	—	—	—				—	—	1,475	
		6		·50	—	25·00	·15	·15	—				—	—	1,844	Memo.—¼ Breaking strain and ½ breaking strain not taken out, the sample not being broken.
		7		·50	—	30·00	·20	·05	—				—	—	2,212	
		8		·50	—	35·00	·25	·05	—				—	—	2,581	
		9		·50	—	40·00	·30	·05	—				—	—	2,950	
		10		·50	—	45·00	·33	·10	—				—	—	3,318	
						46·25	·36	—	—				—	—	—	
		11		·50	—	50·00	·40	·05	—				—	—	3,687	
		12		·50	—	55·00	·50	·05	—				—	—	4,056	
		13		·50	—	60·00	·65	·15	—				—	—	4,425	
						61·67	·78	—	—				—	—	—	
		14		·25	—	62·50	·90	·25	—				—	—	4,609	Not broken, and apparently uninjured. Slipped in clamps.
June 20	No. 8. (Iron and Hemp.) Outside diameter ·875 inch. 2d Sample tested.	1	About one minute between each weight.	1·00	—	10·00	100·00	—	—	Tested in Air.			26·29	13·75	737	
		2		1·00	—	20·00	·22	·22	—				—	—	1,475	
		3		1·00	—	30·00	·40	·18	—				—	—	2,212	
		4		1·00	—	40·00	·55	·15	—				—	—	2,950	
						46·25	·71	—	—				—	—	—	=½ Breaking strain.
		5		1·00	—	50·00	·80	·25	—				—	—	3,687	
		6		·50	—	55·00	·90	·10	—				—	—	4,056	
		7		·50	—	60·00	101·16	·26	—				—	—	4,425	
						61·67	·18	—	—				—	—	—	=¾ Breaking strain.
		8		·25	—	62·50	·25	·15	—				—	—	4,609	Slipped in clamp; to be continued.
		1	About one minute between each weight.	1·00	—	10·00	100·00	—	—	Tested in Air.			26·29	13·75	737	
		2		2·00	—	30·00	·20	·20	—				—	—	212	=½ Breaking strain.
						46·25	—	—	—				—	—	—	
		3		2·00	—	80·00	·41	·21	—				—	—	3,687	
		4		1·00	—	60·00	·52	·11	—				—	—	4,425	
						61·67	—	—	—				—	—	—	=¾ Breaking strain.
		5		·50	—	63·00	·63	·16	—				—	—	4,794	
		6		·50	—	70·00	101·00	·32	—				—	—	5,163	
		7		·25	—	72·50	·20	·20	—				—	—	5,347	
		8		·25	—	75·00	·53	·53	—				—	—	5,531	
		9		·25	—	77·50	·75	·22	—				—	—	5,716	
		10		·25	—	80·00	·00	·15	—				—	—	5,900	
		11		·25	—	82·50	102·08	·18	—				—	—	6,084	
		12		·25	—	85·00	·22	·14	—				—	—	6,269	Elongation doubtful, being clamped twice.
		13		·25	—	87·50	·45	·23	—				—	—	6,453	
		14		·25	—	90·00	·53	·08	—				—	—	6,637	
		15		·25	—	92·50	·68	·15	—				—	—	6,821	
						92·50	·80	·12	—				—	—	—	Eight strands broke near clamp very fairly; four strands left good.

APPENDIX TO REPORT OF THE

APPENDIX No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
July 22	No. 8. (Iron and Hemp.) Outside diameter ·875 inch. Twelve welds in 100 inches, and one gutta percha joint. 3d Sample tested.	1	2 48	1·00	—	10·00	100·00	·00	—	70	100	1	26·29	13·75	737	
		2	51	1·00	—	20·00	·21	·21	—	—			—	—	1,475	A slight loss of insulation before weights were put on, which did not get worse.
		3	53	1·00	—	30·00	·39	·38	—	—			—	—	2,212	
		4	55	·50	—	35·00	·53	·10	—	—			—	—	2,581	= ½ Breaking strain.
						37·50	·55	—	—	—			—	—	2,765	
		5	57	·50	—	46·00	·70	·10	—	—			—	—	2,950	
		6	59	·50	—	45·00	·70	·11	—	—			—	—	3,318	
		7	3 1	·00	—	50·00	·85	·15	—	—			—	—	3,687	= ⅔ Breaking strain.
		8	3	·50	—	55·00	101·09	·24	—	—			—	—	4,056	
		9	5	·50	—	60·00	·43	·33	—	—			—	—	4,425	
		10	7	·25	—	62·50	·62	·20	—	—			—	—	4,609	
		11	9	·25	—	65·00	·87	·25	—	—			—	—	4,794	
		12	11	·25	—	67·50	102·10	·23	—	—			—	—	4,978	
		13	13	·25	—	70·00	·36	·26	—	—			—	—	5,162	
		14	15	·25	—	72·50	·63	·26	—	—			—	—	5,347	
		15	17	·25	—	75·00	—	—	—	—			—	—	5,531	Six wires broken (three at weld). To be tested again electrically.
June 22	No. 8. (Iron and Hemp.) Outside diameter ·875 inch. Twelve welds in 100 inches, one gutta percha joint. 4th Sample tested.	1	1 59	1·00	—	10·00	100·00	·00	—	70			26·29	13·75	737	
		2	53	1·00	—	20·00	·21	·21	—	—			—	—	1,475	A slight loss of insulation before weights were put on, which did not get worse.
		3	55	1·00	—	30·00	·43	·21	—	—			—	—	2,212	
		4	57	·50	—	35·00	·52	·10	—	—			—	—	2,581	= ½ Breaking strain.
						37·50	·57	—	—	—			—	—	2,765	
		5	59	·50	—	40·00	·62	·10	—	—			—	—	2,950	
		6	2 1	·00	—	45·00	·74	·12	—	—			—	—	3,318	
		7	3	·50	—	50·00	·82	·08	—	—			—	—	3,687	= ⅔ Breaking strain.
		8	5	·50	—	55·00	·99	·17	—	—			—	—	4,056	
		9	7	·50	—	60·00	101·33	·35	—	—			—	—	4,425	
		10	9	·25	—	62·50	·53	·20	—	—			—	—	4,609	
		11	11	·25	—	65·00	·82	·27	—	—			—	—	4,794	
		12	13	·25	—	67·50	102·15	·33	—	—			—	—	4,978	
		13	15	·25	—	70·00	·46	·35	—	—			—	—	5,162	
		14	17	·25	—	72·50	·63	·23	—	—			—	—	5,347	
		15	19	·25	—	75·00	—	—	—	—			—	—	5,531	Eight strands broke in all (four at weld) to be tested again electrically.
July 21	No. 8. (Iron and Hemp.) Outside diameter ·875 inch. Six welds in 100 inches, one gutta percha joint. 5th Sample tested.	1	20	1·00	—	10·00	100·00	·00	—	63	100	1	26·29	13·75	737	
		2	23	1·00	—	20·00	·33	·32	—	—			—	—	1,475	
		3	25	1·00	—	30·00	·50	·18	—	—			—	—	2,212	
		4	29	1·00	—	40·00	·69	·19	—	—			—	—	2,950	= ½ Breaking strain.
		5	31	·50	—	45·00	·80	·11	—	—			—	—	3,318	
		6	33	·50	—	50·00	·91	·11	—	—			—	—	3,687	
						55·33	101·03	—	—	—			—	—	3,933	= ⅔ Breaking strain.
		7	35	·50	—	55·00	·19	·19	—	—			—	—	4,056	
		8	37	·50	—	60·00	·45	·35	—	—			—	—	4,425	
		9	39	·25	—	62·50	·46	·21	—	—			—	—	4,609	
		10	41	·25	—	65·00	·64	·27	—	—			—	—	4,794	
		11	43	·25	—	67·50	102·31	·38	—	—			—	—	4,978	
		12	45	·25	—	70·00	·64	·37	—	—			—	—	5,162	
		13	47	·25	—	72·50	·86	·18	—	—			—	—	5,347	
		14	50	·25	—	75·00	103·00	·14	—	—			—	—	5,531	One wire broke at weld.
		15	51	·25	—	77·50	·20	·20	—	—			—	—	5,716	
		16	53	·25	—	80·00	—	—	—	—			—	—	5,900	Eight strands broken (four broke at weld), one broke in two places at weld and at solid. Five welds in all broken.
July 21	No. 8. (Iron and Hemp.) Outside diameter ·875 inch. Six welds in 100 inches, one gutta percha joint. 6th Sample tested.	1	3 35	1·00	—	10·00	100·00	·00	—	63	100	1	26·29	13·75	737	
		2	37	1·00	—	20·00	·16	·16	—	—			—	—	1,475	
		3	39	1·00	—	30·00	·34	·18	—	—			—	—	2,212	
						36·25	·46	—	—	—			—	—	2,673	= ½ Breaking strain.
		4	41	1·00	—	40·00	·53	·19	—	—			—	—	2,950	
		5	43	·50	—	45·00	·64	·11	—	—			—	—	3,318	
						48·33	·72	—	—	—			—	—	3,565	= ⅔ Breaking strain.
		6	45	·50	—	50·00	·78	·14	—	—			—	—	3,687	
		7	47	·50	—	55·00	·93	·20	—	—			—	—	4,056	
		8	49	·50	—	60·00	101·24	·36	—	—			—	—	4,425	
		9	51	·25	—	62·50	·63	·29	—	—			—	—	4,609	
		10	53	·25	—	65·00	·91	·28	—	—			—	—	4,794	
		11	55	·25	—	67·50	102·15	·30	—	—			—	—	4,978	
		12	57	·25	—	70·00	·75	·34	—	—			—	—	5,162	
		13	59	·26	—	72·50	—	—	—	—			—	—	5,347	Broke very slowly (seven strands in all). Four strands broke at weld.
July 23	No. 8. (Iron and Hemp.) Outside diameter ·875 inch. Without welds or gutta percha joint. 7th Sample tested.	1	11 50	1·00	—	10·00	100·00	·00	—				26·29	13·75	737	
		2	52	1·00	—	20·00	·23	·22	—				—	—	1,475	
		3	54	1·00	—	30·00	·46	·18	—				—	—	2,212	
		4	56	1·00	—	40·00	·65	·15	—				—	—	2,950	
						43·75	·62	—	—				—	—	3,226	= ½ Breaking strain.
		5	58	1·00	—	50·00	·75	·29	—				—	—	3,687	
						58·33	101·05	—	—				—	—	4,302	= ⅔ Breaking strain.
		6	12 0	1·00	—	60·00	·19	·44	—				—	—	4,425	
		7	2	·50	—	65·00	·38	·41	—				—	—	4,794	
		8	4	·50	—	70·00	102·06	·46	—				—	—	5,162	
		9	6	·25	—	72·50	·33	·27	—				—	—	5,347	
		10	8	·25	—	75·00	·54	·21	—	Tested in water, but not electrically, raining at the time. The core kept to be tested under water when stripped.			—	—	5,531	
		11	10	·25	—	77·50	·77	·23	—				—	—	5,716	
		12	12	·25	—	80·00	·96	·19	—				—	—	5,900	
		13	14	·25	—	82·50	103·13	·19	—				—	—	6,084	
		14	16	·25	—	85·00	·31	·17	—				—	—	6,269	
		15	18	·25	—	87·50	—	—	—				—	—	6,453	Nine strands broken.
July 23	No. 8. (Iron and Hemp.) Outside diameter ·875 inches. Without welds or gutta percha joint. 8th Sample tested.	1	2 25	1·00	—	10·00	100·00	1·00	—				26·29	13·75	737	
		2	28	1·00	—	20·00	·25	·25	—				—	—	1,475	
		3	30	1·00	—	30·00	·45	·20	—				—	—	2,212	
		4	32	1·00	—	40·00	·61	·16	—				—	—	2,950	= ½ Breaking strain.
		5	34	1·00	—	50·00	·80	·19	—				—	—	2,687	
						53·33	·94	—	—				—	—	3,933	= ⅔ Breaking strain.
		6	36	1·00	—	60·00	101·23	·43	—				—	—	4,425	
		7	38	·50	—	65·00	·63	·42	—				—	—	4,794	
		8	40	·50	—	70·00	102·15	·30	—				—	—	5,162	
		9	43	·50	—	75·00	·54	·39	—				—	—	5,531	
		10	44	·50	—	80·00	—	—	—				—	—	5,900	Seven strands broken.

APPENDIX No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen and Sample tested.	No.	Time	Tests for Strength. Weight. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859.			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
July 23	No. 8. (Iron and Hemp.) Outside diameter ·875 inch (without weld or gutta percha joints). 9th Sample tested.	1	3 1	1·00	—	10·00	100·00	·00	—	Tested in Water, but not electrically, raining at the time. The core kept to be tested under water when stripped			26·29	13·75	737	
		2	3	1·00	—	20·00	·20	·20	—				—	—	1,475	
		3	5	1·00	—	30·00	·40	·20	—				—	—	2,212	
						37·50	·50								2,765	=½ Breaking strain.
		4	7	1·00	—	40·00	·56	·16	—				—	—	2,950	
		5	9	1·00	—	50·00	·77	·21	—				—	—	3,687	=¾ Breaking strain.
		6	11	1·00	—	60·00	101·55	·78	—				—	—	4,425	
		7	13	1·00	—	70·00	103·17	·62	—				—	—	5,162	
		8	15	·50	—	75·00	—	—	—				—	—	5,531	7 strands broken.
July 25	No. 8. (Iron and Hemp.) Outside diameter ·875 inch (without weld or gutta percha joints). 10th Sample tested.	1	4 17	1·00	—	10·00	100·00	·00	—	69°			26·29	13·75	737	
		2	19	1·00	—	20·00	·21	·21	—	—			—	—	1,475	
		3	21	1·00	—	30·00	·36	·15	—	—			—	—	2212	
		4	23	1·00	—	40·00	·51	·15	—	—			—	—	2,950	
						42·50	·55								3,133	=½ Breaking strain.
		5	25	1·00	—	50·00	·67	·16	—	—			—	—	3,687	
						56·67	·87								4,179	=¾ Breaking strain.
		6	27	1·00	—	60·00	·88	·32	—	—	Perfect throughout.	Perfect throughout.	—	—	4,425	
		7	29	1·00	—	70·00	101·84	·85	—	—			—	—	5,162	
		8	31	·50	—	75·00	102·28	·44	—	—			—	—	5,531	
		9	33	·50	—	80·00	·62	·34	—	—			—	—	5,900	
		10	35	·50	—	85·00	103·15	·53	—	—			—	—	6,269	Broke very fair. 7 strands broken.
July 26	No. 8. (Iron and Hemp.) Outside diameter ·875 inch (without weld or gutta percha joints). 11th Sample tested.	1	11 40	1·00	—	10·00	100·00	·00	—	65·0	100	1	26·29	13·75	737	
		2	42	1·00	—	20·00	·15	·15	—	—			—	—	1,475	
		3	44	1·00	—	30·00	·32	·17	—	—			—	—	2,212	
		4	46	·50	—	35·00	·42	·10	—	—			—	—	2,581	
		5	48	·50	—	40·00	·50	·08	—	—			—	—	2,949	
						41·25	·52								3,041	=½ Breaking strain.
		6	50	·50	—	45·00	·59	·09	—	—			—	—	3,318	
		7	52	·50	—	50·00	·70	·11	—	—			—	—	3,686	
		8	54	·25	—	52·50	·76	·06	—	—			—	—	3,870	
		9	56	·25	—	55·00	·83	·07	—	—			—	—	4,055	=¾ Breaking strain.
		10	58	·25	—	57·50	·90	·07	—	—			—	—	4,239	
		11	12 0	·25	—	60·00	101·02	·12	—	—			—	—	4,423	
		12	2	·25	—	62·50	·21	·19	—	—			—	—	4,608	
		13	4	·25	—	65·00	·41	·20	—	—	Perfect throughout.	Perfect throughout.	—	—	4,792	
		14	6	·25	—	67·50	·72	·31	—	—			—	—	4,977	
		15	8	·25	—	70·00	102·06	·28	—	—			—	—	5,161	
		16	10	·25	—	72·50	·28	·23	—	—			—	—	5,345	
		17	12	·25	—	75·00	·50	·25	—	—			—	—	5,530	
		18	14	·25	—	77·50	·70	·20	—	—			—	—	5,714	
		19	16	·25	—	80·00	·87	·17	—	—			—	—	5,898	
		20	18	·25	—	82·50	—	—	—	—			—	—	6,083	Broke fair. 7 strands broken. 1 strand broken in two places.
June 20	No. 9. (Steel and Hemp with Hemp serving.) Outside diameter ·870 inch. (Clamped five times.) 1st Sample tested.	1	About 1 minute between each weight.	1·00	—	10·00	100·00	—	—				25·47	13·11	773	
		2		1·00	—	20·00	·32	·32	—				—	—	1,547	
		3		1·00	—	30·00	·62	·29	—				—	—	2,320	
		4		1·00	—	40·00	·71	·19	—				—	—	3,094	
		5		1·00	—	50·00	·86	·15	—				—	—	3,867	=¾ Breaking strain.
		6		1·00	—	60·00	101·03	·17	—				—	—	4,641	
						66·66	·17									=¾ Breaking strain.
		7		1·00	—	70·00	·23	·20	—				—	—	5,414	
		8		·50	—	75·00	·36	·13	—				—	—	5,801	
		9		·50	—	80·00	·50	·14	—				—	—	6,188	
		10		·50	—	85·00	·84	·34	—				—	—	6,575	
		11		·50	—	90·00	·93	·09	—				—	—	6,961	
		12		·50	—	95·00	102·23	·35	—				—	—	7,348	
		13		·50	—	100·00	·60	·37	—				—	—	7,735	Slipped in clamp; refitted. Cable apparently uninjured.
,,		1	About 1 minute between each weight.	1·00	—	10·00	99·00	—	—				—	—	773	Continued test same day.
		2		1·00	—	30·00	·14	·14	—				—	—	2,320	
		3		2,00	—	50·00	·32	·18	—				—	—	3,867	=½ Breaking strain.
						66·66										=¾ Breaking strain.
		4		2·00	—	70·00	·52	·20	—				—	—	5,414	
		5		2·00	—	90·00	·75	·23	—				—	—	6,961	
		6		1·00	—	100·00	·89	·05	—				—	—	7,735	Slipped in clamp; cable perfect. To be tried again. Continued on 21st June.
June 21		1	4 55	1·00	—	10·00	100·00	—	—				25·47	13·11	773	Continued from 20th June.
		2	—	2·00	—	30·00	·18	·18	—				—	—	2,320	
		3	—	2·00	—	50·00	·39	·21	—	Tested in Air.			—	—	3,867	=½ Breaking strain.
						66·66										=¾ Breaking strain.
		4	—	2·00	—	70·00	·58	·19	—				—	—	5,414	
		5	—	1·00	—	80·00	·70	·12	—				—	—	6,188	
		6	—	1·00	—	90·00	—	—	—				—	—	6,961	Drew in clamps. Cable uninjured.
June 22		1	3 23	1·00	—	10·00	100·00	—	—				—	—	773	Continued on 22d June.
		2	—	2·00	—	30·00	·21	·21	—				—	—	2,320	
		3	—	2·00	—	50·00	·41	·20	—				—	—	3,867	=½ Breaking strain.
						66·66										=¾ Breaking strain.
		4	—	2·00	—	70·00	·63	·22	—				—	—	5,414	
		5	—	1·00	—	80·00	·73	·10	—				—	—	6,188	
		6	—	1·00	—	90·00	—	—	—				—	—	6,961	One clamp slipped (tightened). Scale set at 0 with 90 cwt. of strain.
June 24		1	3 45	1·00	—	10·00	100·00	·00	—				—	—	773	Continued on 24th June. Some short bends in the specimens; the first amount of elongation not to be relied on.
		2	—	2·00	—	30·00	·18	·18	—				—	—	2,320	
		3	—	2·00	—	50·00	·37	·19	—				—	—	3,867	=½ Breaking strain.
						66·66										=¾ Breaking strain.
		4	—	2·00	—	70·00	·60	·23	—				—	—	5,414	
		5	—	1·00	—	80·00	·71	·11	—				—	—	6,188	
		6	—	1·00	—	90·00	·85	·14	—				—	—	6,961	
		7	—	1·00	—	100·00	·28	·43	—				—	—	7,735	1 Wire broke.
		8	—	·25	—	102·50	—	—	—				—	—	7,928	2 Strands broke.

APPENDIX No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen and Sample tested.	No.	Time	Tests for Strength. Weights. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
19 July	No. 9*. (Steel and Hemp with Hemp serving.) Outside diameter ·370 inch. 2d Sample tested.	1	4 0	1·00	—	10·00	50·00	·00	—				23·47	13·11	773	
		2	4	1·00	—	20·00	·27	·27	—				—	—	1,547	
		3	6	1·00	—	30·00	·44	·17	—				—	—	2,320	
		4	8	1·00	—	40·00	·55	·14	—				—	—	3,094	
		5	10	1·00	—	50·00	·60	·02	—				—	—	3,867	Scale shifted.
						58·75	·63	—	—				—	—	4,543	=½ Breaking strain.
		6	12	1·00	—	60·00	·68	·08	—				—	—	4,641	
		7	14	1·00	—	70·00	·79	·11	—				—	—	5,414	
						78·38	·91	—	—				—	—	6,088	=½ Breaking strain.
		8	16	1·00	—	80·00	51·12	·17	—				—	—	6,188	
		9	18	1·00	—	90·00	·23	·11	—				—	—	6,961	
		10	20	1·00	—	100·00*	·42	·23	—				—	—	7,735	
		11	22	·50	—	105·00	·39	·17	—	Tested in Air.			—	—	8,121	
		12	24	·50	—	110·00	—	—	—				—	—	8,508	Scale pan touched the ground.
		1	—	2·00	—	20·00	50·00	·00	—				—	—	1,547	Permanent stretch 2·40 in. per cent.
		2	—	4·00	—	40·00	·25	·25	—				—	—	3,094	
		3	—	2·00	—	30·00	·36	·11	—				—	—	2,188	
		4	—	2·00	—	100·00	·50	·14	—				—	—	7,735	
		5	—	·50	—	105·00	·53	·03	—				—	—	8,121	
		6	—	·50	—	110·00	·71	·06	—				—	—	8,508	
		7	—	·50	—	115·00	·75	·14	—				—	—	8,894	Six strands broken.
		8	—	·25	—	117·50	—	—	—				—	—	9,087	Estimated per-centage of elongation: =¼ Breaking strain = 1·247 per-cent. =½ Breaking strain = 1·82 per-cent. Breaking strain = 3·00 per-cent.
23 July	No. 9*. (Steel and Hemp.) Outside diameter ·375 inches. Twelve welds in 100 inches. One gutta percha joint. 3d Sample tested.	1	4 20	1·00	—	10·00	100·00	·00	—	70·75	100	1	23·47	13·11	773	
		2	25	1·00	—	20·00	·32	·32	—				—	—	1,547	A slight loss of insulation before weights were put on, which did not get worse.
		3	27	1·00	—	30·00	·54	·22	—				—	—	2,320	
		4	29	1·00	—	40·00	·73	·19	—				—	—	3,094	
		5	31	1·00	—	50·00	·90	·17	—	Perfect throughout (see Remarks).		Perfect throughout (see Remarks).	—	—	3,867	=½ Breaking strain.
		6	33	1·00	—	60·00	101·05	·15	—				—	—	4,641	
						67·00	·18	—	—				—	—	5,187	=¼ Breaking strain.
		7	35	1·00	—	70·00	·23	·18	—				—	—	5,414	
		8	37	1·00	—	80·00	·45	·22	—				—	—	6,188	
		9	39	·50	—	85·00	·59	·14	—				—	—	6,575	
		10	41	·50	—	90·00	·77	·18	—				—	—	6,961	
		11	43	·50	—	95·00	·93	·16	—				—	—	7,348	
		12	45	·50	—	100·00	—	—	—				—	—	7,735	Five strands broken, four welds. To be tested again electrically.
23 July	No. 9*. (Steel and Hemp.) Outside diameter ·376 inch. Six welds in 100 inches. One gutta percha joint. 4th Sample tested.	1	5 15	1·00	—	10·00	100·20	·00	—	70·75	100	1	23·47	13·11	773	
		2	17	1·00	—	20·00	·23	·23	—				—	—	1,547	
		3	19	1·00	—	30·00	·41	·18	—				—	—	2,320	
		4	21	1·00	—	40·00	·57	·16	—				—	—	3,094	
		5	23	1·00	—	50·00	·73	·16	—				—	—	3,867	
						51·25	·75	—	—				—	—	3,964	=¼ Breaking strain.
		6	25	1·00	—	60·00	·89	·16	—				—	—	4,641	
						63·38	101·01	—	—				—	—	5,286	=½ Breaking strain.
		7	27	1·00	—	70·00	·03	·16	—				—	—	5,414	
		8	29	1·00	—	80·00	·24	·19	—				—	—	6,188	
		9	31	·50	—	85·00	·36	·12	—	Perfect throughout.		Perfect throughout.	—	—	6,575	
		10	33	·50	—	90·00	·50	·14	—				—	—	6,961	
		11	35	·50	—	95·00	·65	·15	—				—	—	7,348	
		12	37	·25	—	97·50	·75	·10	—				—	—	7,542	
		13	39	·25	—	100·00	·87	·12	—				—	—	7,735	
		14	41	·25	—	102·50	—	—	—				—	—	7,928	Broke very slowly one strand at the time, three strands broke all welds.
23 July	No. 9*. (Steel and Hemp.) Outside diameter ·375 inch. Six welds in 100 inches and one gutta percha joint. 5th Sample tested.	1	11 18	1·00	—	10·00	100·00	·00	—				23·47	13·11	773	
		2	20	1·00	—	20·00	·21	·21	—				—	—	1,547	
		3	22	1·00	—	30·00	·33	·12	—				—	—	2,320	
		4	24	1·00	—	40·00	·52	·17	—	Tested in water, but not electrically. Raining at the time. The core to be kept to be tested under water when stripped.			—	—	3,094	
						43·75	·56	—	—				—	—	3,384	=¼ Breaking strain.
		5	26	1·00	—	50·00	·69	·13	—				—	—	3,867	
						58·83	·77	—	—				—	—	4,549	=½ Breaking strain.
		6	28	1·00	—	60·00	·80	·15	—				—	—	4,641	
		7	30	1·00	—	70·00	·93	·16	—				—	—	5,414	
		8	32	1·00	—	80·00	101·04	·08	—				—	—	6,188	Two strands broke.
		9	34	·25	—	82·50	·20	·16	—				—	—	6,381	
		10	36	·25	—	85·00	·32	·12	—				—	—	6,575	
		11	38	·25	—	87·50	—	—	—				—	—	6,768	One strand broke; broke very slowly. Three strands welded.
25 July	No. 9*. (Steel and Hemp.) Outside diameter ·375 inch. Without welds or gutta percha joints. 6th Sample tested.	1	11 29	1·00	—	10·00	100·00	·00	—	62·5	108	1	23·47	13·11	773	
		2	32	1·00	—	20·00	·10	·10	—				—	—	1,547	
		3	34	1·00	—	30·00	·20	·10	—				—	—	2,320	
		4	36	1·00	—	40·00	·30	·10	—				—	—	3,094	
		5	38	1·00	—	50·00	·40	·10	—				—	—	3,867	
		6	40	1·00	—	60·00	·50	·10	—				—	—	4,641	
		7	42	1·00	—	70·00	·62	·12	—				—	—	5,414	=¼ Breaking strain.
		8	44	·50	—	75·00	·67	·05	—				—	—	5,801	
		9	46	·50	—	80·00	·73	·06	—				—	—	6,188	
		10	48	·50	—	85·00	·76	·03	—				—	—	6,575	
		11	50	·50	—	90·00	·81	·05	—				—	—	6,961	
						95·33	·84	—	—				—	—	7,379	=½ Breaking strain.
		12	52	·50	—	95·00	·85	·04	—				—	—	7,348	
		13	54	·50	—	100·00	101·25	·40	—				—	—	7,735	
		14	56	·25	—	103·50	— ·31	·06	—				—	—	7,998	
		15	58	·25	—	105·00	·40	·09	—				—	—	8,121	
		16	12 0	·25	—	107·50	— ·46	·06	—				—	—	8,314	
		17	2	·25	—	110·00	— ·60	·13	—				—	—	8,508	
		18	4	·25	—	112·50	·70	·10	—				—	—	8,701	
		19	6	·25	—	115·00	— ·85	·15	—				—	—	8,894	

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description Specimen, and mple tested.	No.	Time.	Tests for Strength. Weight. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1850:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
July 26	No. 9a. (Steel and Hemp.) Outside diameter ·875 inch. Without welds or gutta percha joint. 6th Sample tested.	20	12 8	·25	—	117·50	108·98	·13	—	—	108	1	25·47	13·11	9,687	
		21	10	·25	—	120·00	102·13	·16	—	—	—	—	—	—	9,381	
		22	12	·25	—	122·00	·22	·15	—	—	—	—	—	—	9,474	
		23	14	·25	—	125·00	·47	·19	—	—	—	—	—	—	9,667	
		24	16	·25	—	127·50	·69	·13	—	—	—	—	—	—	9,861	
		25	18	·25	—	130·00	·77	·17	—	—	—	—	—	—	10,054	Continued 28th July.
		26	20	·25	—	132·50	·95	·18	—	63·5	Perfect.	Perfect.	—	—	10,248	
		27	22	·25	—	135·00	103·08	·13	—	—	—	—	—	—	10,441	
		28	24	·25	—	137·50	·25	·14	—	—	—	—	—	—	10,634	
		29	26	·25	—	140·00	—	—	—	—	—	—	—	—	10,828	Broke very slowly; seven strands broken. One strand broken in two places.
July 23	No. 9a. (Steel and Hemp.) Outside diameter ·875 inch. Without welds or gutta percha joint. 7th Sample tested.	1	1 45	1·00	—	10·00	100·00	·00	—	63·	—	—	25·47	13·11	773	
		2	47	1·00	—	20·00	·21	·21	—	—	—	—	—	—	1,547	
		3	49	1·00	—	30·00	·46	·25	—	—	—	—	—	—	2,320	
		4	51	1·00	—	40·00	·56	·04	—	—	—	—	—	—	3,094	
		5	53	1·00	—	50·00	·65	·15	—	—	—	—	—	—	3,867	
		6	55	1·00	—	60·00	·79	·14	—	—	—	—	—	—	4,641	
						65·00	·87								5,027	=½ Breaking strain.
		7	57	1·00	—	70·00	·95	·16	—	—	—	—	—	—	5,414	
		8	59	1·00	—	80·00	101·13	·18	—	—	—	—	—	—	6,188	
						86·67	·25								6,703	=⅔ Breaking strain.
		9	2 01	1·00	—	90·00	·35	·22	—	—	—	—	—	—	6,961	
		10	3	1·00	—	100·40	·04	·29	—	—	—	—	—	—	7,735	
		11	5	·50	—	105·00	·55	·21	—	—	—	—	—	—	8,122	
		12	7	·50	—	110·00	103·07	·22	—	—	—	—	—	—	8,508	
		13	9	·50	—	115·00	·35	·28	—	—	—	—	—	—	8,895	
		14	11	·50	—	120·00	·63	·28	—	—	—	—	—	—	9,281	
		15	13	·50	—	125·00	·88	·25	—	—	—	—	—	—	9,667	
		16	15	·25	—	127·50	103·05	·17	—	—	—	—	—	—	9,861	Scale touched the ground; permanent elongation 2·25 per cent.
		17	2 45	—	—	127·50	·25	—	—	—	—	—	—	—	9,861	Continued with same weight.
		18	47	·25	—	130·00	·46	·21	—	—	—	—	—	—	10,054	
		19	49	—	—	—	—	—	—	—	—	—	—	—	—	Broke fair; seven strands broken.
July 25	No. 9a. (Steel and Hemp.) Outside diameter ·875 inch. Without welds or gutta percha joint. 8th Sample tested.	1	4 23	1·00	—	10·00	100·00	·00	—	69·	100	1	25·47	13·11	773	
		2	27	1·00	—	20·00	·13	·13	—	—	—	—	—	—	1,547	
		3	29	1·00	—	30·00	·25	·12	—	—	—	—	—	—	2,320	
		4	31	1·00	—	40·00	·35	·10	—	—	—	—	—	—	3,094	
		5	33	1·00	—	50·00	·55	·16	—	—	—	—	—	—	3,867	
		6	35	1·00	—	60·00	·70	·14	—	—	—	—	—	—	4,641	
						66·25	·79								5,124	=⅓ Breaking strain.
		7	37	1·00	—	70·00	·84	·14	—	—	—	—	—	—	5,414	
		8	39	1·00	—	80·00	101·02	·18	—	—	—	—	—	—	6,188	
						88·28	·18								6,832	=⅔ Breaking strain.
		9	41	1·00	—	90·03	·22	·20	—	—	—	—	—	—	6,961	
		10	43	1·00	—	100·00	·46	·26	—	—	—	—	—	—	7,735	
		11	45	·50	—	105·00	·69	·21	—	—	—	—	—	—	8,121	
		12	47	·50	—	110·00	·93	·24	—	—	—	—	—	—	8,508	Slipped in clamps.
		13	49	·50	—	115·00	—	—	—	—	—	—	—	—	8,894	
		14	51	·50	—	120·00	—	—	—	—	—	—	—	—	9,281	
																Continued same day.
July 23	No. 9a. (Steel and Hemp.) Outside diameter ·875 inch. Without welds or gutta percha joint. 8th Sample tested.	1	5 18	1·00	—	10·00	100·00	·00	—	65·5	100	1	25·47	13·11	773	
		2	20	1·00	—	20·00	·07	·07	—	—	—	—	—	—	1,547	
		3	22	1·00	—	30·00	·12	·05	—	—	—	—	—	—	2,320	
		4	24	1·00	—	40·00	·18	·06	—	—	—	—	—	—	3,094	
		5	26	1·00	—	50·00	·29	·09	—	—	—	—	—	—	3,867	
		6	28	1·00	—	60·00	·39	·09	—	—	—	—	—	—	4,641	
		7	30	1·00	—	70·00	·50	·11	—	—	—	—	—	—	5,414	
		8	32	1·00	—	80·00	·62	·12	—	—	—	—	—	—	6,188	
		9	34	1·00	—	90·00	·74	·12	—	—	—	—	—	—	6,961	
		10	36	1·00	—	100·00	·87	·13	—	—	—	—	—	—	7,735	
		11	38	·50	—	105·00	·96	·09	—	—	—	—	—	—	8,121	
		12	40	·50	—	110·00	101·05	·10	—	—	—	—	—	—	8,508	
		13	42	·50	—	115·00	·21	·15	—	—	—	—	—	—	8,894	
		14	44	·50	—	120·00	·45	·24	—	—	—	—	—	—	9,281	
		15	46	·50	—	125·00	·76	·31	—	—	—	—	—	—	9,667	
		16	48	·50	—	130·00	102·15	·39	—	—	—	—	—	—	10,054	
		17	50	·25	—	132·50	—	—	—	—	—	—	—	—	10,248	Broke fair; seven strands broken.
July 26	No. 9a. (Steel and Hemp.) Outside diameter ·875 inch. Without welds or gutta percha joint. 9th Sample tested.	1	2 12	1·00	—	10·00	100·00	·00	—	67·	—	—	29·47	13·11	773	
		2	14	1·00	—	20·00	·22	·22	—	—	—	—	—	—	1,547	
		3	16	1·00	—	30·00	·38	·16	—	—	—	—	—	—	2,320	
		4	18	1·00	—	40·00	·52	·14	—	—	—	—	—	—	3,094	
		5	20	1·00	—	50·00	·67	·15	—	—	—	—	—	—	3,867	
		6	22	1·00	—	60·00	·71	·04	—	—	—	—	—	—	4,641	
						65·25	·80								5,124	=⅓ Breaking strain.
		7	24	1·00	—	70·00	·97	·26	—	—	—	—	—	—	5,414	
		8	26	1·00	—	80·00	101·14	·17	—	—	—	—	—	—	6,188	
						88·33	·30								6,832	=⅔ Breaking strain.
		9	28	1·00	—	90·00	·33	·19	—	—	—	—	—	—	6,961	
		10	30	1·00	—	100·00	·50	·26	—	—	—	—	—	—	7,735	
		11	32	·50	—	105·00	·81	·23	—	—	—	—	—	—	8,121	
		12	34	·50	—	110·00	102·08	·17	—	—	—	—	—	—	8,508	
		13	36	·50	—	115·00	·42	·34	—	—	—	—	—	—	8,894	
		14	38	·50	—	119·00	·70	·28	—	—	—	—	—	—	9,203	
		15	40	·25	—	122·50	·94	·24	—	—	—	—	—	—	9,474	
		16	42	·25	—	125·00	103·03	·15	—	—	—	—	—	—	9,667	
		17	44	·25	—	127·50	·23	·18	—	—	—	—	—	—	9,861	
		18	46	·25	—	130·00	·40	·15	—	—	—	—	—	—	10,054	
		19	48	·25	—	132·50	—	—	—	—	—	—	—	—	10,248	Broke fair; six strands broken.
July 26	No. 9a. (Steel and Hemp.) Outside diameter ·875 inch. Without welds or gutta percha joint. 10th Sample tested.	1	3 31	1·00	—	10·00	100·00	·00	—	67·	100	1	29·47	13·11	773	
		2	33	1·00	—	20·00	·20	·20	—	—	—	—	—	—	1,547	
		3	35	1·00	—	30·00	·36	·16	—	—	—	—	—	—	2,320	
		4	37	1·00	—	40·00	·51	·15	—	—	—	—	—	—	3,094	
		5	39	1·00	—	50·00	·67	·16	—	—	—	—	—	—	3,867	
		6	41	1·00	—	60·00	·81	·14	—	—	—	—	—	—	4,641	
		7	43	1·00	—	70·00	·92	·11	—	—	—	—	—	—	5,414	
		8	45	1·00	—	80·00	101·15	·23	—	—	—	—	—	—	6,188	
		9	47	1·00	—	90·00	·34	·19	—	—	—	—	—	—	6,961	
						93·33	·43								7,219	=⅓ Breaking strain.
		10	49	1·00	—	100·00	·62	·28	—	—	—	—	—	—	7,735	

App. No. 10.

APPENDIX No. 10.—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for outer covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°. 100	° 1	*Cwts.*	*Cwts.*	*Fms.*	
July 26	No. 9. (Steel and Hemp.) Outside diameter ·875 inches. Without weld or gutta percha joint. 10th Sample tested.	11	3 51	·50	—	105·00	101·85	·23	—	67·			29·47	13·11	6,731	
		12	53	·50	—	110·00	102·08	·23	—				—	—	8,508	
		13	55	·50	—	115·00	·46	·32	—				—	—	8,894	
		14	57	·50	—	120·00	·63	·22	—				—	—	8,281	
		15	59	·00	—	125·00	·96	·33	—				—	—	9,067	
		16	4 1	·50	—	130·00	103·32	·27	—				—	—	10,054	
		17	3	·25	—	132·50	·41	·19	—				—	—	10,248	
		18	5	·25	—	135·00	·59	·18	—				—	—	10,441	
		19	7	·25	—	137·50	·66	·07	—				—	—	10,534	
		20	9	·25	—	140·00	—	—	—				—	—	10,828	Splendid break. Eight strands broken. Core remained unbroken.
July 26	No. 9. (Steel and Hemp.) Outside diameter ·875 inches. Without weld or gutta percha joint. 11th Sample tested.	1	4 35	1·00	—	10·00	100·00	·00	—	58·			25·47	13·11	773	
		2	38	1·00	—	20·00	·24	·24	—				—	—	1,547	
		3	40	1·00	—	30·00	·41	·17	—				—	—	2,320	
		4	42	1·00	—	40·00	·56	·15	—				—	—	3,094	
		5	44	1·00	—	50·00	·71	·15	—				—	—	3,867	
		6	46	1·00	—	60·00	·86	·15	—				—	—	4,641	
		7	48	1·00	—	70·00	101·04	·18	—				—	—	5,414	= ⅛ Breaking strain.
		8	50	1·00	—	80·00	·18	·16	—				—	—	6,188	
		9	5 00	1·00	—	90·00	·44	·26	—				—	—	6,961	
						93·34	·56	—	—				—	—	7,219	= ⅜ Breaking strain.
		10	2	1·00	—	100·00	·69	·25	—				—	—	7,735	
		11	4	1·00	—	110·00	102·16	·47	—				—	—	8,508	
		12	6	1·00	—	120·00	·74	·58	—				—	—	9,281	
		13	8	·50	—	125·00	103·03	·29	—				—	—	9,667	
		14	10	·50	—	130·00	·35	·32	—				—	—	10,054	
		15	12	·25	—	132·50	·55	·20	—				—	—	10,248	
		16	14	·25	—	135·00	·72	·17	—				—	—	10,441	
		17	16	·25	—	137·50	·91	·19	—				—	—	10,634	
		18	18	·25	—	140·00	—	—	—				—	—	10,828	Broke fair. Eight strands broken.
June 21	No. 10. (Hemp and Iron.) Glass, Elliott & Co. Outside diameter inches.	1	4 28	1·00	—	10·00	20·00	—	—				—	—	—	Drew in clamps in consequence of quantity of tar on sample.
		2	—	1·00	—	20·00	—	—	—				—	—	—	Continued June 23. Drew in clamp. Continued June 24.
June 23		1	5 20	1·00	—	10·00	10·00	—	—				—	—	—	Specimen only 10 inches between clamps.
		2	—	1·00	—	20·00	—	—	—				—	—	—	
June 24		1	4 29	1·00	—	10·00	10·00	—	—				—	—	—	
		2	—	1·00	—	20·00	·01	·01	—				—	—	—	
		3	—	1·00	—	30·00	·03	·02	—				—	—	—	
		4	—	1·00	—	40·00	·06	·02	—				—	—	—	Coupling chain broke at welding.
		5	—	·50	—	45·00	·06	·01	—				—	—	—	Broke in the clamp. Specimen too much tar; not a fair break.
		6	—	·50	—	50·00	—	—	—				—	—	—	Per-centage of elongation, 40·00 cwt. = ·80 per cent. Elongation doubtful, the sample being three times clamped, and subjected to strain. Specimen too short for testing.
June 22	No. 11. (Manilla Hemp and Iron.) Outside diameter 1·125 inches. Sir C. Bright's specification. 1st Sample tested.	1	2 17	1·00	—	10·00	100·00	·00	—				44·74	23·40	433	
		2	—	1·00	—	20·00	·12	·12	—				—	—	866	
		3	—	1·00	—	30·00	·28	·16	—				—	—	1,299	
		4	—	1·00	—	40·00	·37	·9	—				—	—	1,732	
		5	—	1·00	—	50·00	·48	·11	—				—	—	2,164	
		6	—	1·00	—	60·00	·59	·11	—				—	—	2,597	
						65·25	·65	—	—				—	—	2,760	= ⅛ Breaking strain.
		7	—	·50	—	70·00	·66	·7	—				—	—	2,814	
		8	—	·50	—	75·00	·72	·6	—				—	—	3,030	
		9	—	·50	—	75·00	·78	·6	—				—	—	3,247	
		10	—	·50	—	80·00	·84	·6	—				—	—	3,463	
		11	—	·50	—	85·00	·90	·6	—				—	—	3,679	= ¼ Breaking strain.
		12	—	·50	—	90·00	·96	·6	—				—	—	3,896	
		13	—	·50	—	95·00	101·02	·6	—				—	—	4,112	
		14	—	·50	—	100·00	·10	·8	—				—	—	4,329	
		15	—	·25	—	102·50	·16	·6	—				—	—	4,437	
		16	—	·25	—	105·00	·23	·7	—		Tested in Air.		—	—	4,545	
		17	—	·25	—	107·50	·32	·9	—				—	—	4,653	
		18	—	·25	—	110·00	·39	·7	—				—	—	4,762	
		19	—	·25	—	112·50	·48	·9	—				—	—	4,870	
		20	—	·25	—	115·00	·58	·10	—				—	—	4,978	
		21	—	·25	—	117·50	·71	·13	—				—	—	5,086	
		22	—	·25	—	120·00	·90	·19	—				—	—	5,195	Tar dropping from outside cable.
		23	—	·25	—	122·50	102·16	·26	—				—	—	5,303	
		24	—	·25	—	125·00	·36	·20	—				—	—	5,411	Cable broke slowly, six strands in all.
		25	2 22	·25	—	127·50	—	—	—				—	—	5,519	
July 13	No. 11. (Manilla Hemp and Iron.) Outside diameter 1·125 inches. 2d Sample tested.	1	4 29	1·00	—	10·00	100·00	·00	—				44·74	23·40	443	
		2	31	1·00	—	20·00	·28	·28	—				—	—	866	Clamped in lead. Sample tested in air, the outside diameter being too great for trough. Tar oozing out. = ¼ Breaking strain.
		3	33	1·00	—	30·00	·42	·14	—				—	—	1,299	
		4	35	1·00	—	40·00	·50	·08	—				—	—	1,732	
		5	37	1·00	—	50·00	·62	·12	—				—	—	2,164	
		6	39	1·00	—	60·00	·74	·12	—				—	—	2,597	
						63·75	·77	—	—				—	—	2,760	
		7	41	1·00	—	70·00	·84	·10	—				—	—	3,030	
		8	43	1·00	—	80·00	·95	·11	—				—	—	3,463	
						85·00	101·03	—	—				—	—	3,679	= ¾ Breaking strain.
		9	45	1·00	—	90·00	·10	·15	—				—	—	3,896	
		10	47	1·00	—	100·00	·23	·13	—				—	—	4,329	
		11	49	·25	—	102·50	·13	·07	—				—	—	4,437	
		12	51	·25	—	105·00	·35	·05	—				—	—	4,545	Tar dripping all along the cable.
		13	53	·25	—	107·50	·42	·07	—				—	—	4,653	
		14	55	·25	—	110·00	·51	·09	—				—	—	4,762	
		15	57	·25	—	112·50	·61	·10	—				—	—	4,870	
		16	59	·25	—	115·00	·73	·12	—				—	—	4,978	
		17	5 1	·25	—	117·50	·82	·09	—				—	—	5,086	
		18	3	·25	—	120·00	102·00	·18	—				—	—	5,195	
		19	5	·25	—	122·50	·18	·18	—				—	—	5,303	
		20	7	·25	—	125·00	·33	·15	—				—	—	5,411	
		21	9	·25	—	127·50	—	—	—				—	—	5,519	Hook broke. Continued July 14.

APPENDIX No. 10—*continued.* APP. No. 10.

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
July 14	No. 11. (Manilla Hemp and Iron.) Outside diameter = 1·125 inches. 2d Sample tested.	1	2 46	1·00	—	10·00	100·00	·00	—				44·74	23·40	445	
		2	47	2·00	—	30·00		·13	—				—	—	1,299	
		3	49	2·00	—	50·00		·26	—				—	—	2,184	
						63·75		·32							2,760	=½ Breaking strain
		4	51	2·00	—	70·00		·36	—				—	—	3,030	
						85·00		·47							3,679	=¾ Breaking strain.
		5	56	2·00	—	90·00		·50	—				—	—	3,896	
		6	59	2·00	—	110·00		·80	—				—	—	4,762	
		7	3 1	1·00	—	120·00		·93	—				—	—	5,195	
		8	2	·25	—	122·50		·86	—				—	—	5,303	
		9	3	·25	—	125·00		·73	—				—	—	5,411	
		10	4	·25	—	127·50		·76	—				—	—	5,519	
		11	5	—	—	127·50		·92	—				—	—	—	Broke immediately after elongation was taken; two strands broken.
June 21	No. 12. (Red Sea Cable.) Outside diameter = ·562 inches. 1st Sample tested.	1	5 58	1·00	—	10·00	75·00	—	—				21·00	17·30	·586	
		2	—	1·00	—	20·00		·10	—				—	—	1,172	
		3	—	1·00	—	30·00		·17	—				—	—	1,757	Drew in clamp.
				1·00	—	10·00	50·00	—	—				—	—	·586	
		2		1·00	—	20·00		·05	—				—	—	1,171	
		3		1·00	—	30·00		·03	·03				—	—	1,757	
						32·50		·10							1,903	=¼ Breaking strain.
		4		1·00	—	40·00		·24	·08				—	—	2,342	
						42·50		·15	—				—	—	2,488	=½ Breaking strain.
		5		·50	—	45·00		·10	·02				—	—	2,635	Tar squeezed out.
		6		·50	—	50·00		·20	·04				—	—	2,928	
		7		·50	—	55·00		·23	·03				—	—	3,220	
		8		·50	—	60·00		·26	·05				—	—	3,513	
		9		·25	—	62·50		·30	·02				—	—	3,659	
		10		·25	—	65·00	—	—	—				—	—	3,806	Broke right across near clamp; core and all. Core not twisted. Elongation doubtful, sample being twice clamped. Estimated per-centage of elongation:— ½ Breaking strain = ·27 ¾ Breaking strain = ·42 Breaking strain = ·67
June 30	No. 12. (Red Sea Cable.) Outside diameter = ·562 inches. 2d Sample tested.	1	Weights put on at intervals of about one minute.	1·00	—	10·00	100·00	·00	—				21·00	17·30	·586	
		2		1·00	—	20·00		·06	—				—	—	1,171	
		3		1·00	—	30·00		·15	·09				—	—	1,757	
		4		1·00	—	40·00		·26	·10				—	—	2,342	
						41·25		·27							2,410	=½ Breaking strain.
		5		1·00	—	50·00		·40	·15				—	—	2,928	Tar oozing out.
		6		·50	—	55·00	—	—	—				—	—	3,220	Drew in clamps. (Continued 4th July.)
July 4		1		1·00	—	10·00	90·00	·00	—				21·00	17·30	·586	
		2		1·00	—	20·00		·08	—				—	—	1,171	
		3		1·00	—	30·00		·14	·06				—	—	1,757	
		4		1·00	—	40·00		·20	·06				—	—	2,342	
						41·25		·21		Tested in Air.					2,410	=½ Breaking strain.
		5		1·00	—	50·00		·27	·07				—	—	2,928	
		6		·50	—	55·00		·31	·04				—	—	3,220	=¾ Breaking strain.
		7		·50	—	60·00		·36	·04				—	—	3,513	
		8		·25	—	62·50		·37	·02				—	—	3,659	
		9		·25	—	65·00		·40	·03				—	—	3,806	
		10		·25	—	67·50		·42	·02				—	—	3,952	
		11		·25	—	70·00		·46	·04				—	—	4,099	Estimated per-centage of elongation:— ¼ Breaking strain = ·27 ½ Breaking strain = ·35 Breaking strain = ·81
		12		·25	—	72·50		·50	·04				—	—	4,245	
		13		·25	—	75·00		·54	·04				—	—	4,392	
		14		·25	—	77·50		·63	·09				—	—	4,518	
		15		·25	—	80·00		·76	·13				—	—	4,674	
		16		·25	—	82·50	—	—	—				—	—	4,821	Broke close to clamps (not fair). Elongation doubtful; sample clamped twice.
June 30	No. 12. (Red Sea Cable.) Outside diameter = ·562 inches. 3d Sample tested.	1	Weights put on at intervals of about one minute.	1·00	—	10·00	100·00	·00	—				21·00	17·30	·586	
		2		1·00	—	20·00		·05	·05				—	—	1,171	
		3		1·00	—	30·00		·14	·09				—	—	1,757	
						32·75		·17							1,909	=¼ Breaking strain.
		4		1·00	—	40·00		·18	·04				—	—	2,342	
		5		1·00	—	50·00		·26	·08				—	—	2,928	Tar oozing out.
						51·87		·29							3,026	=½ Breaking strain.
		6		·50	—	55·00		·30	·04				—	—	3,220	
		7		·50	—	60·00		·35	·05				—	—	3,513	
		8		·25	—	62·50		·38	·03				—	—	3,659	
		9		·25	—	65·00		·41	·03				—	—	3,805	
		10		·25	—	67·50		·44	·03				—	—	3,952	
		11		·25	—	70·00		·47	·03				—	—	4,099	
		12		·25	—	72·50		·51	·04				—	—	4,245	
		13		·25	—	75·00		·56	·05				—	—	4,392	
		14		·25	—	77·50	—	—	—				—	—	4,538	Fairly broken; all strands about two inches from clamp.
June 30	No. 12. (Red Sea Cable.) Outside diameter = ·562 inches. 4th Sample tested.	1	Weights put on at intervals of about one minute.	1·00	—	10·00	98·00	·00	—				21·00	17·30	·586	
		2		1·00	—	20·00		·06	—				—	—	1,171	
		3		1·00	—	30·00		·14	·08				—	—	1,757	
		4		1·00	—	40·00		·25	·11				—	—	2,342	Drew in clamps.
						41·87		·27							2,410	=¼ Breaking strain.
		5		1·00	—	50·00		·36	·11				—	—	2,928	Still slipping in clamps.
		6		·50	—	55·00	—	—	—				—	—	3,220	Slipped. (Continued 4th July.)
July 4		1	Weights put on at intervals of one minute apart.	1·00	—	10·00	70·00	·00	—				21·00	17·30	·586	
		2		1·00	—	20·00		·05	·05				—	—	1,171	
		3		1·00	—	30·00		·19	·05				—	—	1,757	
		4		1·00	—	40·00		·17	·07				—	—	2,342	
						41·25		·18							2,410	=¼ Breaking strain.
		5		1·00	—	50·00		·23	·05				—	—	2,928	
						55·00		·24							3,220	—½ Breaking strain.
		6		1·00	—	60·00		·32	·08				—	—	3,513	

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables.—*continued.*

Date of Experiment.	Description of Specimen and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Conductivity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
June 30	No. 12. (Red Sea Cable.) Outside diameter = ·562 inches. 4th sample tested.	7	Weights put on at one minute intervals.	·50	—	65·00	·36	·04	—				21·00	17·30	3,806	
		8		·50	—	70·00	·42	·06	—				—	—	4,099	
		9		·25	—	72·50	·45	·03	—				—	—	4,245	
		10		·25	—	75·00	·48	·03	—				—	—	4,392	
		11		·25	—	77·50	·53	·05	—				—	—	4,538	
		12		·25	—	80·00	·63	·10	—				—	—	4,684	
		13		·25	—	82·50	—	—	—				—	—	4,821	One strand parted first (broke midway between clamps; twelve strands broken). Elongation uncertain, sample being twice clamped. Estimated per-centage of elongation of cable:— ½ Breaking strain = ·20 ¾ Breaking strain = ·33 Breaking strain = ·90
4 July	No. 12. (Red Sea Cable.) Outside diameter = ·562 inches. 5th Sample tested.	1	Weights added at intervals of one minute.	1·00	—	10·00	100·00	·00	—				21·03	17·30	586	
		2		1·00	—	20·00	·10	·10	—				—	—	1,171	
		3		1·00	—	30·00	·23	·13	—				—	—	1,757	Specimen clamped in lead.
		4		1·00	—	40·00	·32	·09	—				—	—	2,342	
						42·50	·34	—	—				—	—	2,484	=¼ Breaking strain.
		5		·50	—	45·00	·36	·04	—				—	—	2,635	
		6		·50	—	50·00	·43	·07	—				—	—	2,928	
		7		·50	—	55·00	·48	·05	—				—	—	3,220	
						56·68	·49	—	—				—	—	3,319	=⅓ Breaking strain.
		8		·50	—	60·00	·52	·04	—				—	—	3,513	
		9		·25	—	62·50	·55	·03	—				—	—	3,659	
		10		·25	—	65·00	·58	·03	—				—	—	3,806	
		11		·25	—	67·50	·60	·02	—				—	—	3,952	
		12		·25	—	70·00	·65	·05	—				—	—	4,099	
		13		·25	—	72·50	·68	·03	—				—	—	4,245	
		14		·25	—	75·00	·76	·08	—				—	—	4,392	
		15		·25	—	77·50	·83	·07	—				—	—	4,538	
		16		·25	—	80·00	·85	·02	—				—	—	4,684	
		17		·25	—	82·50	101·10)	·21	—	Tested in Air.			—	—	4,821	
		18		·25	—	85·00	—	—	—				—	—	4,968	Ten strands broken (about one foot from clamps).
July 11	No. 13. (Red Sea Cable.) Outside diameter = ·562 inches. 6th Sample tested.	1	Weights put on at intervals of one minute.	1·00	—	10·00	99·00	·00	—				—	—	586	
		2		1·00	—	20·00	·06	·06	—				—	—	1 171	
		3		1·00	—	30·00	·10	·04	—				—	—	1,757	Tar beginning to ooze out.
		4		1·00	—	40·00	·17	·07	—				—	—	2,342	=⅓ Breaking strain.
		5		·50	—	45·00	·19	·02	—				—	—	2,635	
		6		·50	—	50·00	·21	·02	—				—	—	2,928	
						53·23	·23	—	—				—	—	3,116	=⅓ Breaking strain.
		7		·50	—	55·00	·24	·03	—				—	—	3,220	
		8		·25	—	57·50	·25	·01	—				—	—	3,366	
		9		·25	—	60·00	·26	·01	—				—	—	3,513	
		10		—	·50	59·00	·24	—	·02				—	—	3,454	
		11		—	·50	60·00	·22	—	·02				—	—	3,928	
		12		—	·50	45·00	·21	—	·01				—	—	2,635	
		13		—	·50	40·00	·20	—	·01				—	—	2,342	
		14		1·00	—	39·00	·18	—	·02				—	—	1,757	
		15		1·00	—	20·00	·14	—	·01				—	—	1,171	
		16		1·00	—	10·00	·11	—	·03				—	—	586	
		17		1·00	—	20·00	·15	·04	—				—	—	1,171	
		18		1·00	—	30·00	·18	·03	—				—	—	1,757	
		19		1·00	—	40·00	·20	·02	—				—	—	2,342	
		20		·50	—	45·00	·21	·01	—				—	—	2,635	
		21		·50	—	50·00	·23	·01	—				—	—	2,928	
		22		·50	—	55·00	·23	·01	—				—	—	3,220	
		23		·25	—	57·50	·25	·02	—				—	—	3,513	
		24		·25	—	62·50	·27	·02	—				—	—	3,659	
		25		·25	—	65·00	·28	·01	—				—	—	3,805	
		26		·25	—	67·50	·29	·01	—				—	—	3,952	
		27		·25	—	70·00	·30	·01	—				—	—	4,099	
		28		·25	—	72·50	·31	·01	—				—	—	4,245	
		29		·25	—	75·00	·32	·01	—				—	—	4,392	
		30		·25	—	77·50	·33	·01	—				—	—	4,538	
		31		·25	—	80·00	—	—	—				—	—	4,674	Broke close to clamp a few seconds after the weight was put on. All strands and core broken; fair break. Estimated per-centage of elongation:— ½ Breaking strain = ·28 ¾ Breaking strain = ·38 Breaking strain = ·55
July 12	No. 13. (Red Sea Cable.) Outside diameter = ·562 inches. 7th Sample tested.	1		1·00	—	10·00	100·00	·00	—	70·5			21·00	17·30	586	Tested in salt water.
		2		1·00	—	20·00	·04	·04	—				—	—	1,171	
		3		1·00	—	30·00	·10	·06	—				—	—	1,757	
		4		1·00	—	40·00	·16	·06	—				—	—	2,342	
						43·75	·19	—	—				—	—	2,556	=¼ Breaking strain.
		5		1·00	—	50·00	·24	·08	—				—	—	2,928	
		6		·50	—	55·00	·27	·03	—				—	—	3,220	
						58·33	·31	—	—				—	—	3,408	=⅓ Breaking strain.
		7		·50	—	60·00	·35	·04	—				—	—	3,513	
		8		·50	—	55·00	·33	—	·01				—	—	3,805	Commenced taking off weights.
		9		·50	—	50·00	·33	—	·02				—	—	2,928	
		10		1·00	—	40·00	·29	—	·04				—	—	2,342	
		11		1·00	—	30·00	·27	—	·06				—	—	1,757	
		12		1·00	—	20·00	·17	—	·06				—	—	1,171	
		13		1·00	—	10·00	·13	—	·04				—	—	586	
		14		1·00	—	20·00	·16	·08	—				—	—	1,171	Recommenced putting on weights.
		15		1·00	—	30·00	·22	·06	—				—	—	1,757	
		16		1·00	—	40·00	·28	·06	—				—	—	2,342	
		17		1·00	—	50·00	·32	·04	—				—	—	2,928	
		18		·50	—	55·00	·35	·03	—				—	—	3,220	
		19		·50	—	60·00	·43	·08	—				—	—	3,513	
		20		·25	—	62·50	·45	·02	—				—	—	3,659	
		21		·25	—	67·50	·49	·04	—				—	—	3,952	

Perfect throughout with 108 elements.

Continuity perfect with 109 element.

APPENDIX No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
July 12	No. 12. (Red Sea Cable.) Outside diameter = ·592 inches. 7th Sample tested.	22	—	·25	—	70·00	100·54	·05	—	70·5			21·00	17·30	4,999	
		23	—	·25	—	72·50	·58	·02	—	—			—	—	4,345	
		24	—	·25	—	75·00	·63	·07	—	—			—	—	4,392	
		25	—	·25	—	77·50	·70	·07	—	—			—	—	4,538	
		26	—	·25	—	80·00	·75	·25	—	—			—	—	4,674	Tested in salt water.
		27	—	·25	—	82·50	·85	·10	—	—			—	—	4,820	
		28	—	·25	—	85·00	101·00	15	—	—			—	—	4,966	
		29	—	·25	—	87·50	—	—	—	—			—	—	5,112	Broke fairly, after bearing the weight a few seconds. Fifteen iron strands and core broken.
June 23	Sample No. 13. (Atlantic Specimen.)	1	4 10	1·00	—	30·00	50·00	·00	—				21·70	16·30	632	
		2	—	1·00	—	33·00	·03	·03	—				—	—	1,245	
		3	—	1·00	—	36·00	·07	·04	—				—	—	1,867	
		4	—	1·00	—	40·00	·12	·05	—				—	—	2,489	= ⅓ Breaking strain.
		5	—	·50	—	45·00	·14	·02	—				—	—	2,801	
		6	—	·50	—	50·00	·17	·03	—				—	—	3,112	
		7	—	·25	—	52·50	·18	·01	—				—	—	3,267	
						53·34	·19	—	—				—	—	3,315	= ⅔ Breaking strain.
		8	—	·25	—	55·00	·20	·02	—				—	—	3,423	
		9	—	·25	—	57·50	·21	·01	—				—	—	3,579	
		10	—	·25	—	60·00	·23	·02	—				—	—	3,734	
		11	—	·25	—	62·50	·25	·02	—				—	—	3,890	
		12	—	·25	—	65·00	·27	·02	—				—	—	4,045	
		13	—	·25	—	67·50	·29	·02	—				—	—	4,201	
		14	—	·25	—	70·00	·30	·01	—				—	—	4,357	
		15	—	·25	—	72·50	·33	·02	—				—	—	4,512	
		16	—	·25	—	75·00	·34	·01	—				—	—	4,668	
		17	—	·25	—	77·50	·40	·04	—				—	—	4,823	
		18	—	·25	—	80·00	—	—	—				—	—	4,979	Fair break (fourteen strands). Core very much twisted. Estimated per-centage of elongation :— = ⅔ Breaking strain = ·24 = ⅘ Breaking strain = ·38 Breaking strain = ·80
June 23	Shore End Specimen. No. 14. (Gibraltar Specimen, No. 3.) Outside diameter = ·77 inch. 1st Sample tested.	1	5 50	1.00	—	10·00	100·00	·00	—				42·70	32·73	309	
		2	—	1.00	—	28·00	·05	·05	—				—	—	619	
		3	—	1.00	—	30·00	·07	·03	—				—	—	928	
		4	—	1.00	—	40·00	·10	·03	—				—	—	1,238	
		5	—	1.00	—	50·00	·15	·05	—				—	—	1,547	
		6	—	1.00	—	60·00	·20	·05	—				—	—	1,857	Clamp slipped.
June 23	" " "	1	—	1·00	—	10·00		1·00	—				—	—	309	Continued June 23d.
		2	—	2·00	—	30·00	·15	·15	—				—	—	928	
		3	—	2·00	—	50·00	·26	·11	—				—	—	1,547	
		4	—	1·00	—	60·00	·31	·05	—				—	—	1,857	
		5	—	1·00	—	70·00	·35	·04	—				—	—	2,166	
						71·25	·36	—	—				—	—	2,212	= ⅓ Breaking strain.
		6	—	1·00	—	80·00	·41	·06	—				—	—	2,476	
		7	—	1·00	—	90·00	·46	·05	—				—	—	2,785	
		8	—	·50	—	95·00	·48	·02	—				—	—	2,940	= ⅔ Breaking strain.
		9	—	·50	—	100·00	·50	·02	—				—	—	3,095	
		10	—	·50	—	105·00	·53	·03	—				—	—	3,250	
		11	—	·50	—	110·00	·55	·02	—	Tested in Air.			—	—	3,404	
		12	—	·50	—	115·00	·58	·03	—				—	—	3,559	
		13	—	·50	—	120·00	·62	·04	—				—	—	3,714	
		14	—	·25	—	122·50	·65	·03	—				—	—	3,791	
		15	—	·25	—	125·00	·67	·02	—				—	—	3,868	
		16	—	·25	—	127·50	·68	·01	—				—	—	3,945	
		17	—	·25	—	130·00	·71	·03	—				—	—	4,023	
		18	—	·25	—	132·50	·73	·02	—				—	—	4,101	
		19	6 3	·50	—	137·50	·75	·02	—				—	—	4,256	
		20	—	·50	—	142·50	·81	·06	—				—	—	4,411	
		21	—	·50	—	147·50	·86	·05	—				—	—	5,565	Not broken fairly. Broke at clamp. One wire broke some time before the rest. Ten wires broken in all.
July 1	No. 14. (Gibraltar Shore End, No. 3.) Outside diameter = ·77 inches. 2d Sample tested.	1		1·00	—	10·00	100·00	·00	—				42·70	32·73	309	
		2		2·00	—	30·00	·15	·15	—				—	—	928	
		3		2·00	—	50·00	·26	·11	—				—	—	1,547	
		4		2·00	—	70·00	·35	·09	—				—	—	2,166	
		5		1·00	—	80·00	·41	·06	—				—	—	2,476	
		6		1·00	—	90·00	·46	·05	—				—	—	2,785	
		7		1·00	—	100·00	·51	·05	—				—	—	3,095	
		8		1·00	—	110·00	·58	·07	—				—	—	3,404	Drew in clamp.
"	" " " "	1		1·00	—	10·00		·00	—				—	—	309	Continued July 1st.
		2		2·00	—	30·00	·10	·10	—				—	—	928	
		3		2·00	—	50·00	·17	·07	—				—	—	1,547	
		4		2·00	—	70·00	·23	·06	—				—	—	2,166	
		5		2·00	—	90·00	·30	·07	—				—	—	2,785	
		6		2·00	—	110·00	·38	·08	—				—	—	3,404	
		7		1·00	—	120·00	·43	·05	—				—	—	3,714	
		8		1·00	—	130·00	—	—	—				—	—	4,023	Hook broke.
		9		1·00	—	140·00	·12	·12	—				—	—	4,332	
		10		1·00	—	150·00	—	—	—				—	—	4,642	Broke couplings; scale filled for the third time. Large hook attached to scale beam broke. Continued July 4th.
July 4	No. 14. (Gibraltar Shore End, No. 3.) Outside diameter = ·770 inches. 2d Sample tested. (Continued from 1st July.)	1	Weights put on at intervals of one minute.	1·00	—	10·00	100·00	·00	—				42·70	32·73	309	
		2		2·00	—	30·00	·08	·08	—				—	—	928	
		3		2·00	—	50·00	·13	·05	—				—	—	1,547	
		4		2·00	—	70·00	·20	·07	—				—	—	2,166	
		5		2·00	—	90·00	·30	·15	—				—	—	2,785	
		6		2·00	—	110·00	—	—	—				—	—	3,404	Broke shackle, and large hook on beam again fractured. Continued July 9th.
July 9	No. 14. (Shore End, No. 3.) Outside diameter = ·77 inches. 2d Sample tested.	1		1·00	—	10·00	100·00	·00	—	70·	—	—	42·70	32·73	309	Tested in water, but not electrically.
		2		2·00	—	30·00	·06	·06	—	—	—	—	—	—	928	
		3		2·00	—	50·00	·11	·05	—	—	—	—	—	—	1,547	
		4		2·00	—	70·00	·16	·05	—	—	—	—	—	—	2,166	
		5		2·00	—	90·00	·24	·08	—	—	—	—	—	—	2,785	
		6		2·00	—	110·00	·30	·06	—	—	—	—	—	—	3,404	
		7		1·00	—	120·00	·33	·03	—	—	—	—	—	—	3,714	
		8		1·00	—	130·00	·36	·03	—	—	—	—	—	—	4,023	

A 1 P. No. 10.

APPENDIX No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen and Sample tested.	No.	Time	Tests for Strength. Weights. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
July	No. 14. (Shore End No. 3.)* Outside diameter = ·77 inches. 2d Sample tested. * Now Deep Sea, No. 1.	9		1·00	—	140·00	100·46	·54	—	70	—	—	42·70	32·73	4,332	
		10		—	1·00	130·00	·38	—	·62	—	—	—	—	—	4,093	Commenced taking off weights.
		11	—	—	1·00	120·00	·36	—	·62	—	—	—	—	—	3,774	
		12		—	1·00	110·06	·33	—	·63	—	—	—	—	—	3,464	
		13		—	2·00	90·00	·39	—	·33	—	—	—	—	—	2,785	
		14		—	2·00	70·09	·34	—	·33	—	—	—	—	—	2,166	
		15		—	2·00	50·09	·17	—	·97	—	—	—	—	—	1,547	
		16		—	2·00	30·00	·12	—	·05	—	—	—	—	—	928	
		17		—	2·00	10·00	·06	—	·06	—	—	—	—	—	309	
		18		2·00	—	30·00	·12	·06	—	—	—	—	—	—	928	Recommenced putting on weights.
		19		2·00	—	50·00	·16	·04	—	—	—	—	—	—	1,547	
		20		2·00	—	70·00	·22	·06	—	—	—	—	—	—	2,166	
		21		2·00	—	90·00	·27	·05	—	—	—	—	—	—	2,785	
		22		2·00	—	110·00	·33	·06	—	—	—	—	—	—	3,404	
		23		1·00	—	120·00	·36	·03	—	—	—	—	—	—	3,714	
		24		1·00	—	130·00	·39	·03	—	—	—	—	—	—	4,023	
		25		1·00	—	140·00	·42	·03	—	—	—	—	—	—	4,332	
		26		·50	—	145·00	·44	·02	—	—	—	—	—	—	4,487	Tested in water, but not electrically.
		27		·50	—	150·00	·49	·05	—	—	—	—	—	—	4,642	
		28		·25	—	152·50	·54	·05	—	—	—	—	—	—	4,719	
		29		·25	—	155·00	·70	·16	—	—	—	—	—	—	4,797	
		30		·25	—	157·50	—	—	—	—	—	—	—	—	4,874	Broke in clamp, after bearing the weight a few seconds.
July 11		1		1·00	—	10·00	70·00	·00	—				42·70	32·73	309	Continued July 11th.
		2		2·00	—	30·00	·04	·04	—				—	—	928	
		3		2·00	—	50·00	·08	·04	—				—	—	1,547	
		4		2·00	—	70·00	·11	·03	—				—	—	2,166	
		5		2·00	—	78·75	·16	—	—				—	—	2,437	= ½ Breaking strain.
		6		2·00	—	90·00	·17	·06	—				—	—	2,785	
		7		1·00	—	105·00	·23	—	—				—	—	3,249	= ⅝ Breaking strain.
		8		2·00	—	110·00	·26	·09	—				—	—	3,404	
		9		1·00	—	120·00	·28	·02	—				—	—	3,714	
		10		1·00	—	130·00	·30	·02	—				—	—	4,023	
		11		1·00	—	140·00	·32	·02	—				—	—	4,332	
		12		·50	—	145·00	·33	·01	—				—	—	4,487	
		13		·50	—	150·00	·35	·02	—				—	—	4,642	
		14		·50	—	155·00	·37	·02	—				—	—	4,797	
		15		·25	—	157·50	—	—	—				—	—	4,874	Broke close to the clamp, after bearing the weight a few seconds. All strands broken; fair break. Clamped five times.
July 14	No. 15. (Kemp and Iron double lay.) Outside diameter = ·75 inches. E. Wright's Sample. 1st Sample tested.	1	3 36	1·00	—	10·00	100·00	·00	—				24·80	13·80	735	
		2	33	1·00	—	20·00	·60	·60	—				—	—	1,470	Cable steeped in water for about two months, and exposed to the air for about six weeks.
		3	34	1·00	—	25·25	101·23	—	—				—	—	1,939	= ¼ Breaking strain.
						30·00	·55	·95	—				—	—	2,324	
						35·00	·96	—	—				—	—	2,709	= ⅜ Breaking strain.
		4	38	1·00	—	40·00	102·23	·70	—				—	—	3,039	Tar oozing out.
		5	40	·25	—	42·50	·49	·24	—				—	—	3,123	
		6	43	·25	—	45·00	·66	·17	—				—	—	3,308	
		7	44	·25	—	47·50	·83	·17	—				—	—	3,499	
		8	47	·25	—	50·00	103·17	·30	—				—	—	3,674	
		9	49	·25	—	52·50	·50	·13	—				—	—	3,858	Broke right across near lashing.
July 15	2d Sample tested.	1	2 5	1·00	—	10·00	100·00	·00	—	69			24·80	13·80	735	
		2	12	1·00	—	20·00	·60	·60	—				—	—	1,470	Tested in water and electrically.
						22·75	·99	—	—				—	—	1,745	= ¼ Breaking strain.
		3	16	1·00	—	30·00	101·02	1·02	—				—	—	2,204	
						31·67	·73	—	—				—	—	2,327	= ⅜ Breaking strain.
		4	20	·50	—	35·00	102·05	·43	—				—	—	2,572	
		5	23	·50	—	40·00	·28	·63	—				—	—	2,939	
		6	26	·25	—	42·50	·73	·25	—				—	—	3,123	
		7	28	·25	—	45·00	·93	·20	—				—	—	3,306	
		8	30	·25	—	47·50	Over 3 per cent.						—	—	3,490	All broke, including conductor.
July	No. 16. (Tweed and Rogers.) Outside diameter = 1·00 inch. 1st Sample tested.	1		1·00	—	10·00	50·00	—	—				21·42	4·50	2,252	
		2		·50	—	15·00	·72	·72	—				—	—	3,378	
		3		·25	—	17·50	·88	·16	—				—	—	3,941	= ½ Breaking strain.
		4		·25	—	20·00	51·07	·19	—				—	—	4,504	
		5		·25	—	22·50	·33	·31	—				—	—	5,067	
						23·33	·42	—	—				—	—	5,255	= ⅝ Breaking strain.
		6		·25	—	25·00	·48	·10	—				—	—	5,630	
		7		·25	—	27·50	·65	·07	—				—	—	6,193	
		8		·25	—	30·00	·70	·15	—				—	—	6,756	
		9		·25	—	32·50	·80	·10	—				—	—	7,319	
		10		·25	—	35·00	52·03	·23	—				—	—	7,882	Elongated very fast, and broke near centre. Cut core longitudinally, and found copper parted about half an inch. Total elongation previous to breaking 4·06 per cent. Estimated per-centage of elongation:— ½ Breaking strain = 1·76 ⅝ Breaking strain = 2·84 Breaking strain = 4·06
July 5	No. 17. (Iron and Hemp.) Outside diameter ·844 inches. 1st Sample tested.	1		1·00	—	10·00	100·00	·00	—				24·87	12·65	800	
		2		1·00	—	20·00	·13	·13	—				—	—	1,600	
		3		1·00	—	30·00	·20	·07	—				—	—	2,400	
						37·50	·25	—	—				—	—	3,000	= ½ Breaking strain.
		4		1·00	—	40·00	·29	·04	—				—	—	3,200	
		5		1·00	—	50·00	·42	·13	—				—	—	4,000	
		6		1·00	—	60·00	·62	·20	—				—	—	4,800	
		7		·50	—	65·00	101·00	·38	—				—	—	5,200	A portion of 150 feet specimen last sent from Newall's.
		8		·50	—	70·00	·45	·45	—				—	—	5,600	Note the amount of stretch.
		9		·50	—	75·00	·85	·40	—				—	—	6,000	Broke fairly; six strands.

Weights put on at intervals of one minute.

Weights put on at intervals of two minutes.

Tested in Air.

Perfect throughout. Do. Do.

Tested in Air.

APPENDIX No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time.	Testing for Strength. Weight. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. [illegible] P. P.	Continuity. P. P.	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	Cwts.	Cwts.	Cwts.	Ins.	Ins.	Ins.	°		°	Cwts.	Cwts.	Fms.	
6 July	No. 17. (Iron and Hemp.) Outside diameter = ·844 inches. 2d Sample tested. (Joint in gutta percha, and one wire welded.)	1	11 15AM	1·00	—	10·00	100·00	—	—				24·87	12·65	800	
		2	—	1·00	—	20·00	·27	·27	—				—	—	1,600	
		3	—	1·00	—	30·00	·32	·35	—				—	—	2,400	
						35·00	·77	—	—				—	—	2,800	=½ Breaking strain.
		4	—	1·00	—	40·00	·93	·91	—				—	—	3,200	
						46·67	101·05	—	—				—	—	3,733	=½ Breaking strain.
		5	—	1·00	—	50·00	·15	·22	—				—	—	4,000	Tar oozing out.
		6	—	·50	—	55·00	·40	·25	—				—	—	4,400	
		7	—	·50	—	60·00	·80	·40	—				—	—	4,800	
		8	—	·25	—	62·50	102·24	·44	—				—	—	5,000	
																Cable opening slightly round the bend of the thimble.
		9	—	·25	—	65·00	·48	·19	—				—	—	5,200	Tar oozing out considerably
		10	—	·25	—	67·50	·60	·17	—				—	—	5,400	more at that part.
		11	—	·25	—	70·00	103·06	·46	—				—	—	5,600	Broke at the joint of gutta percha and conducting wire, diameter of gutta percha considerably reduced. Seven wires broken. Welded iron wire remained perfect at joint.
										Tested in Air.						Tested by Mr. Biedermann.
7 July	No. 17. (Iron and Hemp Wire.) Outside diameter = ·844 inches. 3d Sample tested. Labelled (weld in wire).	1	—	1·00	—	10·00	100·00	—	—				24·87	12·65	800	
		2	—	1·00	—	20·00	·20	·20	—				—	—	1,600	
		3	—	1·00	—	30·00	·40	·20	—				—	—	2,400	
		4	—	·50	—	35·00	·48	·08	—				—	—	2,800	
						37·50	·55	—	—				—	—	3,000	=½ Breaking strain.
		5	—	·50	—	40·00	·56	·08	—				—	—	3,200	Tar oozing out.
		6	—	·50	—	45·00	·67	·11	—				—	—	3,600	
		7	—	·50	—	50·00	·85	·19	—				—	—	4,000	=½ Breaking strain.
		8	—	·50	—	55·00	101·20	·35	—				—	—	4,400	
		9	—	·25	—	57·50	·38	·18	—				—	—	4,600	
		10	—	·25	—	60·00	·04	·29	—				—	—	4,800	
		11	—	·25	—	62·50	·90	·23	—				—	—	5,000	
		12	—	·25	—	65·00	102·20	·30	—				—	—	5,200	
		13	—	·25	—	67·50	·43	·23	—				—	—	5,400	
		14	—	·25	—	70·00	·60	·17	—				—	—	5,600	
		15	—	·25	—	72·50	·75	·15	—				—	—	5800	
		16	—	·25	—	75·00	103·00	·25	—				—	—	6,000	Broke about two feet from clamp. Six wires. Core stretched equally and apparently uninjured.
8 July	No. 17. (Hemp and Iron.) Without joint. Outside diameter = ·844 inches. 4th Sample tested.	1	4 0	1·00	—	10·00	100·00	—	—	71°			24·87	12·65	800	
		2	2	1·00	—	20·00	·17	·17	—				—	—	1,600	
		3	5	1·00	—	30·00	·33	·18	—				—	—	2,400	Tested in water with 2 % salt.
						37·50	·47	—	—				—	—	3,000	=½ Breaking strain.
		4	7	1·00	—	40·00	·51	·16	—				—	—	3,200	
		5	9	·50	—	45·00	·62	·11	—				—	—	3,600	
		6	11	·50	—	50·00	·75	·13	—				—	—	4,000	=¾ Breaking strain. Tar and air oozing through.
		7	13	·50	—	55·00	101·03	·28	—				—	—	4,400	
		8	15	·25	—	57·50	·20	·27	—				—	—	4,600	
		9	17	·25	—	60·00	·69	·29	—				—	—	4,800	
		10	19	·25	—	62·50	·75	·14	—				—	—	5,000	
		11	21	·25	—	65·00	·85	·20	—				—	—	5,200	
		12	24	·25	—	67·50	102·35	·49	—				—	—	5,400	
		13	25	—	1·00	67·50	·35	—	—				—	—	4,600	Commenced taking off weights.
		14	26	—	1·00	67·50	·18	—	·17				—	—	3,800	
		15	28	—	1·25	35·00	·11	—	·07	Tested in Water with 2 % of salt.			—	—	2,600	
		16	30	—	·50	30·00	·06	—	·05				—	—	2,400	
		17	31	—	1·00	20·00	·00	—	·06				—	—	1,600	
		18	32	—	1·00	10·00	101·91	—	·09				—	—	800	
		19	34	—	1·00	0·00	·81	—	·10				—	—	—	
		20	36	1·00	—	10·00	·91	·04	—				—	—	800	Commenced putting on weights after readjusting lever.
		21	37	1·00	—	20·00	·97	·06	—				—	—	1,600	
		22	38	1·00	—	30·00	·99	·03	—				—	—	2,400	
		23	41	1·00	—	40·00	102·07	·03	—				—	—	3,200	Outside diameter = ·797 ins.
		24	43	·50	—	45·00	·12	·05	—				—	—	3,600	
		25	47	·50	—	50·00	·16	·04	—				—	—	4,000	
		26	50	·50	—	55·00	·21	·05	—				—	—	4,400	
		27	51	·50	—	60·00	·25	·04	—				—	—	4,800	
		28	53	·50	—	65·00	·36	·05	—				—	—	5,200	
		29	53	·25	—	67·50	·38	·06	—				—	—	5,400	
		30	54	·25	—	70·00	·60	·14	—				—	—	5,600	
		31	55	·25	—	72·50	·84	·14	—				—	—	5,800	
		32	5 1	·25	—	75·00	103·10	·46	—				—	—	6,000	Elongating gradually.
		33	2	—	—	75·00	—	—	—				—	—	6,000	Broke fair, near lashing. All twelve wires broke. Core not broken.
12 July	No. 17. (Hemp and Iron with one gutta percha joint.) Outside diameter = ·844 inches. 5th Sample tested.	1		1·00	—	10·00	100·00	·00	—	69°	108 1 Perfect.		24·87	12·65	800	Tested in water. Also electrically.
		2		1·00	—	20·00	·03	·03	—				—	—	1,600	
		3		1·00	—	30·00	·25	·17	—				—	—	2,400	
						34·37	·31	—	—				—	—	2,700	=½ Breaking strain.
		4		1·00	—	40·00	·43	·18	—				—	—	3,200	Tar oozing out.
						45·00	·54	—	—				—	—	3,600	=½ Breaking strain.
		5		1·00	—	50·00	·66	·23	—				—	—	4,000	
		6		·50	—	55·00	·84	·18	—				—	—	4,400	
		7		·50	—	60·00	101·31	·47	—		1·5		—	—	4,800	
		8		·50	—	65·00	·80	·49	—	Remained perfect with no variation.	2·0		—	—	5,200	Broke fairly in several places. Seven strands broken; one strand broke in two places.
		9		·25	—	67·50	—	—	—				—	—	5,400	
13 July	No. 17. (Hemp and Iron without joint). Outside diameter = ·844 inches. 6th Sample tested.	1		1·00	—	10·00	100·00	·00	—	72°			24·87	12·65	800	Tested in water.
		2		1·00	—	20·00	·10	·10	—				—	—	1,600	
		3		1·00	—	30·00	·20	·10	—				—	—	2,400	
						37·50	·29	—	—				—	—	3,000	=½ Breaking strain.
		4		1·00	—	40·00	·31	·11	—				—	—	3,200	
		5		1·00	—	50·00	·46	·15	—				—	—	4,000	=¾ Breaking strain.
		6		·50	—	55·00	·65	·19	—				—	—	4,400	Tar oozing out.
		7		·50	—	60·00	101·05	·41	—	Perfect.			—	—	4,800	Insulation commenced to be imperfect.
		8		·50	—	65·00	·46	·40	—				—	—	5,200	
		9		·25	—	67·50	·70	·24	—				—	—	5,400	
		10		—	·25	65·00	·70	—	—				—	—	5,200	
		11		—	·50	60·00	·70	—	—				—	—	4,800	
		12		—	·50	57·00	·69	·03	—				—	—	4,400	
		13		—	·50	50·00	·64	·05	—				—	—	4,000	
		14		1·00	—	40·00	·69	·04	—				—	—	3,200	

Weights put on at intervals of two minutes.

App. No. 10.

Appendix No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Conductivity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
July 12	No. 17. (Hemp and Iron without joint.) Outside diameter = ·844 inches. 6th Sample tested.	15	Weights put on at intervals of two minutes.	1·00	—	20·00	101·55	—	·05	72·	perfect	perfect	24·87	12·65	2,400	
		16		—	1·00	20·00	·43	—	·07				—	—	1,600	
		17		—	1·00	10·00	·23	—	·03				—	—	800	
		18		1·00	—	20·00	·46	·04	—				—	—	1,600	
		19		1·00	—	30·00	·51	·03	—				—	—	2,400	
		20		1·00	—	40·00	·56	·05	—				—	—	3,200	
		21		1·00	—	50·00	·63	·07	—				—	—	4,000	
		22		·50	—	55·00	·66	·03	—				—	—	4,400	
		23		·50	—	60·00	·70	·04	—				—	—	4,800	
		24		·50	—	65·00	·74	·04	—				—	—	5,200	
		25		·25	—	67·50	·81	·07	—				—	—	5,400	
		26		·25	—	70·00	·96	·15	—		1·6		—	—	5,600	Tested in water.
		27		·25	—	72·50	102·13	·17	—		1·6		—	—	5,800	
		28		·25	—	75·00	—	—	—		—		—	—	6,000	Broke fairly; nine strand broken.
July 13	No. 17. (Iron and Hemp.) Outside diameter = ·844 inches. 7th Sample tested.	1	2 5	1·00	—	0·00	100·00	—	—	74·			24·87	12·65	800	
		2	7	1·00	—	20·00	·08	·08	—	—			—	—	1,600	Tested in water, and electrically.
		3	9	1·00	—	30·00	·23	·15	—	—			—	—	2,400	Cable steeped an hour in water, with a strain of 10 cwts. on.
						35·00	·30								2,800	=½ Breaking strain.
		4	11	1·00	—	40·00	·37	·14	—				—	—	3,200	
		5	13	·50	—	45·00	·47	·10	—				—	—	3,600	
						46·87	·50								3,733	=½ Breaking strain.
		6	15	·50	—	50·00	·60	·13	—				—	—	4,000	
		7	17	—	·50	45·00	·59	—	·01				—	—	3,600	Commenced taking off weight.
		8	19	—	·50	40·00	·57	—	·02				—	—	3,200	
		9	21	—	1·00	30·00	·51	—	·06				—	—	2,400	
		10	23	—	1·00	20·00	·44	—	·07				—	—	1,600	
		11	25	—	1·00	10·00	·36	—	·08				—	—	800	
		12	27	—	1·00	0·00	—	—	—				—	—	—	All weight off. Cable dropped.
		13	29	1·00	—	10·00	·30	—	·05				—	—	800	
		14	31	1·00	—	20·00	·38	·08	—				—	—	1,600	
		15	33	1·00	—	30·00	·45	·07	—				—	—	2,400	
		16	35	1·00	—	40·00	·54	·09	—				—	—	3,200	
		17	37	·50	—	45·00	·58	·04	—				—	—	—	
		18	39	·50	—	50·00	·65	·07	—				—	—	4,000	Tar oozing out.
		19	41	·50	—	55·00	·90	·25	—				—	—	4,400	
		20	43	·25	—	57·50	101·15	·25	—				—	—	4,600	
		21	45	·25	—	60·00	·47	·31	—				—	—	4,800	
		22	47	·25	—	62·50	·84	·37	—				—	—	5,000	
		23	49	·125	—	63·75	·91	·07	—				—	—	5,100	
		24	51	·125	—	70·00	—	—	—				—	—	5,600	Broke in trough about 2′ 6″ from lashing. Seven strands broke. Core remained perfect.
July 19	No. 17. (Iron wire covered with Hemp.) Outside diameter = ·844 inches. 8th Sample tested.	1	1 15	1·00	—	10·00	100·00	·00	—				24·87	12·65	800	
		2	21	1·00	—	20·00	·19	·19	—				—	—	1,600	
		3	23	1·00	—	30·00	·33	·14	—				—	—	2,400	
						35·00	·41								2,800	=½ Breaking strain.
		4	25	1·00	—	40·00	·50	·17	—				—	—	3,200	
						46·64	·64								3,734	=½ Breaking strain.
		5	27	1·00	—	50·00	·71	·21	—				—	—	4,000	
		6	29	·50	—	55·00	·90	·19	—				—	—	4,400	
		7	31	·50	—	60·00	101·20	·30	—				—	—	4,800	
		8	33	·25	—	62·50	·65	·35	—				—	—	5,000	
		9	35	·25	—	63·00	·92	·27	—				—	—	5,200	
		10	37	·25	—	67·50	102·23	·31	—				—	—	5,400	
		11	39	·25	—	70·00	—	—	—				—	—	5,600	Six strands broken.
July 7	No. 18. (Iron and Hemp with compound over core.) Outside diameter = ·815 inches. 1st Sample tested.	1	Weights put on at intervals of two minutes	1·00	—	10·00	100·00	—	—	Tested in Air			25·77	12·80	792	
		2		1·00	—	20·00	·10	·10	—				—	—	1,584	
		3		1·00	—	30·00	·18	·08	—				—	—	2,376	
						33·75	·23								2,673	=½ Breaking strain.
		4		1·00	—	40·00	·30	·12	—				—	—	3,168	
		5		·50	—	45·00	·40	·10	—				—	—	3,564	=½ Breaking strain.
		6		·50	—	50·00	·58	·18	—				—	—	3,960	
		7		·50	—	55·00	·73	·21	—				—	—	4,356	
		8		·50	—	60·00	101·35	·62	—				—	—	4,752	
		9		·25	—	62·50	·56	·21	—				—	—	4,950	
		10		·25	—	65·00	·78	·22	—				—	—	5,148	Tar oozing out.
		11		·25	—	67·50	102·00	—	—				—	—	5,346	Broke right across, core and all.
June 18	No. 19. (Core without outside compound.) Outside diameter = ·50 inches. Labelled core No. 1. 1st Sample tested.	1	Weights put on at intervals of about one minute.	·06	—	0·00	100·00	·00	—	Temperature of Water about 59°	Good to the last.	Good to the last.	7·15	3·36	188	
		2		·06	—	1·20	·03	·00	—				—	—	377	
		3		·13	—	2·30	·05	·05	—				—	—	754	
		4		·13	—	3·80	·08	·03	—				—	—	1,131	
						4·70	·09								1,413	=½ Breaking strain.
		5		·12	—	5·10	·10	·02	—				—	—	1,507	
		6		·12	—	6·30	·15	·05	—				—	—	1,884	=½ Breaking strain.
		7		·12	—	7·50	not observed.						—	—	2,261	Gave sudden start.
		8		·06	—	8·10	not observed.						—	—	2,450	Elongation scale not working properly, also clamp caught on block of wood which supported it.
		1		·50	—	5·00	100·00	0·00	—				—	—	1,507	Recommenced after adjusting scale.
		2		·18	—	6·30	103·05	2·05	—				—	—	1,884	
		3		·12	—	7·50	·10	·05	—				—	—	2,261	
		4		·12	—	8·70	·15	·05	—				—	—	2,638	
		5		·07	—	9·40	108·00	5·85	—				—	—	2,826	Elongated very suddenly.
						9·40	114·80	8·80	—				—	—	—	
						9·40	118·40	4·80	—				—	—	—	
						9·40	123·75	4·35	—				—	—	—	
						9·40	125·50	1·75	—				—	—	—	Broke diameter, lessened ¼ inch. Cut sample and labelled it (Core No. 1.) Cut the gutta percha both longitudinally and across; in both cases the gutta percha contracted violently, and forced out the copper conductor. Elongation very doubtful. (See Remarks.)

APPENDIX No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
Aug. 3	No. 12. (Gibraltar Core). Outside diameter = ·70 inch. 2d Sample tested.	1		1·00	—	10·00	70·00	·00	—	61	100	1	7·15	3·36		
		2		1·00	—	20·00	·06	·06	—	—			—	—		
		3		1·00	—	30·00	·09	·01	—	—			—	—		
		4		1·00	—	40·00	·15	·06	—	—			—	—		
		5		1·00	—	50·00	·27	·12	—	—			—	—		
	Total length of sample 80 feet, coiled up in four loops, 79 inches in length, thus—	6		1·00	—	60·00	81·23	2·23	—	—			—	—		
		7		·50	—	65·00	86·39	4·57	—	—			—	—		Scale pan touched the ground.
		8		—	·80	60·00	86·39	—	·00	—			—	—		Commenced taking off weights.
		9		—	1·00	50·00	·29	—	·10	—			—	—		
		10		—	1·00	40·00	·29	—	·00	—			—	—		
		11		—	1·00	30·00	·24	—	·05	—			—	—		
		12		—	1·00	20·00	·20	—	·04	—			—	—		
		13		—	1·00	10·00	·12	—	·08	—			—	—		
		14		1·00	—	20·00	—	—	—	—			—	—		Adjusted scale and recommenced putting on weights. Permanent stretch of core = 7·13 in 79 inches or 8·9 %.
		15		1·00	—	30·00	—	—	—	—			—	—		
		16		1·00	—	40·00	—	—	—	—			—	—		
		17		·50	—	45·00	—	—	—	—			—	—		
		18		·50	—	50·00	—	—	—	—			—	—		
		19		·50	—	55·00	—	—	—	—	Perfect throughout.	Perfect throughout.	—	—		
		20		·50	—	60·00	86·20	—	—	—			—	—		Hook broke. Scale was emptied.
		21		1·00	—	10·00	All this weight was put on to straighten the core.	—	—	—			—	—		Recommenced putting on weights.
		22		1·00	—	20·00	86·32	—	—	—			—	—		
		23		1·00	—	30·00	—	—	—	—			—	—		
		24		1·00	—	40·00	—	—	—	—			—	—		
		25		1·00	—	50·00	—	—	—	—			—	—		
		26		·50	—	55·00	86·39	7·39	—	—			—	—		
		27		·50	—	60·00	·47	·68	—	—			—	—		
		28		·50	—	65·00	·57	·38	—	—			—	—		
		29		·25	—	67·50	90·00	3·15	—	—			—	—		Broke right across, near centre of trough, about 17 feet from end. Electrically perfect throughout. Estimated per-centage of elongation:— Breaking strain = 14 per cent.
Sept. 8	No. 19. (Gibraltar Core without outside compound.) Outside diameter = ·70 inch. 3d Sample tested. Memo.—This sample tested by hanging weights to the core in a vertical position.	1		·50	—	·50	50·00	·00	—				7·15	3·36	151	
		2		·50	—	1·00	·00	·00	—				—	—	151	
		3		·50	—	1·50	·01	·01	—				—	—	303	
		4		·50	—	2·00	·02	·01	—				—	—	603	
		5		·50	—	2·50	·04	·02	—				—	—	754	
		6		·50	—	3·00	·06	·02	—				—	—	902	
		7		·50	—	3·50	·10	·04	—				—	—	7,055	
		8		·50	—	3·75	·14	—	—				—	—	1,130	= ½ Breaking strain.
		9		·50	—	4·00	·19	·05	—				—	—	1,205	
		10		·25	—	4·25	·55	·36	—				—	—	1,356	
		11		·25	—	4·75	·93	·38	—				—	—	1,431	
		12		·25	—	5·00	51·30	·37	—				—	—	1,507	= ½ Breaking strain.
		13		·25	—	5·25	·75	·45	—				—	—	1,582	
		14		·25	—	5·50	52·20	·45	—				—	—	1,658	
		15		·25	—	5·75	·70	·50	—				—	—	1,733	
		16		·25	—	6·00	53·34	·64	—				—	—	1,808	
		17		·25	—	6·25	54·062	·722	—				—	—	1,884	
		18		·25	—	6·50	55·062	1·00	—				—	—	1,959	
		19		·25	—	6·75	56·31	1·25	—				—	—	2,034	
		20		·25	—	7·00	58·18	1·77	—				—	—	2,110	
		21		·25	—	7·25	60·37	2·19	—				—	—	2,185	
		22		·25	—	7·50	—	—	—				—	—	2,260	Elongated rapidly. Impossible to read. Broke fair.
Sept. 8	No. 19. (Gibraltar Core without outside compound.) Outside diameter = ·70 inch. 4th Sample tested.	1		·50	—	·50	50·00	·00	—				7·15	3·36	151	
		2		1·00	—	1·50	·00	·00	—				—	—	452	Broken by the same arrangement as the *3d Sample* tested.
		3		1·00	—	2·50	·03	·03	—				—	—	754	
		4		1·00	—	3·50	·10	·07	—				—	—	7,055	
		5				3·75	·14	—	—				—	—	1,121	= ½ Breaking strain.
		6		·50	—	4·00	·19	·05	—				—	—	1,206	
		7		·50	—	4·50	·52	·33	—				—	—	1,356	
		8		·50	—	5·00	51·15	·63	—				—	—	1,507	= ½ Breaking strain.
		9		·50	—	5·50	·87	·72	—				—	—	1,658	
		10		·25	—	5·75	52·39	·52	—				—	—	1,733	
		11		·25	—	6·00	·95	·56	—				—	—	1,808	
		12		·25	—	6·25	53·50	·55	—				—	—	1,884	
		13		·25	—	6·50	54·312	·812	—				—	—	1,959	
		14		·25	—	6·75	55·50	1·188	—				—	—	2,034	
		15		·25	—	7·00	56·75	1·25	—				—	—	2,110	
		16		·25	—	7·25	58·25	1·50	—				—	—	2,185	
		17		·125	—	·375	60·50	2·25	—				—	—	2,223	
		18		·0625	—	7·4375	62·50	2·00	—				—	—	2,242	Broke fair.
Sept. 8	No. 20. (Copper conductor. Gibraltar cable.) 1st Sample tested.	1		·50	—	·50	50·00	·00	—				8·87	3·125		
		2		·50	—	1·00	·02	·02	—				—	—		
		3		·50	—	1·50	·05	·03	—				—	—		
		4		·50	—	2·00	·08	·03	—				—	—		
		5		·50	—	2·50	·11	·03	—				—	—		
						2·87	·14						—	—		= ½ Breaking strain.
		6		·50	—	3·00	·15	·04	—				—	—		
		7		·50	—	3·50	·26	·11	—				—	—		= ½ Breaking strain.
						3·83	·45						—	—		
		8		·50	—	4·00	·26	·87	—				—	—		
		9		·25	—	4·25	51·65	·73	—				—	—		

APPENDIX, No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form of Outer Covering of Submarine Telegraphic Cables.

Date of Experiment.	Description and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
Sept. 8	No. 20. (Copper conductor, Gibraltar Cable.) 1st Sample tested.	10	—	·25	—	4·50	52·50	·85	—	Tested in Air in a vertical position, weights hung on direct.			3·57	3·125	—	
		11	—	·25	—	4·75	53·50	1·00	—				—	—	—	
		12	—	·125	—	4·875	54·25	·75	—				—	—	—	Elongation meter slipped.
		13	—	·125	—	5·00	—	—	—				—	—	—	
		14	—	·125	—	5·125	—	—	—				—	—	—	
		15	—	·125	—	5·250	—	—	—				—	—	—	
		16	—	·125	—	5·375	—	—	—				—	—	—	
		17	—	·125	—	5·50	—	—	—				—	—	—	
		18	—	·125	—	5·625	—	—	—				—	—	—	
		19	—	·125	—	5·750	—	—	—				—	—	—	Broke fair.
July 27	No. 16. (All Hemp.) Tweed and Rogers. Outside diameter = 1·00 inch. 2d Sample tested. Memo.—For 1st Sample tested see page 18.	1	2 55	1·00	—	10·00	100·00	·00	—	67·	100	1	21·42	5·58	1,804	=⅓ Breaking strain.
		2	3 0	1·00	—	20·00	' ·98	·98	—	—			—	—	3,382	
		3	3	·50	—	25·00	101·42	·44	—	—	Perfect.	Perfect.	—	—	4,510	=½ Breaking strain.
		4	4	·50	—	30·00	·96	·54	—	—			—	—	5,412	
		5	6	·25	—	32·50	102·20	·24	—	—			—	—	5,863	
		6	8	·25	—	35·00	·46	·26	—	—			—	—	6,314	
		7	10	·25	—	37·50	—	—	—	—			—	—	6,765	Broke fair. Ten longitudinal strands broke. Outside covering and core remained good.
				The increased weight in water in *this* instance is due to the specimen having been soaked in water for a considerable period before having been weighed.												
																Specification of Specimen: Core: per Knot. Copper - - 3·57 Cwt. Gutta percha - 3·07 „ Longitudinal hemp strands put on with marine glue 7·76 „ Web hemp covering - - - 8·53 „ Total weight in air - - 21·42 „ Weight in water 5·58*
Aug. 22	No. 22. (Gibraltar core covered with vulcanized India-rubber tape, the outside covering being 12 iron wires, No. 15 gauge, each wire served with hemp.) Outside diameter = ·750 inch. 1st Sample tested.	1	12 15	1·00	—	10·00	80·00	·00	—	Tested in Air.			Not known.			Lay of covering at this strain = 4·62 inch.
		2	1 1	1·00	—	20·00	·73	·73	—							=⅓ Breaking strain.
		3	3	1·00	—	30·00	81·13	·49	—							At this strain, lay = 4·75 inches; elongation of lay = ½ inch.
		4	5	1·00	—	38·67	·38	—	—							=⅔ Breaking strain.
		5	7	·25	—	42·50	·75	·15	—							At this strain, vulcanized rubber serving squeezed up between outside strands. The surface of rubber serving covered with small yellow bubbles, which on bursting liberated powdered sulphur.
		6	9	·25	—	45·00	·92	·17	—							
		7	11	·25	—	47·50	82·07	·15	—							
		8	13	·25	—	50·00	·29	·13	—							
		9	15	·25	—	52·50	·35	·13	—							At this strain, outside diameter = ·688 inches.
		10	17	·25	—	55·00	—	—	—							Broke instantly weight was put on. Four strands broken midway between clamps.
																At site of fracture core very much twisted and indented, leaving cross section of core much like the figure 8.
																Estimated per-centage of elongation:— ⅓ Breaking strain = 1·28 ½ Breaking strain = 1·72 Breaking strain = 2·03
Aug. 22	No. 23. Red Sea Core covered with vulcanized India-rubber tape, the outside covering being 16 iron wires, No. 18 gauge, each wire served with hemp. Outside diameter = ·625 inch. 1st Sample tested.	1	1 31	1·00	—	10·00	80·00	·00	—							Lay of outer covering at this strain = 4·062 inch. Better laid up than Specimen No. 22.
		2	46	1·00	—	20·00	·40	·40	—							=⅓ Breaking strain.
		3	48	·50	—	25·00	·87	·47	—							
		4	50	·50	—	26·67	81·18	—	—							=⅔ Breaking strain.
		5	52	·50	—	30·00	81·45	·58	—							Rubber showing between the outside covering, but only in a few places.
		6	54	·50	—	40·00	—	—	—							At this strain, lay of covering = 4·188 inch.
																Broke at the place the rubber was visible. Eight strands broken. Core much indented at place of fracture, also twisted; the cross section of core like the figure 8.
																Estimated per-centage of elongation:— ⅓ Breaking strain = ·50 ½ Breaking strain = 1·37 Breaking strain = 2·46

APPENDIX No. 10.—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen and Sample tested.	No.	Time	Tests for Strength. Weight. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
Aug. 22	No. 24. Outside diameter = ·756 inch. 1st Sample tested. Lay of covering at this strain = 6·125.	1	2 29	1·00	—	10·00	60·00	·00	—				41·22	30·60	577	
		2	27	1·00	—	20·00	·06	·06	—				—	—	522	
		3	29	1·00	—	30·00	·09	·03	—				—	—	934	
		4	31	1·00	—	40·00	·10	·01	—				—	—	1,345	
		5	40	1·00	—	50·00	·14	·04	—				—	—	1,757	
	Memo.—This Specimen much the same as Specimen No. 14 (No. 1 Gibraltar deep Sea), with India-rubber tape serving instead of hemp, and No. 12 gauge wires instead of No. 11 gauge.	6	42	1·00	—	60·00	·16	·02	—				—	—	2,068	
						65·00	·18	—	—				—	—	2,324	= ½ Breaking strain.
		7	44	1·00	—	70·00	·20	·04	—				—	—	2,580	
		8	46	1·00	—	80·00	·25	·03	—				—	—	2,691	
						87·00	·27	—	—				—	—	2,811	= ¾ Breaking strain.
		9	48	1·00	—	90·00	·28	·03	—				—	—	3,002	
		10	50	1·00	—	100·00	·31	·05	—				—	—	3,314	Rubber coming through the outside covering, but only where the wires are not well laid.
		11	52	1·00	—	110·00	·40	·09	—				—	—	3,625	
		12	54	1·00	—	120·00	·59	·19	—				—	—	3,936	
		13	56	1·00	—	130·00	—	—	—				—	—	4,247	Broke directly weight was put on (not a fair break). Ten strands broken. Broke close to clamps. Estimated per-centage of elongation:— ¾ Breaking strain = ·22. ¾ Breaking strain = ·33. Breaking strain = ·73.
Sept. 10	No. 25. (Gibraltar core coated with compound and covered with vulcanized India-rubber tape, over which a thick coating of compound, the outer covering 18 iron wires, No. 10 gauge). Outside diameter = ·925 inch.	1	2 6	1·00	—	10·00	100·00	·00	—				58·61	41·70	243	
		2	2	2·00	—	20·00	·15	·15	—				—	—	770	
		3	4	2·00	—	30·00	·25	·10	—				—	—	1,216	
		4	6	2·00	—	70·00	·36	·11	—				—	—	1,702	
						80·00	·41	—	—				—	—	2,026	= ½ Breaking strain.
		5	8	2·00	—	90·00	·45	·09	—				—	—	2,188	
		6	10	2·00	—	110·00	·52	·07	—				—	—	2,675	= ¾ Breaking strain.
		7	12	2·00	—	130·00	·63	·11	—				—	—	3,161	
		8	14	2·00	—	150·00	·80	·17	—				—	—	3,647	
		9	16	1·00	—	160·00	101·25	·45	—				—	—	3,891	
		10	18	·50	—	165·00	—	—	—				—	—	4,012	Broke fair. One strand before the others.
Sept. 10	No. 26. Gibraltar core coated with compound and covered with vulcanized India-rubber tape, over which a thick coating of compound, the outer covering 12 iron wires, No. 14 gauge, each covered with hemp. Outside diameter 1·125 inch. 1st Sample tested.	1		1·00	—	10·00	100·00	·00	—				30·43	15·35	661	
		2		1·00	—	20·00	·13	·13	—				—	—	1,322	
		3		1·00	—	30·00	·25	·22	—				—	—	1,983	
		4		1·00	—	40·00	·70	·38	—				—	—	2,644	
						42·50	·91	—	—				—	—	2,809	= ½ Breaking strain.
		5		1·00	—	50·00	101·55	·85	—				—	—	3,305	One wire broken.
						54·67	102·40	—	—				—	—	3,613	= ¾ Breaking strain.
		6		·50	—	55·00	103·50	·95	—				—	—	3,635	
		7		·50	—	60·00	103·60	1·60	—				—	—	3,966	Second wire broke. Third broke.
		8		·25	—	62·50	103·96	·40	—				—	—	4,131	Four wires broken. Five broken. Six broken.
		9		·25	—	65·00	—	—	—				—	—	4,296	
		10		·25	—	67·50	—	—	—				—	—	4,461	Seven wires broken. Eight broken.
		11		·25	—	70·00	—	—	—				—	—	4,626	
		12		·25	—	72·50	—	—	—				—	—	4,792	
		13		·25	—	75·00	—	—	—				—	—	4,957	Nine wires broken.
		14		·25	—	77·50	—	—	—				—	—	5,122	Ten wires broken.
		15		·25	—	80·00	—	—	—				—	—	5,287	
		16		·25	—	82·50	—	—	—				—	—	5,453	Eleven wires broken. Twelve wires broken.
		17		·25	—	85·00	—	—	—				—	—	5,618	Broke. Memo.—The excess of hemp over iron in this Specimen may account for the above extraordinary results of the hemp holding the cable together after many wires broke.
Sept. 12	No. 27. (Tweed and Rogers.) (No particulars given.) Outside diameter ·719 inch. 1st Sample tested.	1		1·00	—	10·00	40·00	·00	—							= ½ Breaking strain.
		2		1·00	—	20·00	—	—	—							= ¾ Breaking strain.
	Outside diameter = ·656 inch. 2d Sample tested.	1		·50	—	5·00										Broke very slowly. (Core as well as outside covering.)
		2		·25	—	7·50										
						8·75	To admit of measuring elongation.									
		3		·25	—	10·00										= ½ Breaking strain.
						11·87										= ¾ Breaking strain.
		4		·25	—	12·50										Specimen too short to take elongation, so to get the breaking strain accurately.
		5		·25	—	15·00										Broke fair. (Core as well as outside covering.)
		6		·25	—	17·50										
	Outside diameter = ·75 inch. 2d Sample tested.	1		·50	—	5·00	73·75	·00	—							= ½ Breaking strain.
		2		·25	—	7·50	74·15	·40	—							= ¾ Breaking strain.
		3		·25	—	10·00	75·55	1·40	—							
		4		·25	—	12·50	76·35	·80	—							
		5		·25	—	15·00	77·05	·70	—							Broke fair. (Core as well as outside covering.)
																Estimated per-centage of elongation:— ¾ Breaking strain = ·60. ¾ Breaking strain = 2·44. Breaking strain = 4·47.
Oct. 8	No. 28. (Henley.) Gibraltar core covered with 18 Iron and Hemp strands, each strand composed of three iron wires, No. 20 Birmingham gauge, with three small hempen strands. Outside diameter = ·875 inch. 1st Sample tested. Memo.—Hemp serving round core right and left lay, inside serving 22 yarns, outside serving 24 yarns.	1	2 48	1·00	—	10·00	100·00	·00	—				26·32	13·67	742	This Specimen was tested with swivels at each end.
		2	45	1·50	—	20·00	·24	·24	—				—	—	1,486	Tar oozing out.
		3	47	·50	—	25·00	·36	·12	—				—	—	1,856	
		4	49	·25	—	27·50	·43	·07	—				—	—	2,042	
		5	51	·25	—	30·00	·46	·03	—				—	—	2,227	
						31·25	·51	—	—				—	—	2,320	= ½ Breaking strain.
		6	53	·25	—	32·50	·56	·10	—				—	—	2,413	
		7	55	·25	—	35·00	·64	·03	—				—	—	2,599	
		8	57	·25	—	37·50	·72	·08	—				—	—	2,784	
		9	59	·25	—	40·00	·74	·04	—				—	—	2,970	
						41·87	·78	—	—				—	—	3,094	= ¾ Breaking strain.
		10	3 1	·25	—	42·50	·80	·02	—				—	—	3,156	
		11	3	·25	—	45·00	·86	·06	—				—	—	3,341	
		12	5	·25	—	47·50	·93	·08	—				—	—	3,527	A great deal of tar oozing out all over cable.
		13	7	·25	—	50·00	101·00	·07	—				—	—	3,712	
		14	9	·25	—	52·50	·09	·09	—				—	—	3,898	
		15	11	·25	—	55·00	·28	·19	—				—	—	4,084	
		16	13	·25	—	57·50	·31	·12	—				—	—	4,269	
		17	15	·25	—	60·00	·56	·25	—				—	—	4,455	
		18	—	·25	—	62·50	—	—	—				—	—	4,641	Broke as soon as weight was put on. All strands broke.

Tested in Air.

Not known.

Weights added at intervals of about two minutes.

App. No. 10.

Appendix No. 10—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens for the purpose of arriving at the best form for the Outer Covering of Submarine Telegraphic Cables—*continued.*

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time.	Tests for Strength. Weight. Put on.	Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot/ In Air.	In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:			H. M.	*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
Oct. 17	No. 28. (Henley.) Gibraltar core covered with 18 iron and hemp strands, each strand composed of 3 iron wires, No. 20. Birmingham gauge, with 3 small hempen strands. Outside diameter = ·875 inch. 2d Sample tested. Memo.—Hemp serving round core right and left lay. Inside serving 22 yarns, outside ditto 24 yarns.	1	12 35	1·00	—	10·00	70·00	·00	—				26·32	15·67	742	Swivels attached the same as first sample.
		2	45	1·00	—	20·00	·17	·17	—				—	—	1,484	
		3	47	1·00	—	30·00	·33	·15	—				—	—	2,226	
						33·50	·35	—	—				—	—	2,525	
		4	49	1·00	—	40·00	·45	·15	—				—	—	2,968	=⅓ Breaking strain.
						44·67	·54	—	—				—	—	3,139	=⅓ Breaking strain.
		5	51	·50	—	45·00	·55	·10	—				—	—	3,340	
		6	53	·50	—	50·00	·62	·07	—				—	—	3,711	
		7	55	·50	—	55·00	·75	·13	—				—	—	4,082	
		8	57	·25	—	57·50	·84	·09	—				—	—	4,267	
		9	59	·25	—	60·00	71·00	·16	—				—	—	4,453	
		10	1 1	·25	—	62·50	·19	·19	—				—	—	4,638	
		11	3	·25	—	65·00	·27	·08	—				—	—	4,824	
		12	5	·25	—	67·50	—	—	—				—	—	5,009	Broke instantly the last weight was added. (Ten strands.) Estimated per-centage of elongation:— ⅓ Breaking strain = ·50. ⅔ Breaking strain = ·77. Breaking strain = 1·89.
Oct. 19	No. 9. Outside diameter = ·875 inch. 12th Sample tested. Swivels used. Memo.—Lay of 12 strands 6·5 inches. Lay of hemp round wire 1·8 inch.	1	1 16	1·00	—	10·00	100·00	·00	—				25·47	13·11	273	
		2	16	1·00	—	20·00	·17	·17	—				—	—	1,547	
		3	17	1·00	—	30·00	·33	·16	—				—	—	2,320	
		4	19	1·00	—	40·00	·49	·16	—				—	—	3,094	
		5	21	1·00	—	50·00	·64	·15	—				—	—	3,867	
		6	23	1·00	—	60·00	·77	·13	—				—	—	4,641	
		7	25	1·00	—	70·00	·93	·16	—				—	—	5,414	
						73·75	·00	—	—				—	—	5,704	=½ Breaking strain.
		8	27	1·00	—	80·00	101·11	·18	—				—	—	6,188	
		9	29	1·00	—	90·00	·33	·23	—				—	—	6,961	
		10	31	·50	—	95·00	·44	·11	—				—	—	7,348	
						98·38	·54	—	—				—	—	7,605	=⅓ Breaking strain.
		11	33	·50	—	100·00	·63	·18	—				—	—	7,735	
		12	35	·25	—	102·50	—	—	—				—	—	—	Hook broke.
			53	—	—	102·50	·76	·13	—				—	—	7,938	The same weights replaced.
		13	54	·25	—	105·00	·82	·07	—				—	—	8,131	
		14	56	·25	—	107·50	·92	·10	—				—	—	8,324	
		15	58	·25	—	110·00	102·05	·13	—				—	—	8,508	
		16	2 0	·25	—	112·50	·14	·09	—				—	—	8,701	
		17	2	·25	—	115·00	·27	·13	—				—	—	8,894	
		18	4	·25	—	117·50	·38	·11	—				—	—	9,087	
		19	6	·25	—	120·00	·54	·16	—				—	—	9,281	
		20	8	·25	—	122·50	·67	·13	—				—	—	9,474	
		21	10	·25	—	125·00	·83	·16	—				—	—	9,667	
		22	12	·25	—	127·50	·96	·13	—				—	—	9,861	Shackles of clamps broke.
		23	3 39	—	—	130·00	103·20	·24	—	Tested in Air.			—	—	10,054	
		24	41	·25	—	132·50	·43	·13	—				—	—	10,248	
		25	43	·25	—	135·00	·55	·12	—				—	—	10,441	
		26	45	·25	—	137·50	·83	·10	—				—	—	10,634	
		27	47	·25	—	140·00	·79	·14	—				—	—	10,828	
		28	49	·25	—	142·50	·92	·13	—				—	—	11,021	
		29	51	·25	—	145·00	104·06	·14	—				—	—	11,215	
		30	53	·25	—	147·50	—	—	—				—	—	11,407	Broke fair. Eight strands.
Oct. 22	No. 9*. Outside diameter = ·875 inch. 13th Sample tested. Swivels at each end. Memo.—Lay of hemp and steel strand = 6½ inches. Lay of hemp round steel, 4 strands = 1 inch.	1	8 3	1·00	—	10·00	100·00	·00	—				25·47	13·11	273	
		2	5	1·00	—	20·00	·09	·09	—				—	—	1,547	
		3	7	1·00	—	30·00	·31	·22	—				—	—	2,320	
		4	9	1·00	—	40·00	·35	·15	—				—	—	3,094	
		5	11	1·00	—	50·00	·46	·10	—				—	—	3,867	
		6	13	1·00	—	60·00	·57	·11	—				—	—	4,641	
		7	15	1·00	—	70·00	·70	·13	—				—	—	5,414	
						71·25	·73	—	—				—	—	5,510	=½ Breaking strain.
		8	17	1·00	—	80·00	·85	·15	—				—	—	6,188	
		9	19	1·00	—	90·00	101·05	·20	—				—	—	6,961	=⅓ Breaking strain.
						95·00	·11	—	—				—	—	7,348	
		10	21	1·00	—	100·00	·27	·22	—				—	—	7,735	
		11	23	1·00	—	110·00	·52	·29	—				—	—	8,508	
		12	25	1·00	—	120·00	·82	·36	—				—	—	9,281	
		13	27	1·00	—	130·00	102·40	·48	—				—	—	10,054	
		14	29	·50	—	135·00	·70	·30	—				—	—	10,440	
		15	31	·50	—	140·00	—	—	—				—	—	10,826	Elongation not taken. Scale obliterated.
		16	33	·25	—	142·50	—	—	—				—	—	11,019	Broke fair. Six strands.
Oct. 18	No. 12. (Red Sea Cable.) Outside diameter = ·583 inch. 8th Sample tested. Swivels used.	1	4 8	1·00	—	10·00	100·00	·00	—				21·00	17·30	586	
		2	16	1·00	—	20·00	·09	·09	—				—	—	1,171	
		3	18	1·00	—	30·00	·16	·07	—				—	—	1,757	
		4	20	1·00	—	40·00	·23	·07	—				—	—	2,342	
						43·75	·25	—	—				—	—	2,557	=⅓ Breaking strain.
		5	22	1·00	—	50·00	·29	·03	—				—	—	2,928	
						58·34	·35	—	—				—	—	3,409	=½ Breaking strain.
		6	24	1·00	—	60·00	·37	·08	—				—	—	3,513	
		7	26	·50	—	65·00	·42	·05	—				—	—	3,806	
		8	28	·50	—	70·00	·48	·06	—				—	—	4,059	
		9	30	·25	—	72·50	·50	·02	—				—	—	4,245	
		10	32	·25	—	75·00	·55	·05	—				—	—	4,302	
		11	34	·25	—	77·50	·60	·05	—				—	—	4,538	
		12	36	·25	—	80·00	·65	·05	—				—	—	4,674	
		13	38	·25	—	82·50	·73	·08	—				—	—	4,831	
		14	40	·25	—	85·00	·81	·08	—				—	—	4,968	
		15	42	·25	—	87·50	—	—	—				—	—	5,174	Broke close to clamps. (Twelve strands.)

FALMOUTH AND GIBRALTAR TELEGRAPH.

ABSTRACT of EXPERIMENTS made by MESSRS. GISBORNE and FORDE and C. W. SIEMENS, for determining the Strength, &c. of STEEL and IRON WIRES, and HEMP STRANDS, SEPARATE and COMBINED.

Number of Specimen.	Sample tested.	DESCRIPTION OF MATERIALS. WIRE AND HEMP COMBINED. WIRE. Steel (or Iron) Wire.	Number of Wires.	Gauge. Birmingham Wire Gauge.	Diameter of Wire in Decimals of Inch.	Weight per Fathom. Single Wire.	Stranded.	HEMP. Number of Strands. Round each Wire.	Total.	Length of Lay.	Weight per Fathom.	Total Weight per Fathom, Wire and Hempcombined.	Strength. ¼ Breaking.	Breaking.	Per Centage of Elongation. ¼ Breaking.	Breaking.	WIRE AND HEMP SEPARATE. WIRE. Strength. ¼ Breaking.	Breaking.	Per Centage of Elongation. ¼ Breaking.	Breaking.	HEMP. Strength. ¼ Breaking.	Breaking.	Per Centage of Elongation. ¼ Breaking.	Breaking.	Breaking Weight of Wire, plus ditto Hemp, (10 & 23.)	Strength of Materials combined compared to Strength, separate (Cols. 15 & 20.) Plus.	Minus.	Number of Sheet.	REMARKS.
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
No.	*No.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*Cwts*	*Cwts*	*Ins.*	*Ins.*	*Cwts*	*Cwts*	*Ins.*	*Ins.*	*Cwts*	*Cwts*	*Ins.*	*Ins.*	*Cwts*	*Cwts*	*Cwts*	*No.*	
													UNWELDED.								MANILLA.								
1	1	Steel	One	14	·079	·097	—	4	4	¾	·076	·173	5·62	9·25	·39	1·77	4·25	8·50	·28	1·80	—	—	—	—	—	—	—	1	
"	2	"	"	"	"	"	—	"	"	"	"	"	5·62	9·25	·36	1·77	—	—	—	—	—	—	—	—	—	—	—	2	
"	(1)	"	"	"	"	"	—	"	"	"	"	"	5·62	9·25	·36	1·77	—	—	—	—	—	—	—	—	—	—	—	"	
"	1	"	"	"	"	"	—	4	4	1	·049	·146	5·56	11·12	·49	3·20	4·00	8·00	·32	1·68	—	—	—	—	—	—	—	3	
"	2	"	"	"	"	"	—	"	"	"	"	"	5·69	11·37	·51	3·80	—	—	—	—	—	—	—	—	—	—	—	4	
"	3	"	"	"	"	"	—	"	"	"	"	"	5·37	10·75	·46	3·04	—	—	—	—	—	—	—	—	—	—	—	"	
2	1	"	"	"	"	"	—	4	4	1¼	·051	·148	6·24	12·37	·52	3·12	4·00	8·00	·32	1·80	—	—	—	—	11·87	7·80	—	5	
"	2	"	"	"	"	"	—	"	"	"	"	"	6·18	12·37	·38	3·48	—	—	—	—	—	—	—	—	—	—	—	6	
"	3	"	"	"	"	"	—	"	"	"	"	"	6·50	13·00	·48	3·12	—	—	—	—	—	—	—	—	—	—	—	7	
"	4	"	"	"	"	"	—	"	"	"	"	"	6·06	12·12	·48	2·86	—	—	—	—	—	—	—	—	—	—	—	8	
"	5	"	"	"	"	"	—	"	"	"	"	"	6·25	12·50	·45	2·33	—	—	—	—	—	—	—	—	—	—	—	"	
"	6	"	"	"	"	"	—	"	"	"	"	"	6·50	12·87	·40	2·65	—	—	—	—	—	—	—	—	—	—	—	"	
"	7	"	"	"	"	"	—	"	"	"	"	"	—	11·00	—	—	—	—	—	—	—	—	—	—	—	—	—	"	
3	1	"	"	"	"	"	—	4	4	1¼	·052	·149	5·94	11·87	·60	3·36	4·00	8·00	·34	1·40	—	—	—	—	—	—	—	9	Weight put on at once.
"	2	"	"	"	"	"	—	"	"	"	"	"	5·94	11·87	·61	3·33	—	—	—	—	—	—	—	—	—	—	—	10	Wire broke.
"	3	"	"	"	"	"	—	"	"	"	"	"	5·75	11·50	·59	3·36	—	—	—	—	—	—	—	—	—	—	—	"	
													UNWELDED.								RUSSIAN.								
5	1	Steel	One	14	·079	·097	—	4	4	¾	·077	·174	5·00	10·00	·36	3·00	—	—	—	—	—	—	—	—	—	—	—	11	
"	2	"	"	"	"	"	—	"	"	"	"	"	4·75	9·50	·39	1·86	—	—	—	—	—	—	—	—	—	—	—	"	
"	3	"	"	"	"	"	—	"	"	"	"	"	4·75	9·50	·45	2·18	—	—	—	—	—	—	—	—	—	—	—	"	
6	1	"	"	"	"	"	—	"	"	1	·050	·147	5·81	11·63	·51	3·46	—	—	—	—	—	—	—	—	—	—	—	12	Sent from E. S. Newall & Co.
"	2	"	"	"	"	"	—	"	"	"	"	"	5·62	11·25	·48	3·66	—	—	—	—	—	—	—	—	—	—	—	"	
"	3	"	"	"	"	"	—	"	"	"	"	"	5·31	10·63	·44	2·66	—	—	—	—	—	—	—	—	—	—	—	13	
7	1	"	"	"	"	"	—	4	4	1¼	·055	·152	5·44	10·87	·45	3·70	4·25	8·50	·36	1·80	1·31	2·62	—	—	11·12	—	·25	14	
"	2	"	"	"	"	"	—	"	"	"	"	"	5·75	11·50	·45	3·46	—	—	—	—	—	—	—	—	—	—	—	15	
"	3	"	"	"	"	"	—	"	"	"	"	"	5·88	11·75	·50	3·42	—	—	—	—	—	—	—	—	—	—	—	"	
"	4	"	"	"	"	"	—	"	"	"	"	"	5·88	11·75	·46	3·38	—	—	—	—	—	—	—	—	—	—	—	16	
"	5	"	"	"	"	"	—	"	"	"	"	"	5·62	11·25	·37	2·28	—	—	—	—	—	—	—	—	—	—	—	"	
"	6	"	"	"	"	"	—	"	"	"	"	"	—	11·00	—	—	—	—	—	—	—	—	—	—	—	—	—	"	
8	1	"	"	"	"	"	—	4	4	1¼	·050	·147	5·06	11·87	·49	3·32	—	—	—	—	—	—	—	—	—	—	—	17	Weights put on without stopping the usual interval of 15 minutes.
"	2	"	"	"	"	"	—	"	"	"	"	"	5·00	11·25	·47	3·22	—	—	—	—	—	—	—	—	—	—	—	"	
"	3	"	"	"	"	"	—	"	"	"	"	"	5·63	11·25	·43	2·85	—	—	—	—	—	—	—	—	—	—	—	18	
9	1	"	"	"	·085	"	—	—	—	—	—	—	—	—	—	—	4·38	8·76	·35	1·32	—	—	—	—	—	—	—	19	Sent from Messrs. B. Johnson, Manchester.
"	2	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	4·19	8·37	·26	1·35	—	—	—	—	—	—	—	"	
10	3	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	4·44	8·87	·32	1·52	—	—	—	—	—	—	—	20	"
"	1	"	"	"	·082	"	—	—	—	—	—	—	—	—	—	—	3·62	7·25	·24	0·92	—	—	—	—	—	—	—	21	"
"	2	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	3·56	7·12	·22	·81	—	—	—	—	—	—	—	"	
11	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	3·38	6·75	·18	·67	—	—	—	—	—	—	—	22	
"	2	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	3·00	6·00	·05	·43	—	—	—	—	—	—	—	23	Sent from E. S. Newall & Co.
"	3	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	3·00	6·00	·08	·52	—	—	—	—	—	—	—	"	
12	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	3·00	6·00	·16	·52	—	—	—	—	—	—	—	24	
13	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	4·69	9·37	·26	1·30	—	—	—	—	—	—	—	25	
"	2	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	4·25	8·50	·22	·68	—	—	—	—	—	—	—	"	
"	3	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	4·68	9·37	·26	·92	—	—	—	—	—	—	—	26	
"	4	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	4·93	9·87	·22	1·22	—	—	—	—	—	—	—	"	
													WELDED.								WELDED.								
12	1	Steel	One	14	—	—	—	—	—	—	—	—	—	—	—	—	2·13	4·25	·09	·24	—	—	—	—	—	—	—	24	
13	2	"	"	"	—	—	—	—	—	—	—	—	—	—	—	—	3·00	4·00	·22	·20	—	—	—	—	—	—	—	25	
													UNWELDED.								MANILLA.								
14	1	Iron	One	14	·079	·0861	—	4	4	¾	·0770	·1731	3·25	4·50	·17	·47	2·20	4·40	·14	·56	—	—	—	—	—	—	—	27	
"	2	"	"	"	"	"	—	"	"	"	"	"	2·25	4·50	·24	·70	—	—	—	—	—	—	—	—	—	—	—	"	
"	3	"	"	"	"	"	—	"	"	"	"	"	2·31	4·62	·17	·64	—	—	—	—	—	—	—	—	—	—	—	28	
15	1	"	"	"	"	"	—	"	"	1	·0615	·1576	2·75	5·50	·23	1·16	2·18	4·37	·14	·46	2·37	4·75	—	—	9·12	—	5·62	29	Wire broke first, and weight was sustained alone by Hemp; (when tested *combined*).
"	2	"	"	"	"	"	—	"	"	"	"	"	2·31	5·63	·22	2·17	—	—	—	—	—	—	—	—	—	—	—	30	Do.
"	3	"	"	"	"	"	—	"	"	"	"	"	2·37	4·75	·16	·46	—	—	—	—	—	—	—	—	—	—	—	"	Do.
16	1	"	"	"	"	"	—	"	"	1¼	·0598	·1559	4·25	8·50	·33	2·58	2·23	4·43	·18	·69	1·81	3·62	1·11	2·62	8·05	·45	—	31	Do.
"	2	"	"	"	"	"	—	"	"	"	"	"	4·18	8·37	·39	2·56	—	—	—	—	—	—	—	—	—	—	—	32	
"	3	"	"	"	"	"	—	"	"	"	"	"	4·06	8·12	·34	2·56	—	—	—	—	—	—	—	—	—	—	—	33	
"	4	"	"	"	"	"	—	"	"	"	"	"	—	5·00	—	—	—	—	—	—	—	—	—	—	—	—	—	"	Weight put on at once.
"	5	"	"	"	"	"	—	"	"	"	"	"	4·06	8·12	·37	2·86	—	—	—	—	—	—	—	—	—	—	—	33	
17	1	"	"	"	·096	"	—	"	"	1¼	·063	·168	3·43	6·86	·35	2·26	2·21	4·42	·16	·72	—	—	—	—	—	—	—	34	Sent from E. S. Newall & Co.
"	2	"	"	"	"	"	—	"	"	"	"	"	3·06	6·12	·25	1·83	—	—	—	—	—	—	—	—	—	—	—	35	
"	3	"	"	"	"	"	—	"	"	"	"	"	3·56	7·12	·32	2·26	—	—	—	—	—	—	—	—	—	—	—	"	

Abstract of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, for determining the Strength, &c. of Steel and Iron Wires, and Hemp Strands, separate and combined—*continued.*

Number of	DESCRIPTION OF MATERIALS.																											REMARKS.
	WIRE AND HEMP COMBINED.														WIRE AND HEMP SEPARATE.								Strength of Materials combined compared to Strength, separate (Obs. 15 & 26.)					
	WIRE.					HEMP.									WIRE.				HEMP.									
				Gauge.								Strength.		Per-Centage of Elongation.			Strength.		Per-Centage of Elongation.		Strength.		Per-Centage of Elongation.		Wire, plus Breaking Weight of ditto Hemp, (18 & 25.)			
Specimen.	Sample tested.	Steel (or Iron) Wire.	Number of Wires.	Birmingham Wire Gauge.	Decimals of Inch.	Weight per Fathom.	Number of Strands.	Single Wire.	Strand.	Round each Wire.	Total.	Length of Lay.	Weight per Fathom.	Total Weight per Fathom, Wire and Hemp combined.	½ Breaking.	Breaking.	½ Breaking.	Breaking.	½ Breaking.	Breaking.	½ Breaking.	Breaking.	½ Breaking.	Breaking.	Plus.	Minus.	Number of Sheet.	
2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
No.	*No.*		*No.*	*Ins.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*Cwts*	*Cwts.*	*Ins.*	*Ins.*	*Cwts*	*Cwts*	*Ins.*	*Ins.*	*Cwts*	*Cwts*	*Ins.*	*Ins.*	*Cwts*	*Cwts*	*Cwts*	*No.*	
							UNWELDED.				RUSSIAN.									UNWELDED.				RUSSIAN.				
18	1	Iron	One	14	·079	·096	—	4	4	1	·077	·173	3·38	4·75	·22	·66	2·25	4·50	·12	·56	—	—	—	—	—	—	—	36	
"	2	"	"	"	"	"	"	"	"	"	"	"	3·44	4·87	·19	·88	—	—	—	—	—	—	—	—	—	—	—	37	
"	3	"	"	"	"	"	"	"	"	"	"	"	2·56	5·00	·15	·92	—	—	—	—	—	—	—	—	—	—	—		
19	1	"	"	"	"	"	"	"	"	1	·055	·146	2·96	5·37	·22	1·12	—	—	—	—	—	—	—	—	—	—	—	38	
"	2	"	"	"	"	"	"	"	"	"	"	"	5·56	7·12	·27	3·08	—	—	—	—	—	—	—	—	—	—	—		
"	3	"	"	"	"	"	"	"	"	"	"	"	3·06	6·00	·22	2·40	—	—	—	—	—	—	—	—	—	—	—		
20	1	"	"	"	"	"	"	12	·049	"	"	·146	3·25	6·09	·26	2·46	—	—	—	—	—	—	—	—	—	—	—	39	
"	2	"	"	"	"	"	"	"	"	"	"	"	3·18	6·37	·15	2·00	—	—	—	—	—	—	—	—	—	—	—		
"	3	"	"	"	"	"	"	"	"	"	"	"	3·06	6·12	·22	1·60	—	—	—	—	—	—	—	—	—	—	—	40	
"	4	"	"	"	"	"	"	"	"	"	"	"	3·88	6·75	·20	1·34	—	—	—	—	—	—	—	—	—	—	—	41	Soaked in water 22 hours.
"	5	"	"	"	"	"	"	"	"	"	"	"	3·71	7·43	·23	2·46	—	—	—	—	—	—	—	—	—	—	—	42	Do. Do.
"	6	"	"	"	"	"	"	"	"	"	"	"	2·50	5·00	·16	1·04	—	—	—	—	—	—	—	—	—	—	—		Not soaked.
21	1	"	"	"	"	"	"	11	·055	"	"	·146	3·31	6·25	·18	1·92	2·09	4·18	·12	·48	—	—	—	—	—	—	—	43	
"	2	"	"	"	"	"	"	"	"	"	"	"	3·63	7·25	·24	2·30	—	—	—	—	—	—	—	—	—	—	—	44	
"	3	"	"	"	"	"	"	"	"	"	"	"	2·38	4·75	·12	1·48	—	—	—	—	—	—	—	—	—	—	—	45	
							NAKED WIRES.													UNWELDED.								
22	1	Iron	One	14	·079	·096	—	—	—	—	—	—	—	—	—	—	2·13	4·25	·09	·40	—	—	—	—	—	—	—	46	Not known from whom sent.
23	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	2·18	4·37	·12	·50	—	—	—	—	—	—	—		
24	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	2·26	4·50	·18	·52	—	—	—	—	—	—	—	47	
25	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	2·25	4·50	·10	·38	—	—	—	—	—	—	—		
26	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	2·38	4·75	·12	·48	—	—	—	—	—	—	—	48	
27	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	2·38	4·75	·08	·46	—	—	—	—	—	—	—		
28	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	1·93	3·87	·08	·30	—	—	—	—	—	—	—	49	
							NAKED WIRES.													WELDED.								
23	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	1·31	2·62	·04	·38	—	—	—	—	—	—	—	46	
24	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	1·31	2·62	·03	·28	—	—	—	—	—	—	—	47	
25	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	1·50	3·00	·04	·18	—	—	—	—	—	—	—		
26	1	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	1·12	2·25	·03	·40	—	—	—	—	—	—	—	48	
"	2	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	1·19	2·37	·04	·38	—	—	—	—	—	—	—		
"	3	"	"	"	"	"	—	—	—	—	—	—	—	—	—	—	1·12	2·25	·03	·23	—	—	—	—	—	—	—	49	
							COVERED WIRES.													UNWELDED.				RUSSIAN.				
29	1	Steel	One	14	·078	·095	—	4	4	11	·055	·140	5·37	10·75	·38	3·40	3·87	7·75	·90	3·40	1·12	2·25	·24	1·00	10·00	·75	—	50	Sent from R. S. Newall & Co.
30	1	"	"	"	"	"	—	"	"	"	"	"	5·37	10·75	·48	2·20	—	—	—	—	—	—	—	—	—	—	—	52	
"	2	"	"	"	"	"	—	"	"	14	·042	·137	5·25	10·50	·38	1·48	4·00	8·00	·40	1·70	1·37	2·75	·36	1·24	10·75	—	·25	53	
"	3	"	"	"	"	"	—	"	"	"	"	"	5·50	11·00	·36	1·68	—	—	—	—	—	—	—	—	—	—	—		
31	1	"	"	"	"	"	—	"	"	12	·041	·126	5·62	11·25	·52	3·03	3·87	7·75	·34	1·64	1·62	3·25	·62	1·60	11·00	·25	—	54	The hemp strands appear to have been thicker in this specimen than in the others. The breaking weight of the wire and hemp separate are respectively 25 cwt. lower than mentioned in the experiment sheet; the last weight having in each case broken them directly it touched the scales.
							NAKED WIRES.													UNWELDED.								
32	1	"	"	14	·075	·089	—	—	—	—	—	—	—	—	—	—	3·62	7·25	·30	1·00	—	—	—	—	—	—	—	55	Sent from Messrs. Webster and Horsfall.
"	2	"	"	"	"	"	"	"	"	"	"	"	3·62	7·25	·32	1·00	—	—	—	—	—	—	—	—	—	—	—		Do. do.
33	1	"	"	13	·085	·118	—	—	—	—	—	—	4·75	9·50	·30	·90	—	—	—	—	—	—	—	—	—	—	—	56	Do. do.
34	1	"	"	12	·097	·150	—	—	—	—	—	—	5·12	10·25	·22	·92	—	—	—	—	—	—	—	—	—	—	—	57	Do. do.
"	2	"	"	"	"	"	—	—	—	—	—	—	5·25	10·50	·22	·84	—	—	—	—	—	—	—	—	—	—	—		Do. do.
							WIRES, NAKED.													WELDED.								
32	1	"	"	14	·075	·089	—	—	—	—	—	—	—	—	—	—	1·62	3·25	·22	·32	—	—	—	—	—	—	—	55	Do. do.
"	2	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	1·50	3·00	·10	·24	—	—	—	—	—	—	—		Do. do.
33	1	"	"	13	·085	·118	—	—	—	—	—	—	—	—	—	—	2·12	4·25	—	—	—	—	—	—	—	—	—	56	Elongation not taken.
"	3	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	2·50	5·00	—	—	—	—	—	—	—	—	—		Wire much bent close to weld.
35	1	"	"	12	·097	·150	—	—	—	—	—	—	—	—	—	—	3·12	6·25	—	—	—	—	—	—	—	—	—	58	Do. do.
"	2	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	2·75	5·50	—	—	—	—	—	—	—	—	—		Do. do.
							NAKED WIRES.													UNWELDED.								
36	1	"	"	14	·070	·078	—	—	—	—	—	—	—	—	—	—	3·37	5·62	·75	·50	1·00	—	—	—	—	—	—	59	Patent steel.
"	2	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	3·25	6·50	·25	·92	—	—	—	—	—	—	—		Do.
"	3	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	3·25	6·50	·25	·90	—	—	—	—	—	—	—		Do.
37	1	"	"	13	·087	·122	—	—	—	—	—	—	—	—	—	—	4·50	9·00	·30	1·06	—	—	—	—	—	—	—	60	Do.
"	2	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	4·62	9·25	·30	1·50	—	—	—	—	—	—	—		Do.
38	1	"	"	16	·054	·065	—	—	—	—	—	—	—	—	—	—	1·75	3·50	·30	·22	—	—	—	—	—	—	—	61	Salter's steel.
"	2	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	1·62	3·25	·14	·46	—	—	—	—	—	—	—		Do.
"	3	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	1·68	3·37	·16	·50	—	—	—	—	—	—	—		Do.
39	1	"	"	15	·063	·111	—	—	—	—	—	—	—	—	—	—	2·25	4·50	·16	·39	—	—	—	—	—	—	—	62	Do.
"	2	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	2·26	4·50	·18	·44	—	—	—	—	—	—	—		Do.
"	3	"	"	"	"	"	"	"	"	"	"	"	—	—	—	—	2·22	4·44	·16	·06	—	—	—	—	—	—	—		Do.

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.* APP. No. 8

RESULTS of EXPERIMENTS made by Messrs. GISBORNE and FORDE and C. W. SIEMENS, upon the STRENGTH of STEEL and IRON WIRES (WELDED and UNWELDED), and HEMP STRANDS, SEPARATE and COMBINED.

Date of Experiments.	Number of Specimen.	Number of Samples tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*			*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						MANILLA.					
28 Sept.	1	1	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	½	·076	·173	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·08	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50		·12	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	3·50		·20	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weight put on at intervals of 1½ min.
			1·00	4·50		·28	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
				4·62		·29												
			1·00	5·50		·37	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	6·50		·46	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	7·50		·61	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·00		·72	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·50		·90	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	9·00	76·33	·83	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	9·25	—	—	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Broke fairly. Memo.—The above is a steel and hemp strand combined. Per-centage of elongation:— ½ Breaking strain = ·30 Breaking strain = 1·07
28 Sept.	1	1	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	½	·076	·173	Sent from Messrs. R. S. Newall and Co. Birkenhead.
			1·00	1·50		·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50		·07	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	3·50		·11	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
				4·25		·14												
			1·00	5·50		·16	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	6·00		·19	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	6·50		·23	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	6·75		·24	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	7·00		·29	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	6·25		·28	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	6·50		·30	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	6·75		·32	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	7·00		·35	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	7·25		·38	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	7·50		·41	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	7·75		·45	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	7·875		·49	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	7·9375		·50	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	8·0000		·53	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	8·0625		·54	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	8·1250		·55	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	8·1875		·57	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	8·2500		·61	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	8·3125		·64	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	8·3750		·67	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	8·4375		·76	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·0625	8·500		·90	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Broke. Memo.—Naked steel wire. Same Guage, &c. as above. Per-centage of elongation:— ½ Breaking strain = ·28 Breaking strain = 1·80
28 Sept.	1	2	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	½	·076	·173	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50		·11	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	3·50		·18	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	4·50		·27	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
				4·62		·26												
			1·00	5·50		·34	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	6·50		·44	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	7·50		·59	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·00		·69	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·50		·86	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	9·00	76·33	·97	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	9·25	—	—	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Broke while the last weight was being put on. Per-centage of elongation:— ½ Breaking strain = ·35 Breaking strain = 1·77
28 Sept.	1	3	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	½	·076	·173	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50		·13	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	3·50		·19	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	4·50		·26	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
				4·62		·27												
			1·00	5·50		·35	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	6·50		·46	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	7·50		·57	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·00		·69	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·50		·79	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	9·00	76·33	·88	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	9·25	—	—	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Broke while the last weight was being put on. Per-centage of elongation:— ½ Breaking strain = ·36 Breaking strain = 1·77
28 Sept.	2	1	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1	·049	·146	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	1·05	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50		·12	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	3·50		·19	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	4·50		·27	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
			1·00	5·50		·36	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
				5·56		·37												
			1·00	6·50		·53	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	7·00		·63	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	7·50		·76	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·00		·86	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·50	76·04	·21	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	9·00		·46	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	

Exs. No. 10.

APPENDIX—No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires, (Welded and Unwelded), and Hemp Strands; Separate and Combined—*continued.*

Date of Experiments.	Number of Specimens.	Number of Samples tested.	Weight. Put on.	Weight. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimal of Inch.	Wire. Weight per Fathom. Rounds each Wire.	Wire. Weight per Fathom. Total.	Hemp. No. of Strands.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Wire and Hemp combined. Weight per Fathom.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*			*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						MANILLA.				
23 Sept.	2	1	·25	0·25	76·51	·05	Steel	One	14	·079	·097	—	4	4	1	·049	·146	
			0·50	·68		·17											
			·25	0·75		·15											
			·25	1·00	77·00	·16											
			·25	10·25		·13											
			·25	10·50		·21											
			·25	10·75		·30											
			·25	11·00		·10											
			·125	11·125	—	—											Broke fairly, after sustaining the weight 1 min. Per-centage of elongation:— ½ Breaking strain = ·49 Breaking strain = 3·29
23 Sept.	2	1	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	—	—	—	—	—	Naked Steel Wire.
			1·00	1·50		·03											
			1·00	2·50		·09											Weights put on at intervals of 1½ min.
			1·00	3·50		·15											
			·50	4·00		·16											
			·50	4·50		·19											
			·50	5·00		·22											
			·50	5·50		·25											
			·50	6·00		·29											
			·50	6·50		·33											
			·50	7·00		·29											
			·25	7·25		·42											
			·25	7·50		·49											
			·25	7·75		·54											
			·25	8·00	—	—											Scale touched ground.
																	The wire broke after the weight, (same as before), was replaced. Per-centage of elongation:— ½ Breaking strain = ·33 Breaking strain = 1·98
23 Sept.	2	2	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1	·049	·146	Sent from Messrs. R. S. Newall & Co., Birkenhead.
			1·00	1·50		·06											
			1·00	2·50		·12											Weight put on at intervals of 1½ min.
			1·00	3·50		·20											
			1·00	4·50		·27											
			1·00	5·50		·36											
				5·50		·33											
			1·00	6·50		·43											
			1·00	7·50		·47											
			·50	8·00		·50											
			·50	8·50		·99											
			·50	9·00	76·27	·28											
			·25	9·25		·50											
			·25	9·50		·57											
			·25	9·75		·75											
			·25	10·00		·96											
			·125	10·125	77·05	·00											
			·125	10·250		·19											
			·125	10·375		·32											
			·125	10·500		·30											
			·125	10·625		·35											
			·125	10·750		·53											
			·125	10·875		·61											
			·125	11·00		·60											
			·125	11·125		·67											
			·125	11·250		·73											
			·125	11·375	—	—											Broke fairly. Per-centage of elongation:— ½ Breaking strain = ·51 Breaking strain = 3·60
23 Sept.	2	3	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1	·049	·146	Sent from Messrs. R. S. Newall & Co., Birkenhead.
			1·00	1·50		·05											
			1·00	2·50		·10											Weights put on at intervals of 1½ min.
			1·00	3·50		·16											
			1·00	4·50		·21											
				5·37		·30											
			1·00	5·50		·31											
			1·00	6·50		·42											
			1·00	7·50		·48											
			·50	8·00		·52											
			·50	8·50		·93											
			·50	9·00	76·20	·25											
			·25	9·25		·52											
			·25	9·50		·57											
			·25	9·75		·85											
			·25	10·00	77·00	·15											
			·25	10·25		·18											
			·25	10·50		·28											
			·25	10·75	—	—											Broke fairly. Per-centage of elongation:— ½ Breaking strain = ·40 Breaking strain = 3·04
29 Sept.	3	1	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·051	·148	Sent from Messrs R. S. Newall & Co., Birkenhead.
			1·00	1·50		·02											
			1·00	2·50		·06											Weights put on at intervals of 1½ min.
			1·00	3·50		·10											
			1·00	4·50		·13											
			1·00	5·50		·17											
				6·34		·21											
			1·00	6·50		·21											
			1·00	7·50		·25											
			1·00	8·50		·34											
			·50	9·00		·35											
			·50	9·50		·42											
			·50	10·00		·50											
			·50	10·50		·61											
			·25	10·75		·67											
			·25	11·00		·73											

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Sample between Length of Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimal of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*			*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
											UNWELDED.				MANILLIA.			
20 Sept.	3	1	·25	11·25	59·75	·03	Steel	One	14	·079	·097	—	4	4	1½	·051	148	Continued.
			·25	11·50	·85	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	11·75	·85	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	11·875	·93	·08	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·00	·97	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·125	51·01	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·250	·05	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·375	·11	·06	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·500	·16	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·625	—	—	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·42 Breaking strain = 2·32
29 Sept.	3	1	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	—	—	—	—	—	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·03	·03	,,	,,	,,	,,	,,	—	—	—	—	—	—	Weights put on at intervals of 1½ min.
			1·00	2·50	·09	·06	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			1·00	3·50	·13	·04	,,	,,	,,	,,	,,	—	—	—	—	—	—	Naked steel wire.
			·50	4·00	·16	·03	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·50	4·50	·19	·03	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·50	5·00	·21	·02	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·50	5·50	·24	·03	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·50	6·00	·29	·05	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·50	6·50	·31	·02	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·50	7·00	·39	·03	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·25	7·25	·41	·02	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·25	7·50	·45	·04	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·25	7·75	·50	·05	,,	,,	,,	,,	,,	—	—	—	—	—	—	
			·25	8·00	—	—	,,	,,	,,	,,	,,	—	—	—	—	—	—	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·38 Breaking strain = 1·00
																		The Hemp (Manilla) stripped from the above sample carried a weight of 8·875 cwt.
20 Sept.	3	2	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·051	·148	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·03	·03	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	2·50	·05	·03	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
			1·00	3·50	·10	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	4·50	·13	·03	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	5·50	·18	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
				6·18	·19													
			1·00	6·50	·21	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	7·50	·29	·08	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	8·50	·36	·07	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·50	9·00	·41	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·50	9·50	·48	·07	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·50	10·00	·55	·07	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·50	10·50	·60	·14	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	10·75	·77	·08	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	11·00	·83	·06	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	11·25	·90	·07	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	11·50	51·00	·10	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	11·75	·04	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	11·875	·10	·06	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·00	·16	·06	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·125	·21	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·250	·34	·03	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
																		Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·38 Breaking strain = 2·48
20 Sept.	3	3	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·051	·148	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·02	·02	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	2·50	·07	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
			1·00	3·50	·12	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	4·50	·16	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	5·50	·20	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	6·50	·24	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	7·50	·30	·06	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	8·50	·38	·09	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·50	9·00	·45	·06	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·50	9·50	·52	·07	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·50	10·00	·61	·09	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·50	10·50	·72	·11	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	10·75	·81	·09	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	11·00	·90	·09	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	11·25	·99	·09	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	11·50	51·08	·09	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·25	11·75	·17	·09	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	11·875	·22	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·00	·28	·06	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·125	·31	·03	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·250	·34	·03	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·375	·39	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·50	·42	·03	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·625	·47	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·750	·51	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	12·875	·56	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·125	13·00	—	—	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·48 Breaking strain = 3·12
28 Sept.	3	4	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·051	·148	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·03	·03	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	2·50	·08	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	3·50	·12	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
			1·00	4·50	·17	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	5·50	·21	·04	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
				6·06	·24													
			1·00	6·50	·27	·06	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	7·50	·33	·05	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			1·00	8·50	·42	·10	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	
			·50	9·00	·50	·08	,,	,,	,,	,,	,,		,,	,,	,,	,,	,,	

App. No. 10.

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined.—*continued.*

Date of Experiment.	Number of Specimens.	Number of Samples tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimal of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Wire and Hemp combined. Weight per Fathom.	REMARKS.
1869:			*Cwts.*	*Cwts.*	*Ins.*			*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
									UNWELDED.					MANILLA.				
28 Sept.	3	4	·50	9·50	·89	·10	Steel	One	14	·079	·097	—	4	4	1½	·051	·148	Continued.
			·50	10·00	·72	·13	"	"	"	"	"	"	"	"	"	"	"	
			·50	10·50	·88	·16	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·75	·96	·16	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·00	51·05	·07	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·25	·15	·10	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·50	·33	·08	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·75	·32	·09	"	"	"	"	"	"	"	"	"	"	"	
			·125	11·875	·35	·06	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·00	·43	·05	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·125	—	—	"	"	"	"	"	"	"	"	"	"	"	Broke fairly. Per-centage of elon. ½ Breaking strain = ·48 Breaking strain = 2·96
28 Sept.	3	5	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·051	·148	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·04	·04	"	"	"	"	"	"	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	2·50	·10	·05	"	"	"	"	"	"	"	"	"	"	"	
			1·00	3·50	·16	·06	"	"	"	"	"	"	"	"	"	"	"	
			1·00	4·50	·23	·06	"	"	"	"	"	"	"	"	"	"	"	
			1·00	5·50	·29	·07	"	"	"	"	"	"	"	"	"	"	"	
				6·25	·34													
			1·00	6·50	·36	·07	"	"	"	"	"	"	"	"	"	"	"	
			1·00	7·50	·45	·09	"	"	"	"	"	"	"	"	"	"	"	
			·50	8·00	·51	·06	"	"	"	"	"	"	"	"	"	"	"	
			·50	8·50	·58	·05	"	"	"	"	"	"	"	"	"	"	"	
			·50	9·00	·63	·07	"	"	"	"	"	"	"	"	"	"	"	
			·50	9·50	·71	·08	"	"	"	"	"	"	"	"	"	"	"	
			·50	10·00	·83	·11	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·25	·90	·08	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·50	·97	·07	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·75	76·04	·07	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·00	·12	·08	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·25	·21	·09	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·50	·31	·10	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·75	·40	·09	"	"	"	"	"	"	"	"	"	"	"	
			·25	12·00	·50	·10	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·125	·57	·07	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·250	·62	·05	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·375	·69	·06	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·50	·75	·06	"	"	"	"	"	"	"	"	"	"	"	Broke fr..... Per-centage of elongation :— ½ Breaking strain = ·45 Breaking strain = 2·83
28 Sept.	3	6	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·051	·148	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·04	·04	"	"	"	"	"	"	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	2·50	·07	·03	"	"	"	"	"	"	"	"	"	"	"	
			1·00	3·50	·10	·03	"	"	"	"	"	"	"	"	"	"	"	
			1·00	4·50	·16	·06	"	"	"	"	"	"	"	"	"	"	"	
			1·00	5·50	·22	·06	"	"	"	"	"	"	"	"	"	"	"	
			1·00	6·50	·30	·08	"	"	"	"	"	"	"	"	"	"	"	
			1·00	7·50	·40	·10	"	"	"	"	"	"	"	"	"	"	"	
			·50	8·00	·45	·05	"	"	"	"	"	"	"	"	"	"	"	
			·50	8·50	·53	·07	"	"	"	"	"	"	"	"	"	"	"	
			·50	9·00	·60	·08	"	"	"	"	"	"	"	"	"	"	"	
			·25	9·25	·64	·04	"	"	"	"	"	"	"	"	"	"	"	
			·25	9·50	·70	·06	"	"	"	"	"	"	"	"	"	"	"	
			·25	9·75	·76	·06	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·00	·84	·08	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·25	·90	·06	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·50	·99	·09	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·75	76·10	·11	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·00	·20	·10	"	"	"	"	"	"	"	"	"	"	"	
			·125	11·125	·30	·10	"	"	"	"	"	"	"	"	"	"	"	
			·125	11·250	·36	·06	"	"	"	"	"	"	"	"	"	"	"	
			·126	11·375	·41	·05	"	"	"	"	"	"	"	"	"	"	"	
			·125	11·500	·47	·06	"	"	"	"	"	"	"	"	"	"	"	
			·125	11·625	·53	·06	"	"	"	"	"	"	"	"	"	"	"	
			·125	11·750	·57	·05	"	"	"	"	"	"	"	"	"	"	"	
			·125	11·875	·61	·04	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·00	·67	·06	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·125	·74	·07	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·250	·80	·06	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·375	·86	·06	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·500	·90	·05	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·625	·95	·05	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·75	77·01	·06	"	"	"	"	"	"	"	"	"	"	"	
			·125	12·875	—	—	"	"	"	"	"	"	"	"	"	"	"	
	3	7	11·00	11·00	—	—	"	"	"	"	"	"	"	"	"	"	"	Broke fairly. Put on at once. Wire broke after bearing weight a few seconds. Per-centage of elongation :— ½ Breaking strain = ·40 Breaking strain = 2·68
9 Sept.	4	1	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·052	·149	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·05	·05	"	"	"	"	"	"	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	2·50	·09	·04	"	"	"	"	"	"	"	"	"	"	"	
			1·00	3·50	·13	·04	"	"	"	"	"	"	"	"	"	"	"	
			1·00	4·50	·18	·05	"	"	"	"	"	"	"	"	"	"	"	
			1·00	5·50	·26	·08	"	"	"	"	"	"	"	"	"	"	"	
				5·94	·30													
			1·00	6·50	·35	·09	"	"	"	"	"	"	"	"	"	"	"	
			1·00	7·50	·47	·12	"	"	"	"	"	"	"	"	"	"	"	
			·50	8·00	·56	·09	"	"	"	"	"	"	"	"	"	"	"	
			·50	8·50	·67	·11	"	"	"	"	"	"	"	"	"	"	"	
			·50	9·00	·79	·12	"	"	"	"	"	"	"	"	"	"	"	
			·50	9·50	·88	·10	"	"	"	"	"	"	"	"	"	"	"	
			·50	10·00	51·18	·30	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·25	·32	·07	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·50	·38	·07	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·75	·49	·08	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·00	·59	·10	"	"	"	"	"	"	"	"	"	"	"	

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Sat on.	Weights. Total.	Length of Sample between Stamps.	Elongation.	DESCRIPTION OF SAMPLE. WIRE. Steel or Iron.	WIRE. No. of Wires.	WIRE. Gauge. Birmingham Wire Gauge.	WIRE. Gauge. Decimal of Inch.	WIRE. Weights per Fathom. Single Wire.	WIRE. Weights per Fathom. Strand.	HEMP. No. of Strands. Round each Wire.	HEMP. No. of Strands. Total.	HEMP. Length of Lay.	HEMP. Weight per Fathom.	Weight per Fathom Wire and Hemp combined.	REMARKS.
1862:			*Cwts.*	*Cwts.*	*Ins.*			*No.*	*lbs.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.								MANILLA.			
29 Sept.	4	1	·125	11·125	·54	·04	Steel	One	14	·079	·097	—	4	4	1½	·052	·149	Continued.
			·125	11·250	·57	·03	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·375	·60	·03	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·50	·62	·03	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·625	·65	·03	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·750	·68	·03	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·875	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·60 Breaking strain =3·36
29 Sept.	4	1	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	—	—	—	—	—	Naked steel wire.
			1·00	1·50	·05	·05	"	"	"	"	"	—	—	—	—	—	—	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	2·50	·10	·05	"	"	"	"	"	—	—	—	—	—	—	
			1·03	3·53	·14	·04	"	"	"	"	"	—	—	—	—	—	—	
			·50	4·00	·17	·03	"	"	"	"	"	—	—	—	—	—	—	Weights put on at intervals of 1½ min.
			·50	4·50	·20	·03	"	"	"	"	"	—	—	—	—	—	—	
			·50	5·00	·23	·03	"	"	"	"	"	—	—	—	—	—	—	
			·30	5·30	·26	·03	"	"	"	"	"	—	—	—	—	—	—	
			·50	6·00	·31	·05	"	"	"	"	"	—	—	—	—	—	—	
			·50	6·50	·35	·07	"	"	"	"	"	—	—	—	—	—	—	
			·50	7·00	·48	·13	"	"	"	"	"	—	—	—	—	—	—	
			·25	7·25	·53	·05	"	"	"	"	"	—	—	—	—	—	—	
			·25	7·50	·63	·10	"	"	"	"	"	—	—	—	—	—	—	
			·25	7·75	·70	·07	"	"	"	"	"	—	—	—	—	—	—	
			·25	8·00	—	—	"	"	"	"	"	—	—	—	—	—	—	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·34 Breaking strain =1·40
29 Sept.	4	2	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·052	·149	Sent from Messrs. R. S. Newall & Co., Birkenhead.
			1·00	1·50	·07	·07	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50	·14	·07	"	"	"	"	"	—	"	"	"	"	"	
			1·00	3·50	·22	·08	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	4·50	·31	·09	"	"	"	"	"	—	"	"	"	"	"	
			1·00	5·50	·40	0·9	"	"	"	"	"	—	"	"	"	"	"	
				5·94	·46													
			1·00	6·50	·53	·13	"	"	"	"	"	—	"	"	"	"	"	
			1·00	7·50	·72	·19	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·00	·84	·12	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·50	76·02	·18	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·00	·22	·20	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·50	·43	·21	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·00	·65	·23	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·25	·79	·14	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·50	·90	·11	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·75	77·02	·12	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·00	·11	·09	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·125	·16	·07	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·250	·24	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·375	·29	·05	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·500	·33	·03	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·625	·38	·05	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·750	·42	·04	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·875	·49	·07	"	"	"	"	"	—	"	"	"	"	"	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·81 Breaking strain =3·32
29 Sept.	4	3	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·052	·149	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·07	·07	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	2·50	·14	·07	"	"	"	"	"	—	"	"	"	"	"	
			1·00	3·50	·22	·08	"	"	"	"	"	—	"	"	"	"	"	
			1·00	4·50	·31	·09	"	"	"	"	"	—	"	"	"	"	"	
			1·00	5·50	·40	·09	"	"	"	"	"	—	"	"	"	"	"	
				5·75	·44													
			1·00	6·50	·53	·13	"	"	"	"	"	—	"	"	"	"	"	
			1·00	7·50	·72	·19	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·00	·86	·14	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·50	78·01	·15	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·00	·23	·23	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·50	·46	·23	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·00	·71	·25	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·25	·85	·14	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·50	·98	·13	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·75	77·12	·14	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·00	·32	·10	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·125	·31	·09	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·250	·37	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·375	·44	·08	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·500	·52	·07	"	"	"	"	"	—	"	"	"	"	"	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·59 Breaking strain =3·36
															RUSSIAN.			
21 Sept.	5	1	·50	·50	50·00	·00	Steel	One	13	·079	·097	—	4	4	1	·077	·174	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·02	·02	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50	·08	·06	"	"	"	"	"	—	"	"	"	"	"	
			1·00	3·50	·11	·03	"	"	"	"	"	—	"	"	"	"	"	
			1·00	4·50	·16	·05	"	"	"	"	"	—	"	"	"	"	"	
				5·00	·18													
			1·00	5·50	·20	·04	"	"	"	"	"	—	"	"	"	"	"	
			1·00	6·50	·27	·07	"	"	"	"	"	—	"	"	"	"	"	
			1·00	7·50	·36	·09	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	8·50	·50	·14	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·00	·55	·15	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·50	51·00	·25	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·00	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke while the last weight was being added. Per-centage of elongation :— ½ Breaking strain = ·36 Breaking strain = 1·00

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued*.

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued*.

Date of Experiment.	Number of Specimen.	Number of Samples tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimal of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						RUSSIAN.					
21 Sept.	5	2	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	¾	0·77	·174	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·03												
			1·00	2·50		·05												
			1·00	3·50		·12												Weights put on at intervals of 1½ minutes.
			1·00	4·50		·07												
				4·75		·18												
			1·00	5·50		·23												
			1·00	6·50		·30												
				7·00		·25												
				7·50		·40												
				8·00		·45												
				8·50		·55												
				8·75		·63												
				9·00		·76												
				9·25		·90												
				9·50	—	·14												Broke fair. Per-centage of elongation:— ½ Breaking strain = ·37 Breaking strain = 1·89
21 Sept.	5	3.	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	¾	·077	·174	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·03												
			1·00	2·50		·11												
			1·00	3·50		·04												Weights put on at intervals of 1½ minutes.
			1·00	4·50		·21												
				4·75		·23												
			1·00	5·50		·27												
			1·00	6·50		·32												
				7·00		·35												
				7·50		·43												
				8·00		·40												
				8·50		·55												
				8·75		·65												
				9·00		·76												
				9·25		·92												
			·125	9·375	51·09	·17												
			·125	9·500	—	—												Broke fair. Per-centage of elongation:— ½ Breaking strain = ·45 Breaking strain = 2·18
23 Sept.	6	1	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1	·050	·147	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·03												
			1·00	2·50		·05												
			1·00	3·50		·13												
			1·00	4·50		·18												Weights put on at intervals of 1½ minutes.
			1·00	5·50		·24												
				5·81		·25												
			1·00	6·50		·30												
			1·00	7·50		·39												
				8·00		·46												
			·50	8·50		·51												
			·50	9·00		·61												
			·25	9·25		·69												
			·25	9·50		·79												
			·25	9·75		·88												
			·25	10·00		·97												
			·25	10·25	51·09	·12												
			·25	10·50		·18												
			·125	10·625		·24												
			·125	10·750		·28												
			·125	10·875		·38												
			·125	11·000		·53												
			·125	11·125		·49												
			·125	11·250		·53												
			·125	11·375		·58												
			·125	11·500		·63												
			·125	11·625	—	—												Broke fair. Per-centage of elongation:— ½ Breaking strain = ·51 Breaking strain = 3·28
23 Sept.	6	2	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1	·050	·147	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·03												
			1·00	2·50		·08												
			1·00	3·50		·13												
			1·00	4·50		·18												Weights put on at intervals of 1½ minutes.
			1·00	5·50		·25												
				5·62		·24												
			1·00	6·50		·29												
			1·00	7·50		·38												
			·50	8·00		·40												
			·50	8·50		·51												
			·50	9·00		·61												
			·25	9·25		·70												
			·25	9·50		·79												
			·25	9·75		·89												
			·25	10·00	51·09	·11												
			·25	10·25		·11												
			·25	10·50		·23												
			·125	10·625		·30												
			·125	10·750		·37												
			·125	10·875		·43												
			·125	11·000		·49												
			·125	11·125		·54												
			·125	11·250	—	—												Broke fair. Per-centage of elongation:— ½ Breaking strain = ·48 Breaking strain = 3·09

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded) and Hemp Strands, Separate and Combined—*continued.*

Date of Experiment.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimal of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Wire and Hemp combined. Weight per Fathom.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						RUSSIAN.					
27 Sept.	6	3	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1	·050	·147	Sent from Messrs. R. S. Newall and Co., Birkenhead
			1·00	1·50		·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50		·12	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	3·50		·18	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	4·50		·26	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
				4·31		·23												
			1·00	5·50		·34	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weight put on at intervals of 1½ min.
			1·00	6·50		·44	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	7·50		·55	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·00		·63	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	8·50		·76	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	9·00		·95	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	9·25	76·04	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	9·50		·20	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	9·75		·36	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	10·00		·53	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	10·25		·73	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	10·50		·90	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	10·625		·99	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Broke fair. Per-centage of elongation:— ¼ Breaking strain = ·44 Breaking strain = 2·66

RESULTS of EXPERIMENTS made by Messrs. GISBORNE and FORDE and C. W. SIEMENS, for the purpose of arriving at the best form for OUTER COVERING of CABLES.

Date of Experiment.	Description of Specimen, and Sample tested.	No.	Time. H.M.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:				*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
7 Sept.	No. 14. (Gauge Steel Wires). Diameter of Wire ·079 inch. 1st Sample tested.	6-4	—	·50	—	·50	59·00	·00	—				—	—	—	
			—	1·00	—	1·50		·03	—				—	—	—	
			—	1·00	—	2·50		·07	—				—	—	—	
			—	1·00	—	3·50		·11	—				—	—	—	
						4·25		·15								
			—	1·00	—	4·50		·16	—				—	—	—	
			—	·50	—	5·00		·19	—				—	—	—	
			—	·50	—	5·50		·22	—				—	—	—	
			—	·50	—	6·00		·26	—				—	—	—	
			—	·50	—	6·50		·30	—				—	—	—	
			—	·25	—	6·75		·32	—				—	—	—	
			—	·25	—	7·00		·35	—				—	—	—	
			—	·25	—	7·25		·38	—				—	—	—	
			—	·25	—	7·50		·41	—				—	—	—	
			—	·25	—	7·75		·45	—				—	—	—	
			—	·125	—	7·875		·49	—				—	—	—	
			—	·125	—	8·000		·52	—				—	—	—	
			—	·125	—	8·1250		·55	—				—	—	—	
			—	·125	—	8·2500		·61	—				—	—	—	
			—	·125	—	8·3750		·71	—				—	—	—	
			—	·0625	—	8·4375		·76	—				—	—	—	
			—	·0625	—	8·50		·80	—				—	—	—	Broke fair immediately the elongation was taken. Per-centage of elongation:— ¼ Breaking strain = ·30 Breaking strain = 1·50

App. No. 10.

Appendix No. 10.—Falmouth and Gibraltar Telegraph—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires, (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimal of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. each Round Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Wire and Hemp. Weight per Fathom, Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						RUSSIAN.					
8 Sept.	7	1	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1¼	·055	·152	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·04	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50		·08	"	"	"	"	"	—	"	"	"	"	"	
			1·00	3·50		·13	"	"	"	"	"	—	"	"	"	"	"	
			1·00	4·50		·17	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
				5·44		·23												
			1·00	6·50		·27	"	"	"	"	"	—	"	"	"	"	"	
			·50	6·00		·29	"	"	"	"	"	—	"	"	"	"	"	
			·50	6·50		·31	"	"	"	"	"	—	"	"	"	"	"	This sample was steeped in water for 22 hours.
			·50	7·00		·33	"	"	"	"	"	—	"	"	"	"	"	
			·50	7·50		·35	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·00		·43	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·50		·55	"	"	"	"	"	—	"	"	"	"	"	
			·25	8·75		·57	"	"	"	"	"	—	"	"	"	"	"	
			·25	9·00		·62	"	"	"	"	"	—	"	"	"	"	"	
			·25	9·25		·71	"	"	"	"	"	—	"	"	"	"	"	
			·25	9·50		·80	"	"	"	"	"	—	"	"	"	"	"	
			·25	9·75		·90	"	"	"	"	"	—	"	"	"	"	"	
			·125	9·875		·95	"	"	"	"	"	—	"	"	"	"	"	
			·125	10·00	51·02	·97	"	"	"	"	"	—	"	"	"	"	"	
			·125	10·125		·07	"	"	"	"	"	—	"	"	"	"	"	
			·125	10·250		·17	"	"	"	"	"	—	"	"	"	"	"	
			·125	10·375		·19	"	"	"	"	"	—	"	"	"	"	"	
			·125	10·500		·24	"	"	"	"	"	—	"	"	"	"	"	
			·125	10·625		·30	"	"	"	"	"	—	"	"	"	"	"	
			·125	10·750		·35	"	"	"	"	"	—	"	"	"	"	"	
			·125	10·875	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fairly.
													Breaking strain of Russian Hemp, 2 cwt. ·825.					Per-centage of elongation :— ¼ Breaking strain = ·45 Breaking strain = 2·70
8 Sept.	7	1	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·03	"	"	"	"	"	—	—	—	—	—	—	
			1·00	2·50		·07	"	"	"	"	"	—	—	—	—	—	—	
			1·00	3·50		·11	"	"	"	"	"	—	—	—	—	—	—	
				4·25		·15												
			1·00	4·50		·16	"	"	"	"	"	—	—	—	—	—	—	
			·50	5·00		·19	"	"	"	"	"	—	—	—	—	—	—	
			·50	5·50		·22	"	"	"	"	"	—	—	—	—	—	—	
			·50	6·00		·26	"	"	"	"	"	—	—	—	—	—	—	
			·50	6·50		·30	"	"	"	"	"	—	—	—	—	—	—	
			·25	6·75		·32	"	"	"	"	"	—	—	—	—	—	—	
			·25	7·00		·35	"	"	"	"	"	—	—	—	—	—	—	
			·25	7·25		·38	"	"	"	"	"	—	—	—	—	—	—	
			·25	7·50		·41	"	"	"	"	"	—	—	—	—	—	—	
			·25	7·75		·45	"	"	"	"	"	—	—	—	—	—	—	
			·125	7·875		·49	"	"	"	"	"	—	—	—	—	—	—	
			·125	8·000		·52	"	"	"	"	"	—	—	—	—	—	—	
			·125	8·125		·55	"	"	"	"	"	—	—	—	—	—	—	
			·125	8·250		·61	"	"	"	"	"	—	—	—	—	—	—	
			·125	8·375		·71	"	"	"	"	"	—	—	—	—	—	—	
			·125	8·500		·90	"	"	"	"	"	—	—	—	—	—	—	Broke fair immediately elongation was taken. Per-centage of elongation :— ¼ Breaking strain = ·36 Breaking strain = 1·44
8 Sept.	7	2	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1¼	·055	·152	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·02	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50		·05	"	"	"	"	"	—	"	"	"	"	"	
			1·00	3·50		·10	"	"	"	"	"	—	"	"	"	"	"	
			1·00	4·50		·16	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	5·50		·22	"	"	"	"	"	—	"	"	"	"	"	
				5·76		·23												
			1·00	6·50		·29	"	"	"	"	"	—	"	"	"	"	"	Steeped in water 22 hours.
			1·00	7·50		·37	"	"	"	"	"	—	"	"	"	"	"	
			1·00	8·50		·47	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·00		·54	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·50		·61	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·00		·80	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·50		·90	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·75	51·00	·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·00		·12	"	"	"	"	"	—	"	"	"	"	"	
			11·25			·23	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·50	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ¼ Breaking strain = ·36 Breaking strain = 2·46
20 Sept.	7	3	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1¼	·055	·152	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·03	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50		·08	"	"	"	"	"	—	"	"	"	"	"	
			1·00	3·50		·13	"	"	"	"	"	—	"	"	"	"	"	
			1·00	4·50		·18	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			·00	5·50		·24	"	"	"	"	"	—	"	"	"	"	"	
				5·88		·25												
			1·00	6·50		·30	"	"	"	"	"	—	"	"	"	"	"	
			1·00	7·50		·37	"	"	"	"	"	—	"	"	"	"	"	
			1·00	8·50		·44	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·00		·51	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·50		·58	"	"	"	"	"	—	"	"	"	"	"	
			10·00	10·00		·70	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·50		·85	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·75		·93	"	"	"	"	"	—	"	"	"	"	"	
			11·00	11·00	51·09	·08	"	"	"	"	"	—	"	"	"	"	"	
			11·25			·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·50		·21	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·75	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ¼ Breaking strain = ·60 Breaking strain = 2·42

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights Put on.	Weights Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimal of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*		*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						RUSSIAN.					
29 Sept.	7	4	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·055	·152	Sent from Messrs. R. S. Newall and Co, Birkenhead.
			1·00	1·50		·03	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50		·05	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	3·50		·11	"	"	"	"	"	—	"	"	"	"	"	
			1·00	4·50		·16	"	"	"	"	"	—	"	"	"	"	"	
			1·00	5·50		·20	"	"	"	"	"	—	"	"	"	"	"	
				5·88		·24	"	"	"	"	"	—	"	"	"	"	"	Hemp soaked in water 8 hours.
			1·00	6·50		·26	"	"	"	"	"	—	"	"	"	"	"	
			1·00	7·50		·32	"	"	"	"	"	—	"	"	"	"	"	
			1·00	8·50		·42	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·00		·52	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·50		·62	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·00		·71	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·50		·79	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·75		·88	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·00		·99	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·25	51·00	·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·50		·19	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·75	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ¼ Breaking strain = ·49 Breaking strain = 2·38
29 Sept.	7	5	·50	·50	50·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·055	·152	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·02	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50		·04	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	3·50		·09	"	"	"	"	"	—	"	"	"	"	"	
			1·00	4·50		·13	"	"	"	"	"	—	"	"	"	"	"	
			1·00	5·50		·18	"	"	"	"	"	—	"	"	"	"	"	This sample was soaked in water 38 hours.
				5·62	—	—												
			1·00	6·50		·22	"	"	"	"	"	—	"	"	"	"	"	
			1·00	7·50		·31	"	"	"	"	"	—	"	"	"	"	"	
			1·00	8·50		·39	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·00		·50	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·50		·58	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·00		·72	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·50		·93	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·75	51·03	·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·00		·14	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·25	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ¼ Breaking strain = ·37 Breaking strain = 2·38
29 Sept.	7	6	11·00	11·00		·00	Steel	One	14	·079	·097	—	4	4	1½	·055	·152	Sent from Messrs. R. S. Newall and Co., Birkenhead.
																		Weights put on without stopping the usual interval of 1½ min.
29 Sept.	8	1	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·050	·147	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·05	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50		·11	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	3·50		·18	"	"	"	"	"	—	"	"	"	"	"	
			1·00	4·50		·23	"	"	"	"	"	—	"	"	"	"	"	
			1·00	5·50		·33	"	"	"	"	"	—	"	"	"	"	"	
				5·93		·37												
			1·00	6·50		·41	"	"	"	"	"	—	"	"	"	"	"	
			1·00	7·50		·53	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·00		·60	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·50		·68	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·00		·78	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·50		·96	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·00	76·03	·12	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·25		·13	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·50		·20	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·75		·30	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·00		·40	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·125		·48	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·250		·52	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·375		·58	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·500		·61	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·625		·68	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·750		·75	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·875	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ¼ Breaking strain = ·49 Breaking strain = 2·32
29 Sept.	8	2	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·050	·147	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·05	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50		·10	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	3·50		·16	"	"	"	"	"	—	"	"	"	"	"	
			1·00	4·50		·22	"	"	"	"	"	—	"	"	"	"	"	
			1·00	5·50		·30	"	"	"	"	"	—	"	"	"	"	"	
				5·69		·31												
			1·00	6·50		·39	"	"	"	"	"	—	"	"	"	"	"	
			1·00	7·50		·50	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·00		·55	"	"	"	"	"	—	"	"	"	"	"	
			·50	8·50		·61	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·00		·71	"	"	"	"	"	—	"	"	"	"	"	
			·50	9·50		·83	"	"	"	"	"	—	"	"	"	"	"	
			·50	10·00		·95	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·25	76·03	·08	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·50		·11	"	"	"	"	"	—	"	"	"	"	"	
			·25	10·75		·20	"	"	"	"	"	—	"	"	"	"	"	
			·25	11·00		·30	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·125		·38	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·250		·44	"	"	"	"	"	—	"	"	"	"	"	
			·125	11·375	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ¼ Breaking strain = ·42 Breaking strain = 1·93

Results of Experiments made by Messrs Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued*.

Date of Experiments.	Number of Specimen.	Number of Samples tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimal of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859	—	—	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
											UNWELDED.					RUSSIAN.		
29 Sept.	8	3	·50	·50	75·00	·00	Steel	One	14	·079	·097	—	4	4	1½	·050	·147	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·05	"	"	"	"	"	"	"	"	"	"	"	
			1·00	2·50		·10	"	"	"	"	"	"	"	"	"	"	"	
			1·00	3·50		·17	"	"	"	"	"	"	"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	4·50		·23	"	"	"	"	"	"	"	"	"	"	"	
			1·00	5·50		·31	"	"	"	"	"	"	"	"	"	"	"	
				5·62		·33	"	"	"	"	"	"	"	"	"	"	"	
			1·00	6·50		·40	"	"	"	"	"	"	"	"	"	"	"	
			1·00	7·50		·51	"	"	"	"	"	"	"	"	"	"	"	
			·50	8·00		·57	"	"	"	"	"	"	"	"	"	"	"	
			·50	8·50		·64	"	"	"	"	"	"	"	"	"	"	"	
			·50	9·00		·73	"	"	"	"	"	"	"	"	"	"	"	
			·50	9·50		·83	"	"	"	"	"	"	"	"	"	"	"	
			·50	10·00		·96	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·25	76·06	·03	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·50		·15	"	"	"	"	"	"	"	"	"	"	"	
			·25	10·75		·24	"	"	"	"	"	"	"	"	"	"	"	
			·25	11·00		·32	"	"	"	"	"	"	"	"	"	"	"	
			·125	11·125		·20	"	"	"	"	"	"	"	"	"	"	"	
			·125	11·250		·07	"	"	"	"	"	"	"	"	"	"	"	
																		Broke fair. Per-centage of elongation:— ½ Breaking strain = ·83 Breaking strain = 1·85
29 Sept.	9	1	·50	·50	75·00	·00		One	14	·085	Not taken	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·04		"	"	"	"	"						Sent from Messrs. B. Johnson's, Manchester.
			1·00	2·50		·11		"	"	"	"	"						
			1·00	3·50		·19		"	"	"	"	"						Weights put on at intervals of 1½ min.
				4·38		·25												
			1·00	4·50		·27		"	"	"	"	"						
			1·00	5·50		·35		"	"	"	"	"						
			1·00	6·50	75·47	·13		"	"	"	"	"						
			1·00	7·50		·62		"	"	"	"	"						
			·50	8·00		·12		"	"	"	"	"						
			·50	8·50		·20		"	"	"	"	"						
			·25	8·75		—		"	"	"	"	"						Broke fair. Per-centage of elongation:— ½ Breaking strain = ·35 Breaking strain = 1·32
29 Sept.	9	2	·50	·50	75·02	·00		One	14	·085	Not taken	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·05		"	"	"	"	"						Sent from Messrs. B. Johnson's, Manchester.
			1·00	2·50		·10		"	"	"	"	"						
			1·00	3·50		·15		"	"	"	"	"						Weights put on at intervals of 1½ min.
				4·19		·19												
			1·00	4·50		·23		"	"	"	"	"						
			·50	5·00		·27		"	"	"	"	"						
			·50	5·50		·32		"	"	"	"	"						
			·50	6·00		·37		"	"	"	"	"						
			·50	6·50		·43		"	"	"	"	"						
			·25	6·75		·47		"	"	"	"	"						
			·25	7·00		·51		"	"	"	"	"						
			·25	7·25		·55		"	"	"	"	"						
			·25	7·50		·61		"	"	"	"	"						
			·25	7·75		·67		"	"	"	"	"						
			·25	8·00		·76		"	"	"	"	"						
			·25	8·25		·91		"	"	"	"	"						
			·125	8·375		·10		"	"	"	"	"						Broke fair. Per-centage of elongation:— ½ Breaking strain = ·26 Breaking strain = 1·35
30 Sept.	9	3	·50	·50	75·00	·00		One	14	·085	Not taken	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·05		"	"	"	"	"						Sent from Messrs. B. Johnson's, Manchester.
			1·00	2·50		·11		"	"	"	"	"						
			1·00	3·50		·17		"	"	"	"	"						Weights put on at intervals of 1½ min.
				4·44		·24												
			1·00	4·50		·24		"	"	"	"	"						
			·50	5·00		·29		"	"	"	"	"						
			·50	5·50		·32		"	"	"	"	"						
			·50	6·00		·37		"	"	"	"	"						
			·50	6·50		·42		"	"	"	"	"						
			·25	6·75		·46		"	"	"	"	"						
			·25	7·00		·50		"	"	"	"	"						
			·25	7·25		·54		"	"	"	"	"						
			·25	7·50		·59		"	"	"	"	"						
			·25	7·75		·64		"	"	"	"	"						
			·25	8·00		·70		"	"	"	"	"						
			·125	8·125		·74		"	"	"	"	"						
			·125	8·250		·79		"	"	"	"	"						
			·125	8·375		·84		"	"	"	"	"						
			·125	8·500		·90		"	"	"	"	"						
			·125	8·625		·96		"	"	"	"	"						
			·125	8·750	76·14	·16		"	"	"	"	"						
			·125	8·875		—		"	"	"	"	"						Broke fair. Per-centage of elongation:— ½ Breaking strain = ·32 Breaking strain = 1·32
30 Sept.	10	1	·50	·50	75·00	·00		One	14	·082	Not taken	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·05		"	"	"	"	"						Sent from Messrs. B. Johnson's, Manchester.
			1·00	2·50		·11		"	"	"	"	"						
			1·00	3·50		·17		"	"	"	"	"						Weights put on at intervals of 1½ min.
				3·62		·18												
			1·00	4·50		·26		"	"	"	"	"						
			·50	5·00		·31		"	"	"	"	"						
			·50	5·50		·36		"	"	"	"	"						
			·50	6·00		·42		"	"	"	"	"						
			·50	6·50		·52		"	"	"	"	"						
			·25	6·75		·57		"	"	"	"	"						
			·25	7·00		·62		"	"	"	"	"						
			·25	7·25		·69		"	"	"	"	"						Broke fairly. Per-centage of elongation:— ½ Breaking strain = ·24 Breaking strain = ·92

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued*. APP. No.

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires, (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued*.

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	No. of Wires.	Gauge. Birmingham Wire Gauge.	Gauge. Decimal of Inch.	Weight per Fathom. Single Wire.	Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							NAKED WIRES.—UNWELDED.											
30 Sept.	10	2	·50	·50	75·00	·09	Patent tempered Cast Steel.	One	·14	·082	Not taken	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·05		"	"	"	"	—	—	—	—	—	—	
			1·00	2·50		·11		"	"	"	"	—	—	—	—	—	—	Sent from Messrs. B. Johnson's, Manchester.
			1·00	3·50		·17		"	"	"	"	—	—	—	—	—	—	
				3·65														
			1·00	4·50		·24		"	"	"	"	—	—	—	—	—	—	Weights put on at intervals o 1½ min.
			·50	5·00		·30		"	"	"	"	—	—	—	—	—	—	
			·50	5·50		·35		"	"	"	"	—	—	—	—	—	—	
			·50	6·00		·40		"	"	"	"	—	—	—	—	—	—	
			·25	6·25		·44		"	"	"	"	—	—	—	—	—	—	
			·25	6·50		·49		"	"	"	"	—	—	—	—	—	—	
			·25	6·75		·54		"	"	"	"	—	—	—	—	—	—	
			·25	7·00		·61		"	"	"	"	—	—	—	—	—	—	
			·125	7·125		·87		"	"	"	"	—	—	—	—	—	—	Broke fairly. Per-centage of elongation:— ¼ Breaking strain = ·22 Breaking strain = ·81
30 Sept.	10	3	·50	·50	75·00	·09	Patent tempered Cast Steel.	One	·14	·082	Not taken	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·04		"	"	"	"	—	—	—	—	—	—	
			1·00	2·50		·09		"	"	"	"	—	—	—	—	—	—	Sent from Messrs. B. Johnson's, Manchester.
				3·38														
			1·00	3·50		·14		"	"	"	"	—	—	—	—	—	—	
			1·00	4·50		·20		"	"	"	"	—	—	—	—	—	—	Weights put on at intervals of 1½ min.
			·50	5·00		·24		"	"	"	"	—	—	—	—	—	—	
			·50	5·50		·29		"	"	"	"	—	—	—	—	—	—	
			·50	6·00		·36		"	"	"	"	—	—	—	—	—	—	
			·25	6·25		·42		"	"	"	"	—	—	—	—	—	—	
			·25	6·50		·50		"	"	"	"	—	—	—	—	—	—	
			·25	6·75		·67		"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ¼ Breaking strain = ·18 Breaking strain = ·67

App. No. 10. APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

TESTS for STRENGTH of SINGLE IRON and STEEL WIRES, both WELDED and UNWELDED, made by H. C. FORDE and C. W. SIEMENS.

No.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	Description of Sample and Remarks.
	Cwts.	*Cwts.*	*Ins.*	*Ins.*	
		Tested 2d July.			
11-1	2·00	2·00	56·00	·00	Naked wire.
		3·00	·05		Labelled Best Steel. No. 1. R. S. Newall and Co. Not welded. Birmingham, 14.
	2·00	4·00	·36	·06	Broke fairly.
	2·00	6·00	·21	·15	Per-centage of elongation :— ½ Breaking strain = ·06 Breaking strain = ·42
11-2	2·00	2·00	56·00	·00	Naked wire.
	1·00	3·00	·04	·04	Labelled Best Steel, No. 2. R. S. Newall and Co. Not welded.
	1·00	4·00	·10	·06	
	1·00	5·00	·18	·08	
	·50	5·50	·23	·05	
	·50	6·00	·40	·17	
	·25	—	—	—	Broke fairly. ·25 just touched the scales. Per-centage of elongation :— ½ Breaking strain = ·08 Breaking strain = ·80
11-3	2·00	2·00	50·00	·00	Naked wire.
	1·00	3·00	·05	·05	Labelled No. 3. Best Steel. R. S. Newall and Co. Not welded.
		3·06			
	1·00	4·00	·11	·06	
	1·00	5·00	·20	·09	
	·50	5·50	·25	·05	
	·25	5·75	·31	·06	
	·25	6·00	·39	·08	
	·125	6·125	—	—	Broke fairly. Per-centage of elongation :— ¼ Breaking strain = ·10 Breaking strain = ·78
		Tested 28th June.			
12-1	1·50	1·50	50·00	·00	Naked wire.
	·50	2·00	·00	·00	Sample from R. S. Newall and Co. Marked Steel Wire, No. 14 Gauge. (Light 14 Gauge as measured). Not welded.
	·50	2·50	·02	·02	
	·50	3·00	·04	·02	
	·50	3·50	·06	·02	
	·50	4·00	·09	·03	
	·50	4·50	·11	·02	
	·50	5·00	·13	·02	
	·50	5·50	·15	·02	
	·25	5·75	·17	·02	
	·25	6·00	·19	·02	
	·25	6·25	·20	·01	
	·25	6·50	·21	·01	
	·25	6·75	·22	·01	
	·25	7·00	·23	·01	
	·25	7·25	·24	·01	
	·25	7·50	·24	·00	
	·25	7·75	·25	·01	
	·25	8·00	·26	·01	Broke at thimble, but not fairly. Per-centage of elongation :— ¼ Breaking strain = ·13 Breaking strain = ·52
		Sample tested.			
Specimen 12-1	1·00	1·00	50·00	·00	Naked wire.
	·50	1·50	·00	·00	Same Sample. Welded. Cut off same piece.
	·25	1·75	·02	·02	
		2·13	·04		
	·50	2·25	·05	·03	
	·50	2·75	·07	·02	
	·25	3·00	·09	·02	
	·25	3·25	·10	·01	
	·25	3·50	·12	·02	
	·25	3·75	·14	·02	
	·25	4·00	·17	·03	
	·25	4·25	—	—	Broke at the weld. Per-centage of elongation :— ¼ Breaking strain = ·08 Breaking strain = ·34
		Tested 29th June.			
13-1	1·00	1·00	50·00	·00	Naked wire.
	1·00	2·00	·03	·03	No. 14 Canvas. Labelled Steel Wire, 14 Gauge. Not welded.
	1·00	3·00	·07	·04	Sent from R. S. Newall and Co.
	1·00	4·00	·10	·03	
		4·69	·13		
	1·00	5·00	·14	·04	Best Steel Wire.

No.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	Description of Sample and Remarks.
	Cwts.	*Cwts.*	*Ins.*	*Ins.*	
	1·00	6·00	·19	·05	Continued.
	·50	6·50	·21	·02	
	·50	7·00	·24	·03	
	·50	7·50	·28	·04	
	·50	8·00	·32	·04	
	·25	8·25	·35	·03	
	·25	8·50	·38	·03	
	·25	8·75	·41	·03	
	·25	9·00	·47	·06	
	·25	9·25	·65	·18	
	·125	9·375	—	—	Fair break. Per-centage of elongation :— ½ Breaking strain = ·26 Breaking strain = 1·80
13-2	1·00	1·00	50·00	·00	Naked wire.
	2·00	3·00	·06	·06	No. 14 Gauge. Not welded.
	1·00	4·00	·10	·04	
		4·25	·11		
	1·00	5·00	·13	·03	
	1·00	6·00	·18	·05	
	·50	6·50	·20	·02	
	·50	7·00	·23	·03	
	·50	7·50	·25	·02	
	·50	8·00	·30	·05	
	·50	8·50	·34	·04	Broke quite fair. Per-centage of elongation :— ½ Breaking strain = ·22 Breaking strain = ·88
13-2	1·00	1·00	50·00	·00	Naked wire.
	1·00	2·00	·01	·01	Cut off the same. Welded.
	1·00	3·00	·03	·02	
	·50	3·50	·00	·03	
	·50	4·00	·15	·09	Breaking weight.
	·25	4·25	—	—	Broke instantly. The weight was put on (at the weld), not from any imperfection at the weld, but from a less sectional area of steel. Per-centage of elongation :— ¼ Breaking strain = ·02 Breaking strain = ·30
		Tested.			
13-3	1·00	1·00	50·00	·00	Naked wire.
	1·00	2·00	·04	·04	No. 14 Gauge. Canvas. Labelled, Not welded. Steel Wire.
	1·00	3·00	·06	·02	
	1·00	4·00	·10	·04	
		4·68	·13		
	1·00	5·00	·14	·04	
	1·00	6·00	·16	·02	
	1·00	7·00	·20	·04	
	·50	7·50	·24	·04	
	·50	8·00	·28	·04	
	·25	8·25	·30	·02	
	·25	8·50	·32	·02	
	·25	8·75	·34	·02	
	·25	9·00	·40	·06	
	·25	9·25	·46	·06	
	·125	9·375	—	—	Per-centage of elongation :— ¼ Breaking strain = ·26 Breaking strain = ·92
13-4	2·50	2·50	50·00	·00	Naked wire.
	2·00	4·50	·09	·09	No. 14 Gauge. Cut off the same piece. Not welded. Steel Wire.
		4·68	·11		
	2·00	6·50	·18	·09	
	2·00	8·50	·31	·13	
	·25	8·75	·33	·01	
	·25	9·00	·34	·02	
	·25	9·25	·38	·04	
	·125	9·375	·41	·03	
	·125	9·50	·43	·02	
	·125	9·625	·47	·04	
	·125	9·750	·51	·04	
	·125	9·875	·61	·10	Broke fair. Per-centage of elongation :— ¼ Breaking strain = ·22 Breaking strain = 1·22

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.* APP. No. 10.

RESULTS of EXPERIMENTS made by Messrs. GISBORNE and FORDE and C. W. SIEMENS, upon the Strength of STEEL and IRON WIRES (WELDED and UNWELDED), and HEMP STRANDS, SEPARATE and COMBINED.

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. WIRE. Steel or Iron.	WIRE. No. of Wires.	WIRE. Gauge. Birmingham Wire Gauge.	WIRE. Gauge. Decimal of Inch.	WIRE. Weight per Fathom. Single Wire.	WIRE. Weight per Fathom. Strand.	HEMP. No. of Strands. Round each Wire.	HEMP. No. of Strands. Total.	HEMP. Length of Lay.	HEMP. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						MANILLA.					
23 Sept.	14	1	·50	·50	75·00	·00	Iron	One	14	·079	·0961	—	4	4	¾	·077	·1731	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·07	·07	"	"	"	"	"	—	"	"	"	"	"	
				2·25	·13													
			1·00	3·25	·15	·08	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ minutes.
			·50	3·75	·21	·06	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·59	·26	·05	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·00	·35	·09	"	"	"	"	"	—	"	"	"	"	"	Broke fair.
			·50	4·50	—	—	"	"	"	"	"	—	"	"	"	"	"	Per-centage of elongation :— ¼ Breaking strain = ·17 Breaking strain = ·47
23 Sept.	14	2	·50	·50	75·00	·00	Iron	One	14	·079	·0961	—	4	4	¾	·077	·1731	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·00	1·50	·08	·08	"	"	"	"	"	—	"	"	"	"	"	
				2·37	·18													
			1·00	3·50	·19	·11	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ minutes.
			·50	4·00	·23	·04	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·50	·31	·08	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·00	·39	·08	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·50	·59	·20	"	"	"	"	"	—	"	"	"	"	"	Broke fairly.
			·25	4·75	—	—	"	"	"	"	"	—	"	"	"	"	"	Per-centage of elongation :— ¼ Breaking strain = ·24 Breaking strain = ·79
					×4.													
23 Sept.	14	1	·50	·50	50·00	·00	Iron	One	14	·079	·0961	—	—	—	—	—	—	Naked wire.
			·50	1·00	·02	·02	"	"	"	"	"	—	—	—	—	—	—	
			·50	1·50	·04	·02	"	"	"	"	"	—	—	—	—	—	—	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·50	2·00	·07	·03	"	"	"	"	"	—	—	—	—	—	—	
				2·20														
			·50	2·50	·09	·02	"	"	"	"	"	—	—	—	—	—	—	Weights put on at intervals of 1½ min.
			·50	3·00	·11	·02	"	"	"	"	"	—	—	—	—	—	—	
			·50	3·50	·14	·03	"	"	"	"	"	—	—	—	—	—	—	
			·25	3·75	·17	·03	"	"	"	"	"	—	—	—	—	—	—	
			·25	4·00	·19	·02	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·125	·21	·02	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·250	·23	·02	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·375	·28	·05	"	"	"	"	"	—	—	—	—	—	—	
			·0625	4·4375	—	—	"	"	"	"	"	—	—	—	—	—	—	Broke fairly. Per-centage of elongation :— ¼ Breaking strain = ·14 Breaking strain = ·36
23 Sept.	14	3	·50	·50	75·00	·00	Iron	One	14	·079	·0961	—	4	4	¾	·077	·1731	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·50	1·00	·03	·03	"	"	"	"	"	—	"	"	"	"	"	
			·50	1·50	·07	·04	"	"	"	"	"	—	"	"	"	"	"	
			·50	2·00	·10	·03	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
				2·31	·13													
			·50	3·50	·14	·04	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·00	·19	·05	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·25	·21	·02	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·50	·24	·03	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·75	·27	·03	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·00	·31	·04	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·25	·37	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·375	·41	·04	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·500	·48	·07	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·625	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fairly. Per-centage of elongation :— ¼ Breaking strain = ·17 Breaking strain = ·64
23 Sept.	15	1	·50	·50	75·00	·00	Iron	One	14	·079	·0961	—	4	4	1	·0615	·1576	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·08	·08	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50	·14	·06	"	"	"	"	"	—	"	"	"	"	"	
				2·75	·18													
			·50	3·00	·21	·07	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			·50	3·50	·24	·03	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·00	·33	·09	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·50	·46	·13	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·75	·61	·15	"	"	"	"	"	—	"	"	"	"	"	Wire broke first, hemp alone sustained the weight for an appreciable length of time.
			·25	5·00	·89	·28	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·25	76·31	·42	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·50	82	·31	"	"	"	"	"	—	"	"	"	"	"	Per-centage of elongation :— ¼ Breaking strain = ·23 Breaking strain = 2·16
23 Sept.	15	1	·50	·50	50·00	·00	Iron	One	14	·079	·0961	—	4	4	1	·0615	·1576	Naked wire.
			·50	1·00	·02	·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	1·50	·04	·02	"	"	"	"	"	—	"	"	"	"	"	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·50	2·00	·06	·02	"	"	"	"	"	—	"	"	"	"	"	
				2·18	·07													
			·50	2·50	·09	·03	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·00	·11	·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·50	·14	·03	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·75	·17	·03	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·00	·20	·03	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·125	·21	·01	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·250	·23	·02	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·375	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fairly. Per-centage of elongation :— ¼ Breaking strain = ·14 Breaking strain = ·46

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined.—*continued.*

Date of Experiments.	Number of Specimen.	Number of Samples tested.	Weights. Put on.	Weights. Total.	Length of Samples between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. each Round Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						MANILLA.					
21 Sept.	*1*	*2*	·50	·50	*75·00*	·00	Iron	One	*14*	·079	·0961	—	4	4	1	·0615	·1576	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·08	·08	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50	·14	·08	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
				2·81														
			·50	3·00	·17	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
			·50	3·50	·22	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	4·00	·28	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·750	4·750	·38	·10	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	4·75	·40	·11	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·00	·73	·33	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·25	76·13	·38	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·50	·49	·38	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	5·625	·63	·14	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	At this weight wire broke.
			·750	5·750	·94	·31	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	5·875	77·14	·20	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weights were taken off. Wire broken close to the top. Suspension hemp perfect. Per-centage of elongation :— ½ Breaking strain = ·22 Breaking strain = 2·17
													Breaking strain of the hemp after the removal of the wire 4·75 cwt.					
23 Sept.	*15*	*3*	·50	·50	*75·00*	·00	Iron	One	*14*	·079	·0961	—	4	4	1	·0615	·1576	Covered iron wire.
			1·00	1·50	·06	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Same as 1 and 2.
				2·87	·12													
			1·00	2·50	·13	·07	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	3·00	·17	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	3·50	·21	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	4·00	·27	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	4·50	·36	·09	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	4·75	—	—	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Wire broke. Hemp sustained the weight about ½ min. Per-centage of elongation :— ½ Breaking strain = ·16 Breaking strain = ·48
21 Sept.	*16*	*1*	·50	·50	*50·00*	·80	Iron	One	*14*	·079	·0961	—	4	4	1½	·0508	·1559	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·04	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50	·09	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	3·50	·13	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
			·50	4·00	·16	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
				4·18														
			·50	4·50	·19	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	5·00	·22	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	5·50	·33	·11	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	6·00	·51	·18	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	6·25	·61	·10	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	6·50	·71	·10	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	6·75	·80	·09	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	7·00	·87	·07	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	7·25	·93	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	7·50	51·02	·09	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	7·625	·00	·07	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	7·750	·11	·02	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	7·875	·15	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	8·000	·16	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	8·125	·21	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·250	8·375	·29	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	8·500	—	—	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Broke while weight was being put on. Per-centage of elongation :— ½ Breaking strain = ·33 Breaking strain = 2·56
21 Sept.	*16*	*1*	·50	·50	*50·00*	·00	Iron	One	*14*	·079	·0961	—	—	—	—	—	—	Naked wire.
			1·00	1·50	·03	·03	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
				2·32	·09													Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	2·50	·10	·07	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·50	3·00	·13	·03	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·50	3·50	·16	·03	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	The hemp strands had been previously removed from this sample.
			·25	3·75	·18	·02	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·25	4·00	·21	·03	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·125	4·125	·23	·02	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·125	4·250	·25	·02	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·125	4·375	·30	·05	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·0625	4·4375	—	—	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·18 Breaking strain = ·60
21 Sept.	*16*	*1*	·50	·50	*50·00*	·00							—	—	—	—	—	
			·50	1·00	·25	·25	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	Manilla hemp. Removed from the above sample.
			·50	1·50	·48	·23	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
				1·81	·56													
			·50	2·00	·63	·15	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·50	2·50	·81	·18	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·25	2·75	·90	·09	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·25	3·00	·98	·08	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·125	3·125	51·08	·12	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·125	3·250	·18	·10	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·125	3·375	·25	·07	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·125	3·500	·31	·06	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	
			·125	3·625	—	—	,,	,,	,,	,,	,,	—	—	,,	—	,,	,,	Broke fairly. Per-centage of elongation :— ½ Breaking strain = 1·11 Breaking strain = 3·62
21 Sept.	*16*	*2*	·50	·50	*50·00*	·00	Iron	One	*14*	·079	·0961	—	4	4	1½	·0508	·1559	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·03	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50	·09	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	3·50	·12	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Weights put on at intervals of 1½ min.
			·50	4·00	·15	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
				4·18	·16													
			·50	4·50	·19	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	5·00	·23	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·25	·28	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·50	·32	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·75	·39	·07	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	6·00	·49	·10	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·125	·57	·08	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·250	·61	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Wire and Hemp combined. Weight per Fathom, Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						MANILLA.					
21 Sept.	16	2	·125	6·375	·67	·06	Iron	One	14	·079	·0961	—	4	4	1¼	·0398	·1359	Continued.
			·125	6·500	·71	·04	"	"	"	"	"		"	"	"	"	"	
			·125	6·625	·77	·06	"	"	"	"	"		"	"	"	"	"	
			·125	6·750	·80	·03	"	"	"	"	"		"	"	"	"	"	
			·125	6·875	·85	·05	"	"	"	"	"		"	"	"	"	"	
			·125	7·000	·90	·05	"	"	"	"	"		"	"	"	"	"	
			·125	7·125	·93	·03	"	"	"	"	"		"	"	"	"	"	
			·125	7·250	·98	·05	"	"	"	"	"		"	"	"	"	"	
			·125	7·375	51·01	·03	"	"	"	"	"		"	"	"	"	"	
			·125	7·500	·07	·06	"	"	"	"	"		"	"	"	"	"	
			·125	7·625	·10	·03	"	"	"	"	"		"	"	"	"	"	
			·125	7·750	·13	·03	"	"	"	"	"		"	"	"	"	"	
			·125	7·875	·19	·06	"	"	"	"	"		"	"	"	"	"	
			·125	8·000	·23	·01	"	"	"	"	"		"	"	"	"	"	
			·125	8·125	·24	·04	"	"	"	"	"		"	"	"	"	"	
			·125	8·250	·28	·04	"	"	"	"	"		"	"	"	"	"	
			·125	8·375	—	—	"	"	"	"	"		"	"	"	"	"	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·32 Breaking strain = 2·26
21 Sept.	16	3	·50	·50	50·00	0·00	Iron	One	14	·079	·0961	—	4	4	1¼	·0398	·1359	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·04	·04	"	"	"	"	"		"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	2·50	·09	·05	"	"	"	"	"		"	"	"	"	"	
			1·00	3·50	·12	·03	"	"	"	"	"		"	"	"	"	"	
			·50	4·00	·17	·05	"	"	"	"	"		"	"	"	"	"	
			·50	4·50	·22	·05	"	"	"	"	"		"	"	"	"	"	
			·50	5·00	·23	·01	"	"	"	"	"		"	"	"	"	"	
			·50	5·50	·37	·14	"	"	"	"	"		"	"	"	"	"	
			·50	6·00	·58	·21	"	"	"	"	"		"	"	"	"	"	
			·25	6·25	·70	·12	"	"	"	"	"		"	"	"	"	"	
			·25	6·50	·80	·10	"	"	"	"	"		"	"	"	"	"	
			·25	6·75	·90	·10	"	"	"	"	"		"	"	"	"	"	
			·25	7·00	·98	·08	"	"	"	"	"		"	"	"	"	"	
			·25	7·25	51·06	·08	"	"	"	"	"		"	"	"	"	"	
			·25	7·50	·15	·07	"	"	"	"	"		"	"	"	"	"	
			·25	7·75	·19	·06	"	"	"	"	"		"	"	"	"	"	
			·125	7·875	·23	·04	"	"	"	"	"		"	"	"	"	"	
			·125	8·00	·28	·05	"	"	"	"	"		"	"	"	"	"	
			·125	8·125	—	—	"	"	"	"	"		"	"	"	"	"	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·34 Breaking strain = 2·56
21 Sept.	16	4	5·00	5·00	—	—	—	—	—	—	—	—	—	—	—	—	—	This weight having been put on at once, the hemp covering was cut through, and wire broke immediately.
29 Sept.	16	5	·50	·50	75·00	·00	Iron	One	14	·079	·0961	—	4	4	1¼	·0398	·1359	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·05	·05	"	"	"	"	"		"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	2·50	·10	·05	"	"	"	"	"		"	"	"	"	"	
			·50	3·00	·13	·03	"	"	"	"	"		"	"	"	"	"	
			·50	3·50	·18	·05	"	"	"	"	"		"	"	"	"	"	
			·50	4·00	·29	·04	"	"	"	"	"		"	"	"	"	"	
			·50	4·50	·35	·06	"	"	"	"	"		"	"	"	"	"	
			·25	4·75	·39	·05	"	"	"	"	"		"	"	"	"	"	
			·25	5·00	·54	·05	"	"	"	"	"		"	"	"	"	"	
			·25	5·25	·45	·11	"	"	"	"	"		"	"	"	"	"	
			·25	5·50	·57	·12	"	"	"	"	"		"	"	"	"	"	
			·25	5·75	·80	·23	"	"	"	"	"		"	"	"	"	"	
			·25	6·00	·99	·19	"	"	"	"	"		"	"	"	"	"	
			·25	6·25	76·17	·18	"	"	"	"	"		"	"	"	"	"	
			·25	6·50	·33	·13	"	"	"	"	"		"	"	"	"	"	
			·125	6·625	·40	·10	"	"	"	"	"		"	"	"	"	"	
			·125	6·750	·49	·09	"	"	"	"	"		"	"	"	"	"	
			·125	6·875	·54	·05	"	"	"	"	"		"	"	"	"	"	
			·125	7·000	·61	·07	"	"	"	"	"		"	"	"	"	"	
			·125	7·125	·66	·05	"	"	"	"	"		"	"	"	"	"	
			·125	7·250	·72	·06	"	"	"	"	"		"	"	"	"	"	
			·125	7·375	·80	·08	"	"	"	"	"		"	"	"	"	"	
			·125	7·500	·86	·06	"	"	"	"	"		"	"	"	"	"	
			·125	7·625	·91	·05	"	"	"	"	"		"	"	"	"	"	
			·125	7·750	·99	·08	"	"	"	"	"		"	"	"	"	"	
			·125	7·875	77·03	·04	"	"	"	"	"		"	"	"	"	"	
			·125	8·000	·10	·07	"	"	"	"	"		"	"	"	"	"	
			·125	8·125	—	—	"	"	"	"	"		"	"	"	"	"	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·37 Breaking strain = 2·80
27 Sept.	17	1	·50	·50	75·00	·00	Iron	One	14	·079	·096	—	4	4	1¼	·062	·158	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·07	·07	"	"	"	"	"		"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	2·50	·14	·07	"	"	"	"	"		"	"	"	"	"	
			·50	3·00	·19	·05	"	"	"	"	"		"	"	"	"	"	
				3·43														
			·50	3·50	·24	·05	"	"	"	"	"		"	"	"	"	"	
			·50	4·00	·28	·04	"	"	"	"	"		"	"	"	"	"	
			·50	4·50	·35	·07	"	"	"	"	"		"	"	"	"	"	
			·25	4·75	·51	·16	"	"	"	"	"		"	"	"	"	"	
			·25	5·00	·66	·15	"	"	"	"	"		"	"	"	"	"	
			·25	5·25	·73	·07	"	"	"	"	"		"	"	"	"	"	
			·25	5·50	76·02	·10	"	"	"	"	"		"	"	"	"	"	
			·25	5·75	·18	·16	"	"	"	"	"		"	"	"	"	"	
			·25	6·00	·31	·13	"	"	"	"	"		"	"	"	"	"	
			·125	6·125	·39	·08	"	"	"	"	"		"	"	"	"	"	
			·125	6·250	·46	·07	"	"	"	"	"		"	"	"	"	"	
			·125	6·375	·52	·06	"	"	"	"	"		"	"	"	"	"	
			·125	6·500	·58	·06	"	"	"	"	"		"	"	"	"	"	
			·125	6·625	·83	·05	"	"	"	"	"		"	"	"	"	"	
			·125	6·750	1·70	·07	"	"	"	"	"		"	"	"	"	"	
			·125	6·875	—	—	"	"	"	"	"		"	"	"	"	"	Broke fairly. Per-centage of elongation :— ½ Breaking strain = ·35 Breaking strain = 2·26

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

Date of Experiments.	Number of Weights. Specimen.	Number of Weights. Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom Wire, and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
										UNWELDED.				MANILLA.				
27 Sept.	17	1	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	—	—	—	—	—	Naked Wire.
			·50	1·00	·02	·02	"	"	"	"	"	—	—	—	—	—	—	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·50	1·50	·04	·02	"	"	"	"	"	—	—	—	—	—	—	
			·50	2·00	·06	·02	"	"	"	"	"	—	—	—	—	—	—	Weights put on at intervals of 1½ min.
				2·21	·06													
			·50	2·50	·10	·04	"	"	"	"	"	—	—	—	—	—	—	
			·50	3·00	·12	·02	"	"	"	"	"	—	—	—	—	—	—	
			·50	3·50	·15	·03	"	"	"	"	"	—	—	—	—	—	—	
			·25	3·75	·18	·03	"	"	"	"	"	—	—	—	—	—	—	
			·25	4·00	·20	·02	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·125	·24	·04	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·250	·30	·06	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·375	·36	·06	"	"	"	"	"	—	—	—	—	—	—	
			·0625	4·4375	—	—	"	"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·16 Breaking strain = ·72
28 Sept.	17	2	·50	·50	75·00	·00	Iron	One	14	·079	0·96	—	4	4	1½	·062	·158	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·07	·07	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50	·14	·07	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·00	·19	·05	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
				3·06														
			·50	3·50	·24	·05	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·00	·30	·06	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·50	·40	·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·75	·50	·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·00	·64	·14	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·25	·77	·13	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·50	76·01	·24	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·75	·20	·19	"	"	"	"	"	—	"	"	"	"	"	
			·25	6·00	·37	·17	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·125	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·25 Breaking strain = 1·83
Sept.	17	3	·50	·50	75·00	·00	Iron	One	14	·079	·096	—	4	4	1½	·063	·158	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·07	·07	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50	·14	·07	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·00	·20	·06	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			·50	3·50	·24	·04	"	"	"	"	"	—	"	"	"	"	"	
				3·56														
			·50	4·00	·29	·05	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·50	·36	·07	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·75	·41	·05	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·00	·48	·07	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·25	·60	·12	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·50	·79	·19	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·75	·98	·19	"	"	"	"	"	—	"	"	"	"	"	
			·25	6·00	76·13	·15	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·125	·22	·09	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·250	·31	·09	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·375	·37	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·500	·43	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·625	·50	·07	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·750	·55	·05	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·875	·61	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	7·000	·66	·05	"	"	"	"	"	—	"	"	"	"	"	
			·125	7·125	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·32 Breaking strain = 2·20
														RUSSIAN.				
22 Sept.	18	1	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	¾	·077	·173	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·50	1·00	·01	·01	"	"	"	"	"	—	"	"	"	"	"	
			·50	1·50	·03	·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	2·00	·08	·05	"	"	"	"	"	—	"	"	"	"	"	
				2·38														
			·50	2·50	·11	·03	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·00	·14	·03	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·25	·16	·02	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·50	·19	·03	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·75	·21	·02	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·00	·22	·01	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·125	·23	·01	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·250	·24	·01	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·375	·26	·02	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·500	·29	·03	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·625	·33	·04	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·750	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·22 Breaking strain = ·66
22 Sept.	18	1	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	—	—	—	—	—	Naked wire.
			1·00	1·50	·02	·02	"	"	"	"	"	—	—	—	—	—	—	Sent from Messrs. R. S. Newall and Co., Birkenhead.
				2·25	·06													
			1·00	2·50	·07	·05	"	"	"	"	"	—	—	—	—	—	—	
			·50	3·00	·10	·03	"	"	"	"	"	—	—	—	—	—	—	
			·50	3·50	·12	·02	"	"	"	"	"	—	—	—	—	—	—	
			·25	3·75	·14	·02	"	"	"	"	"	—	—	—	—	—	—	
			·25	4·00	·17	·03	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·125	·20	·03	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·250	·22	·02	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·375	·28	·06	"	"	"	"	"	—	—	—	—	—	—	
			·125	4·500	—	—	"	"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·12 Breaking strain = ·56

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined.

Date of Experiments.	Number of Specimen.	Number of Samples tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. WIRE. Steel or Iron.	WIRE. No. of Wires.	WIRE. Gauge. Birmingham Wire Gauge.	WIRE. Gauge. Decimals of Inch.	WIRE. Weight per Fathom. Single Wire.	WIRE. Weight per Fathom. Strand.	HEMP. No. of Strands. Round each Wire.	HEMP. No. of Strands. Total.	HEMP. Length of Lay.	HEMP. Weight per Fathom.	Wire and Hemp combined. Weight per Fathom.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.								RUSSIAN.			
23 Sept.	18	2	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	½	·077	·173	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·50	1·00		·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	1·50		·05	"	"	"	"	"	—	"	"	"	"	"	
			·50	2·00		·07	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
				2·44														
			·50	2·50		·10	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·00		·12	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·25		·11	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·50		·13	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·75		·17	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·00		·23	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·125		·21	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·250		·23	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·375		·28	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·500		·28	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·625		·32	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·750		·44	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·875		—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·19 Breaking strain = ·38
23 Sept.	18	3	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	½	·077	·173	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·50	1·00		·01	"	"	"	"	"	—	"	"	"	"	"	
			·50	1·50		·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	2·00		·04	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			·50	2·50		·08	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·00		·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·25		·11	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·50		·12	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·75		·12	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·00		·15	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·125		·17	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·250		·29	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·375		·22	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·500		·24	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·625		·27	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·750		·30	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·875		·46	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·000		—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·16 Breaking strain = ·32
23 Sept.	19	1	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	·1	·050	·146	Sent from Messrs. R. S. Newall and Co. Birkenhead.
			·50	1·00		·01	"	"	"	"	"	—	"	"	"	"	"	
			·50	1·50		·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	2·00		·03	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			·50	2·50		·03	"	"	"	"	"	—	"	"	"	"	"	
				2·93														
			·50	3·00		·11	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·25		·13	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·50		·15	"	"	"	"	"	—	"	"	"	"	"	
			·25	3·75		·17	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·00		·19	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·125		·21	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·250		·23	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·375		·24	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·500		·25	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·625		·34	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·750		·45	"	"	"	"	"	—	"	"	"	"	"	
			·125	4·875		·59	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·00		·59	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·125		·63	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·250		·89	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·375		·88	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·500		·94	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·625	51·00	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·750		·05	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·875		—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·22 Breaking strain = 2·10
23 Sept.	19	2	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	·1	·050	·146	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·03	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50		·06	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·00		·11	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			·50	3·50		·13	"	"	"	"	"	—	"	"	"	"	"	
				3·61														
			·50	4·00		·17	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·25		·22	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·50		·25	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·75		·31	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·00		·47	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·25		·64	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·075		·73	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·500		·21	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·625		·90	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·750		·98	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·875	51·00	·04	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·000		·10	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·125		·19	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·250		·24	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·375		·27	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·500		·31	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·625		·33	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·750		·43	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·875		·56	"	"	"	"	"	—	"	"	"	"	"	
			·125	7·000		·54	"	"	"	"	"	—	"	"	"	"	"	
			·125	7·125		—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·27 Breaking strain = 3·08

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded, and Unwelded), and Hemp Strands, Separate and Combined.—*continued.*

Date of Experiments.	Number of Specimen.	Number of Samples tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						RUSSIAN.					
23 Sept.	19	3	·50	·50	50·00	0·00	Iron	One	14	·079	·096	—	4	4	1	·050	·146	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·03												
			1·00	2·50		·05												Weights put on at interval of 1½ min.
			·50	3·00		·06												
			·50	3·50		·07												
			·50	4·00		·08												
			·50	4·50		·09												
			·25	4·75		·09												
			·25	5·00		·10												
			·25	5·25		·11												
			·25	5·50		·11												
			·125	5·625	51·05	·09												
			·125	5·750		·10												
			·125	5·875		·10												
			·125	6·000	—	—												Broke fair. Per-centage of elongation :— ½ Breaking strain = ·22 Breaking strain = 2·40
23 Sept.	19	3	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·02												Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·50	2·00		·04												
				2·00														Weights put on at intervals of 1½ min.
			·50	2·50		·06												
			·50	3·00		·10												
			·25	3·25		·11												
			·25	3·50		·13												
			·25	3·75		·15												
			·25	4·00		·20												
			·125	4·125		·24												
			·0625	4·1875	—	—												Broke fair. Per-centage of elongation :— ½ Breaking strain = ·12 Breaking strain = ·48
17 Sept.	20	1	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	1½	·049	·145	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·05												
			·50	2·00		·08												Weights put on at intervals of 1½ min.
			·50	2·50		·09												
			·50	3·00		·11												
			·25	3·25		·12												
			·25	3·50		·13												
			·25	3·75		·17												
			·24	4·00		·19												
			·125	4·123		·21												
			·0625	4·1875		·22												
			·0625	4·2500		·23												
			·0625	4·3125		·24												
			·0625	4·3750		·26												
			·0625	4·4375		·27												
			·0625	4·5000		·29												
			·0625	4·5625		·30												
			·0625	4·6250		·32												
			·0625	4·6875		·34												
			·0625	4·7500		·35												
			·0625	4·8125		·41												
			·0625	4·8750		·45												
			·0625	4·9375		·50												
			·0625	5·0000		·54												
			·0625	5·0625		·59												
			·0625	5·1250		·62												
			·0625	5·1875		·63												
			·0625	5·2500		·70												
			·0625	5·3125		·73												
			·0625	5·3750		·77												
			·0625	5·4375		·80												
			·0625	5·5000		·81												
			·0625	5·5625		·84												
			·0625	5·6250		·87												
			·0625	5·6875		·89												
			·0625	5·7500		·91												
			·0625	5·8125		·94												
			·0625	5·8750		·96												
			·0625	5·9375	51·01	·03												
			·0625	6·0000		·04												
			·0625	6·0625		·07												
			·0625	6·1250		·09												
			·0625	6·1875		·11												
			·0625	6·2500		·13												
			·0625	6·3125		·16												
			·0625	6·3750		·19												
			·0625	6·4375		·24												
			·0625	6·5000	—	—												Broke fair. Per-centage of elongation :— ½ Breaking strain = ·28 Breaking strain = 2·46
17 Sept.	20	2	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	1½	·049	·145	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50		·01												
			1·00	2·50		·04												Weights put on at intervals of 1½ min.
			·50	3·00		·07												
				3·18		·08												
			·50	3·50		·10												
			·50	4·00		·13												
			·50	4·50		·20												
			·25	4·75		·25												
			·25	5·00		·35												
			·125	5·125		·44												
			·125	5·250		·51												
			·125	5·375		·59												
			·125	5·500		·68												

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

Date of Experiments.	Number of Specimens.	Number of Samples tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weights per Fathom. Single Wire.	Wire. Weights per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Wire and Hemp combined. Weight per Fathom.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						RUSSIAN.					
17 Sept.	20	2	·125	5·625	·70	·07	Iron	One	14	·079	·096	—	4	4	1½	·049	·145	Continued.
			·125	5·750	·77	·07	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·875	·83	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·000	·88	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·125	·94	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·250	51·00	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·375	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·16 Breaking strain = 2·00
17 Sept.	20	3	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	1½	·049	·145	Same as above.
			1·00	1·50	·04	·04	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50	·09	·05	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·00	·11	·02	"	"	"	"	"	—	"	"	"	"	"	
				3·00														
			·50	3·50	·13	·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·00	·15	·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·50	·19	·04	"	"	"	"	"	—	"	"	"	"	"	This sample was soaked in water for 22 hours.
			·50	5·00	·11	·07	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·25	·40	·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·50	·51	·11	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·75	·69	·18	"	"	"	"	"	—	"	"	"	"	"	
			·25	6·00	·89	·21	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·125	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·22 Breaking strain = 1·60
17 Sept.	20	4	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	1½	·049	·145	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·03	·03	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50	·09	·06	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
				2·88														
			·50	3·00	·11	·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·50	·13	·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·00	·17	·04	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·50	·21	·04	"	"	"	"	"	—	"	"	"	"	"	
			·25	4·75	·23	·05	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·00	·25	·09	"	"	"	"	"	—	"	"	"	"	"	This Sample was soaked in water 22 hours.
			·25	5·25	·30	·15	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·50	·67	·17	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·75	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·20 Breaking strain = 1·54
17 Sept.	20	5	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	1½	·049	·145	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·03	·03	"	"	"	"	"	—	"	"	"	"	"	
			1·00	2·50	·09	·06	"	"	"	"	"	—	"	"	"	"	"	Weights put on at intervals of 1½ min.
			·50	3·00	·11	·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	3·50	·15	·04	"	"	"	"	"	—	"	"	"	"	"	Sample not soaked in water.
				3·71	·16													
			·50	4·00	·17	·02	"	"	"	"	"	—	"	"	"	"	"	
			·50	4·50	·22	·05	"	"	"	"	"	—	"	"	"	"	"	
			·50	5·00	·32	·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·25	·42	·10	"	"	"	"	"	—	"	"	"	"	"	
			·25	5·50	·56	·14	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·625	·64	·08	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·750	·72	·08	"	"	"	"	"	—	"	"	"	"	"	
			·125	5·875	·77	·05	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·000	·81	·04	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·125	·86	·05	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·250	·90	·04	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·375	·94	·04	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·500	·99	·05	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·625	51·03	·03	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·750	·08	·06	"	"	"	"	"	—	"	"	"	"	"	
			·125	6·875	·13	·04	"	"	"	"	"	—	"	"	"	"	"	
			·0625	6·9375	·14	·02	"	"	"	"	"	—	"	"	"	"	"	With this weight the scale touched the ground. After the removal of weight, the amount of elongation was ·02.
			·0625	7·000	·15	·01	"	"	"	"	"	—	"	"	"	"	"	
			·0625	7·0625	·16	·01	"	"	"	"	"	—	"	"	"	"	"	
			·0625	7·1250	·17	·01	"	"	"	"	"	—	"	"	"	"	"	Weights replaced. Elongation as before 51·14.
			·0625	7·1875	·19	·02	"	"	"	"	"	—	"	"	"	"	"	
			·0625	7·2500	·20	·01	"	"	"	"	"	—	"	"	"	"	"	
			·0625	7·3125	·22	·02	"	"	"	"	"	—	"	"	"	"	"	
			·0625	7·3750	·23	·01	"	"	"	"	"	—	"	"	"	"	"	
			·0625	7·4375	—	—	"	"	"	"	"	—	"	"	"	"	"	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·32 Breaking strain = 2·46

No. 10.

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

RESULTS of EXPERIMENTS made by MESSRS. GISBORNE and FORDE and C. W. SIEMENS, for the purpose of arriving at the best form for OUTER COVERING of CABLE.

Date of Experiments.	Description of Specimen, and Sample tested.	No.	Time. H. M.	Tests for Strength. Weight. Put on.	Weight. Off.	Strain on Cable.	Length of Sample between Clamps.	Elongation. Plus.	Elongation. Minus.	Electrical Tests. Temperature of Water.	Insulation. P. P.	Continuity. P. P.	Weight per Knot. In Air.	Weight per Knot. In Water.	Equivalent in Depth for Weight in Water.	REMARKS.
1859:				*Cwts.*	*Cwts.*	*Cwts.*	*Ins.*	*Ins.*	*Ins.*	°	°	°	*Cwts.*	*Cwts.*	*Fms.*	
8 Sept.	No. 14. (Gauge Iron Wire covered with hemp.) Diameter of Iron Wire ·079 inch. Specimen immersed in Water for about 22 hours.	20–6	—	·50	—	·50	50·00	·00	—	—	—	—	—	—	—	Sent from R. S. Newall and Co.
			—	1·00	—	1·50		·02	—	—	—	—	—	—	—	
			—	1·00	—	2·50		·08	—	—	—	—	—	—	—	
			—	1·00	—	3·50		·13	—	—	—	—	—	—	—	
			—	·50	—	4·00		·19	—	—	—	—	—	—	—	
			—	·50	—	4·50		·21	—	—	—	—	—	—	—	
			—	·25	—	4·75		·43	—	—	—	—	—	—	—	
			—	·125	—	4·875		·62	—	—	—	—	—	—	—	
			—	·125	—	5·00	—	—	—	—	—	—	—	—	—	Broke fair. Per-centage of elongation:— ½ Breaking strain = ·16 Breaking strain = 1·04
7 Sept.	No. 14. (Gauge Iron Wire, Diameter ·079 inch.	20–6	—	·50	—	·50	50·00	·00	—	—	—	—	—	—	—	Naked wire.
			—	1·00	—	1·50		·02	—	—	—	—	—	—	—	
			—	·50	—	2·00		·06	—	—	—	—	—	—	—	
			—		—	2·00			—	—	—	—	—	—	—	
			—	·50	—	2·50		·08	—	—	—	—	—	—	—	
			—	·50	—	3·00		·10	—	—	—	—	—	—	—	
			—	·25	—	3·25		·11	—	—	—	—	—	—	—	
			—	·25	—	3·50		·13	—	—	—	—	—	—	—	
			—	·25	—	3·75		·15	—	—	—	—	—	—	—	
			—	·25	—	4·00		·20	—	—	—	—	—	—	—	
			—	·125	—	4·125		·24	—	—	—	—	—	—	—	
			—	·0625	—	4·1875	—	—	—	—	—	—	—	—	—	Broke fair. Per-centage of elongation:— ½ Breaking strain = ·12 Breaking strain = ·48

RESULTS of EXPERIMENTS made by Messrs. GISBORNE and FORDE and C. W. SIEMENS, upon the Strength of STEEL and IRON WIRES (WELDED and UNWELDED), and HEMP STRANDS, SEPARATE and COMBINED.

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom. Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
								UNWELDED.						RUSSIAN.				
23 Sept.	21	1	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	1½	·05	·146	Sent from Messrs. R. S. Newall and Co. Birkenhead.
			1·00	1·50	·04	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50	·10	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	3·00	·12	·02	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
				3·31	·14													Weight put on at intervals of 1½ min.
			·50	3·50	·16	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	4·00	·20	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	4·50	·26	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	4·75	·30	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·00	·37	·07	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·25	·47	·10	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·50	·59	·12	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	5·625	·63	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	5·750	·69	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	5·875	·75	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·000	·79	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·125	·83	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·250	·88	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·375	·91	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·500	·96	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·625	—	—	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Broke fairly. Per-centage of elongation:— ½ Breaking strain = ·28 Breaking strain = 1·02
23 Sept.	21	2	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	1½	·05	·146	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·04	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			1·00	2·50	·10	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	3·00	·13	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	3·50	·17	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
				3·53														Weights put on at intervals of 1½ min.
			·50	4·00	·19	·02	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	4·50	·23	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	4·75	·28	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·00	·33	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·25	·44	·11	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	5·50	·56	·12	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	5·625	·63	·07	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	5·750	·69	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	5·878	·73	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·000	·76	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·125	·81	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·250	·84	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·375	·90	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·500	·93	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·625	·99	·06	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·750	51·03	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	6·875	·09	·07	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	7·000	·11	·02	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	7·125	·15	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·125	7·250	—	—	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Broke fairly. Per-centage of elongation:— ½ Breaking Strain = ·34 Breaking strain = 2·30
23 Sept.	21	3	·50	·50	50·00	·00	Iron	One	14	·079	·096	—	4	4	1½	·05	·146	Sent from Messrs. R. S. Newall and Co. Birkenhead.
			1·00	1·50	·04	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
				2·38														Weights put on at intervals of 1½ min.
			1·00	2·56	·09	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	3·00	·14	·05	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	3·50	·17	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	4·00	·20	·03	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·50	4·50	·24	·04	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	
			·25	4·75	—	—	,,	,,	,,	,,	,,	—	,,	,,	,,	,,	,,	Broke fairly. Per-centage of elongation:— ½ Breaking strain = ·17 Breaking strain = 1·48

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

TESTS for STRENGTH of SINGLE IRON and STEEL WIRES, both WELDED and UNWELDED, made by H. C. FORDE and C. W. SIEMENS.

No.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	Description of Sample and Remarks.
	Cwts.	*Cwts.*	*Ins.*	*Ins.*	
	Tested 22th June.				
22-1	1·00	1·00	50·00	·00	Naked wire.
	·50	1·50	·02	·02	A 1. Leather labelled. Small 14
	·50	2·00	·04	·02	Gauge. Iron Wire. Not welded.
		2·13			
	·50	2·50	·06	·02	
	·50	3·00	·09	·03	
	·25	3·25	·10	·01	
	·25	3·50	·11	·01	
	·25	3·75	·13	·02	
	·25	4·00	·15	·02	
	·25	4·25	·20	·05	Broke fairly.
					Per-centage of elongation :—
					½ Breaking strain = ·09
					Breaking strain = ·40
23-1	1·00	1·00	50·00	·00	A 2. Leather labelled. Not welded.
	1·00	2·00	·05	·05	
		2·13	·06		
	1·00	3·00	·10	·05	
	·50	3·50	·12	·02	
	·50	4·00	·18	·06	
	·25	4·25	·22	·04	
	·125	4·375	·25	·03	Broke fair.
					Per-centage of elongation :—
					½ Breaking strain = ·12
					Breaking strain = ·50
22-1	1·00	1·00	50·00	·00	Naked wire.
		1·21	·02		A 1. Welded. Cut off the same
	·50	1·50	·03	·03	piece.
	·50	2·00	·07	·04	
	·25	2·25	·11	·04	
	·25	2·50	·19	·08	
	·125	2·625	—	—	Broke 1·5 in. from weld.
					Per-centage of elongation :—
					½ Breaking strain = ·04
					Breaking strain = ·38
24-1	·50	·50	50·00	·00	Naked wire.
	1·00	1·50	·05	·05	Sample B 1. Not welded. 14 Gauge
	·25	1·75	·07	·02	Iron. Leather label.
	·25	2·00	·08	·01	
	·25	2·25	·09	·01	
	·25	2·50	·10	·01	
	·25	2·75	·11	·01	
	·25	3·00	·12	·01	
	·25	3·25	·13	·01	
	·25	3·50	·14	·01	
	·25	3·75	·16	·02	
	·25	4·00	·18	·02	
	·25	4·25	·23	·05	
	·125	4·375	·26	·03	
	·125	4·500	—	—	Broke quite fair.
					Per-centage of elongation :—
					½ Breaking strain = ·15
					Breaking strain = ·62
25-1	1·00	1·00	50·00	·00	Naked wire.
	1·00	2·00	·04	·04	B 2. Leather labelled. 14 Gauge.
		2·25	·05		Not welded.
	1·00	3·00	·09	·05	
	·50	3·50	·12	·03	
	·50	4·00	·13	·01	
	·25	4·25	·15	·02	
	·125	4·375	·17	·02	
	·125	4·500	·19	·02	Fair break.
					Per-centage of elongation :—
					½ Breaking strain = ·10
					Breaking strain = ·38
24-1	1·00	1·00	50·00	·00	Naked wire.
		1·21	·01		Same sample as B 1. Welded. Cut
	·50	1·50	·02	·02	off same piece.
	·50	2·00	·06	·04	
	·25	2·25	·09	·03	
	·25	2·50	·14	·05	
	·125	2·625	—	—	Broke at weld.
					Per-centage of elongation :—
					½ Breaking strain = ·03
					Breaking strain = ·28
26-1	1·00	1·00	50·00	·00	Naked wire.
	1·00	2·00	·04	·04	C 1. Leather labelled. Iron wire.
		2·39			Small 14 Gauge. Not welded.
	·50	2·50	·06	·02	
	·50	3·00	·08	·02	

No.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	Description of Sample and Remarks.
	Cwts.	*Cwts.*	*Ins.*	*Ins.*	
	·50	3·50	·12	·04	Continued.
	·25	3·75	·13	·01	
	·25	4·00	·15	·02	
	·25	4·25	·17	·02	
	·25	4·50	·22	·05	
	·125	4·625	·24	·02	
	·125	4·750	—	—	Fair break.
					Per-centage of elongation :—
					½ Breaking strain = ·12
					Breaking strain = ·48
27-1	1·00	1·00	50·00	·00	Naked wire.
	1·00	2·00	·03	·03	C 2. Leather labelled. 14 Gauge.
		2·39	·04		Not welded.
	1·00	3·00	·07	·04	
	·50	3·50	·11	·04	
	·50	4·00	·14	·03	
	·125	4·125	·15	·01	
	·125	4·250	·16	·01	
	·125	4·375	·18	·02	
	·125	4·500	·20	·02	
	·125	4·625	·22	·02	
	·125	4·750	·24	·02	Broke.
					Per-centage of elongation :—
					½ Breaking strain = ·08
					Breaking strain = ·48
26-1	1·00	1·00	50·00	·00	Naked wire.
	·50	1·50	·02	·02	C 1. Cut off the same piece.
	·50	2·00	·05	·03	Welded.
	·25	2·25	·06	·01	
	·25	2·50	·07	·01	
	·125	2·625	·07	·00	
	·125	2·750	·08	·01	
	·125	2·875	·09	·01	
	·125	3·00	—	—	Broke 1·5 in. from weld.
					Per-centage of elongation :—
					½ Breaking strain = ·04
					Breaking strain = ·18
	Tested 24 July.				
28-1	1·00	1·00	50·00	·00	Naked wire.
		1·93			No. 1. Best Charcoal Iron. Not
	1·00	2·00	·04	·04	welded. No. 14 Gauge.
	·50	2·50	·08	·04	
	·50	3·00	·12	·03	
	·25	3·25	·14	·03	
	·25	3·50	·19	·05	
	·125	3·625	·22	·03	
	·125	3·750	·25	·03	
	·125	3·875	—	—	Broke fair.
					Per-centage of elongation :—
					½ Breaking strain = ·08
					Breaking strain = ·50
28-1	1·00	1·00	50·00	·00	Naked wire.
		1·12	·01		No. 1. Best Charcoal Iron. Welded.
	·50	1·50	·05	·05	No. 14 Gauge.
	·50	2·00	·13	·08	
	·125	2·125	·20	·07	
	·125	2·250	—	—	Broke at weld; the last weight
					scarcely touching scale.
					Per-centage of elongation :—
					½ Breaking strain = ·02
					Breaking strain = ·40
28-2	1·00	1·00	·00	·00	Naked wire.
		1·19	·02		No. 2. Best Charcoal Iron. Welded.
	·50	1·50	·03	·05	
	·50	2·00	·10	·07	
	·125	2·125	·12	·02	
	·125	2·250	·19	·07	
	·125	2·375	—	—	Broke at weld.
					Per-centage of elongation :—
					½ Breaking strain = ·04
					Breaking strain = ·38
28-3	1·00	1·00	50·00	·00	Naked wire.
		1·12	·01		No. 3. Best Charcoal Iron. Welded.
	·50	1·50	·04	·04	
	·50	2·00	·11	·07	
	·25	2·25	—	—	Broke at weld instantly; the last
					weight touched the scales.
					Per-centage of elongation :—
					½ Breaking strain = ·02
					Breaking strain = ·22

App. No. 10.

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

RESULTS of EXPERIMENTS made by Messrs. GISBORNE and FORDE and C. W. SIEMENS, upon the Strength of STEEL and IRON WIRES (WELDED and UNWELDED), and HEMP STRANDS, SEPARATE and COMBINED.

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weights per Fathom. Single Wire.	Wire. Weights per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weights per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
									UNWELDED.				RUSSIAN.					
13 Dec.	29	1	·50	·50	50·00	·00	Steel	One	14	·076	·095	—	4	4	1¼	·045	0·14	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·01	·01	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	2·50	·05	·04	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Weights put on at intervals of 1¼ min.
			1·00	3·50	·10	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	4·50	·15	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
				5·37														
			1·00	5·50	·20	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	6·50	·30	·10	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	7·50	·40	·10	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	8·00	·47	·07	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	8·50	·56	·09	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	9·00	·69	·13	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	9·25	·75	·06	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	9·50	·83	·08	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	9·75	·88	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·00	·96	·08	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·25	51·03	·00	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·50	·11	·00	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·75	·20	·00	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Broke fair. Per-centage of elongation:— ½ Breaking strain = ·38 Breaking strain = 2·40
13 Dec.	29	1	·50	·50	50·00	·00	Steel	One	14	·076	·095	—	4	4	1¼	·045	·140	Naked wire.
			1·00	1·50	·10	·10	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	2·50	·22	·12	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	3·50	·36	·14	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Weights put on at intervals of 1¼ min.
				3·87	·45													
			1·00	4·50	·55	·19	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	5·00	·62	·07	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	5·50	·70	·08	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	6·00	·79	·09	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·25	·84	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·50	·90	·06	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·75	·95	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	7·00	51·01	·06	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	7·25	·10	·09	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	7·50	·20	·10	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	7·75	—	—	〃	〃	〃	〃	〃	—	〃	〃	〃	〃	〃	Broke directly weight was put on. Per-centage of elongation:— ½ Breaking strain = ·20 Breaking strain = 2·40
13 Dec.	29	1	·50	·50	50·00	·00							4	4	1¼	·045	·140	Hemp from above sample.
				1·12	·12													
			1·00	1·50	·21	·21	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	2·00	·41	·20	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	2·25	·50	·09	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Broke fair. Per-centage of elongation:— ½ Breaking strain = ·24 Breaking strain = 1·00
14 Dec.	29	2	·50	·50	50·00	·00	Steel	One	14	·076	·095	—	4	4	1¼	·045	·140	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·05	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	2·50	·10	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	3·50	·15	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Weights put on at intervals of 1¼ min.
			1·00	4·50	·29	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
				5·37														
			1·00	6·50	·24	·04	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	6·50	·31	·07	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	7·50	·40	·09	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	8·00	·45	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	8·50	·51	·06	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	9·00	·55	·04	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	9·50	·70	·15	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	9·75	·76	·06	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·00	·82	·06	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·25	·90	·08	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·50	·96	·06	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·75	51·10	·14	〃	〃	〃	〃	〃	—	〃	〃	〃	〃	〃	Broke after sustaining weight near 2 minutes. Per-centage of elongation:— ½ Breaking strain = ·48 Breaking strain = 2·20
16 Dec.	30	1	·50	·50	50·00	·00	Steel	One	14	·076	·095	—	4	4	1¼	·042	·137	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·02	·02	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	2·50	·05	·03	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	3·50	·10	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Weights put on at intervals of 1¼ min.
			1·00	4·50	·15	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
				5·25	·19													
			1·00	6·50	·20	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	6·50	·25	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	7·50	·34	·09	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	8·00	·40	·06	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	8·50	·45	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	9·00	·52	·07	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	9·50	·59	·07	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	9·75	·63	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·00	·69	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·25	·74	·05	〃	〃	〃	〃	〃	—	〃	〃	〃	〃	〃	
			·25	10·50	—	—	〃	〃	〃	〃	〃	—	〃	〃	〃	〃	〃	Broke fair. Per-centage of elongation:— ½ Breaking strain = ·33 Breaking strain = 1·46
16 Dec.	30	1	·50	·50	50·00	·00	Steel	One	14	·076	·095	—						Naked wire.
			1·00	1·50	·05	·05	〃	〃	〃	〃	〃	〃						Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	2·50	·10	·05	〃	〃	〃	〃	〃	〃						
			1·00	3·50	·16	·06	〃	〃	〃	〃	〃	〃						Weights put on at intervals of 1¼ min.
				4·00	·20													
			1·00	4·50	·23	·07	〃	〃	〃	〃	〃	〃						
			1·00	5·50	·30	·07	〃	〃	〃	〃	〃	〃						
			·50	6·00	·34	·04	〃	〃	〃	〃	〃	〃						
			·50	6·50	·41	·07	〃	〃	〃	〃	〃	〃						

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

Date of Experiment.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							UNWELDED.						RUSSIAN.					
16 Dec.	30	1	·50	7·00	·50	·09	Steel	One	14	·076	·095	—	4	4	1½	·042	·137	Continued.
			·25	7·25	·87	·07	"	"	"	"	"		"	"	"	"	"	
			·25	7·50	·62	·12	"	"	"	"	"		"	"	"	"	"	
			·25	7·75	·85	·16	"	"	"	"	"		"	"	"	"	"	
			·25	8·00	—	—	"	"	"	"	"		"	"	"	"	"	Broke fair. Per-centage of elongation:— ½ Breaking strain = ·40 Breaking strain = 1·70
16 Dec.	30	1	·50	·50	50·00	·00	Steel	One	14	·076	·095	—	4	4	1½	·042	·137	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			·50	1·00	·10	·19	"	"	"	"	"		"	"	"	"	"	Weights put on at intervals of 1½ min.
				1·27	·19													
			·50	1·69	·23	·13	"	"	"	"	"		"	"	"	"	"	
			·50	2·00	·39	·16	"	"	"	"	"		"	"	"	"	"	Hemp from the above Sample.
			·25	2·25	·22	·13	"	"	"	"	"		"	"	"	"	"	
			·25	2·50	·62	·16	"	"	"	"	"		"	"	"	"	"	Broke fair. Per-centage of elongation:— ½ Breaking strain = ·38 Breaking strain = 1·24
			·25	2·75	—	—	"	"	"	"	"		"	"	"	"	"	
16 Dec.	30	2	·50	·50	50·00	·00	Steel	One	14	·076	·095	—	4	4	1½	·042	·137	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·02	·02	"	"	"	"	"		"	"	"	"	"	
			1·00	2·50	·04	·02	"	"	"	"	"		"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	3·50	·09	·05	"	"	"	"	"		"	"	"	"	"	
			1·00	4·50	·14	·05	"	"	"	"	"		"	"	"	"	"	
			1·00	5·50	·18	·04	"	"	"	"	"		"	"	"	"	"	
			1·00	6·50	·24	·06	"	"	"	"	"		"	"	"	"	"	
			1·00	7·50	·31	·07	"	"	"	"	"		"	"	"	"	"	
			1·00	8·50	·40	·09	"	"	"	"	"		"	"	"	"	"	
			·50	9·00	·47	·07	"	"	"	"	"		"	"	"	"	"	
			·50	9·50	·55	·08	"	"	"	"	"		"	"	"	"	"	
			·25	9·75	·60	·05	"	"	"	"	"		"	"	"	"	"	
			·25	10·00	·63	·03	"	"	"	"	"		"	"	"	"	"	
			·25	10·25	·70	·05	"	"	"	"	"		"	"	"	"	"	
			·25	10·50	·76	·05	"	"	"	"	"		"	"	"	"	"	
			·25	10·75	·84	·00	"	"	"	"	"		"	"	"	"	"	
			·25	11·00	—	—	"	"	"	"	"		"	"	"	"	"	Broke instantly weight was put on Per-centage of elongation:— ½ Breaking strain = ·32 Breaking strain = 1·63
17 Dec.	31	1	·50	·50	50·00	·00	Steel	One	14	·076	·095	—	4	4	1½	·041	·136	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	1·50	·05	·05	"	"	"	"	"		"	"	"	"	"	
			1·00	2·50	·10	·05	"	"	"	"	"		"	"	"	"	"	Weights put on at intervals of 1½ min.
			1·00	3·50	·16	·05	"	"	"	"	"		"	"	"	"	"	
			1·00	4·50	·19	·04	"	"	"	"	"		"	"	"	"	"	
			1·00	5·50	·25	·06	"	"	"	"	"		"	"	"	"	"	
				5·62	·26													
			1·00	6·50	·31	·06	"	"	"	"	"		"	"	"	"	"	
			1·00	7·50	·40	·09	"	"	"	"	"		"	"	"	"	"	
			·50	8·00	·45	·05	"	"	"	"	"		"	"	"	"	"	
			·50	8·50	·51	·06	"	"	"	"	"		"	"	"	"	"	
			·50	9·00	·56	·05	"	"	"	"	"		"	"	"	"	"	
			·50	9·50	·64	·08	"	"	"	"	"		"	"	"	"	"	
			·25	10·00	·71	·07	"	"	"	"	"		"	"	"	"	"	
			·25	10·25	·79	·08	"	"	"	"	"		"	"	"	"	"	
			·25	10·50	·84	·05	"	"	"	"	"		"	"	"	"	"	
			·25	10·75	·89	·05	"	"	"	"	"		"	"	"	"	"	
			·25	11·00	·95	·06	"	"	"	"	"		"	"	"	"	"	
			·25	11·25	51·01	·06	"	"	"	"	"		"	"	"	"	"	Broke after sustaining weight 1½ min. Per-centage of elongation:— ½ Breaking strain = ·52 Breaking strain = 2·02
17 Dec.	31	1	·50	·50	50·00	·00	Steel	One	14	·076	·095	—	—	—	—	—	—	Naked wire.
			1·00	1·50	·05	·05	"	"	"	"	"		—	—	—	—	—	Sent from Messrs. R. S. Newall and Co., Birkenhead.
			1·00	2·50	·10	·05	"	"	"	"	"		—	—	—	—	—	
			1·00	3·50	·15	·05	"	"	"	"	"		—	—	—	—	—	Weights put on at intervals of 1½ min.
				3·57	·17													
			1·00	4·50	·20	·05	"	"	"	"	"		—	—	—	—	—	
			1·00	5·50	·30	·10	"	"	"	"	"		—	—	—	—	—	
			·50	6·00	·34	·04	"	"	"	"	"		—	—	—	—	—	
			·50	6·50	·40	·06	"	"	"	"	"		—	—	—	—	—	
			·50	7·00	·50	·10	"	"	"	"	"		—	—	—	—	—	
			·50	7·50	·63	·13	"	"	"	"	"		—	—	—	—	—	
			·25	7·75	·82	·19	"	"	"	"	"		—	—	—	—	—	
			·25	8·00	—	—	"	"	"	"	"		—	—	—	—	—	Broke at once. Per-centage of elongation:— ½ Breaking strain = ·34 Breaking strain = 1·64
17 Dec.	31	1	·50	·50	50·00	·00	"	"	"	"	"	—	4	4	1½	·041	·136	Hemp stripped from above.
			1·00	1·50	·22	·22	"	"	"	"	"		"	"	"	"	"	
				1·62	·31													
			·50	2·00	·40	·18	"	"	"	"	"		"	"	"	"	"	
			·25	2·25	·50	·10	"	"	"	"	"		"	"	"	"	"	
			·25	2·50	·55	·05	"	"	"	"	"		"	"	"	"	"	
			·25	2·75	·60	·05	"	"	"	"	"		"	"	"	"	"	
			·25	3·00	·68	·08	"	"	"	"	"		"	"	"	"	"	
			·25	3·25	·80	·14	"	"	"	"	"		"	"	"	"	"	
			·25	3·50	—	—	"	"	"	"	"		"	"	"	"	"	Broke instantly. Per-centage of elongation:— ½ Breaking strain = ·62 Breaking strain = 1·00
19 Dec.	32	1	·50	·50	50·00	·00	Steel	One	14	·075	·089	—	—	—	—	—	—	Naked wire.
			1·00	1·50	·05	·05	"	"	"	"	"		—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	2·50	·10	·05	"	"	"	"	"		—	—	—	—	—	
			1·00	3·50	·14	·04	"	"	"	"	"		—	—	—	—	—	Weights put on at intervals of ·1½ min.
				3·62	·15	—												

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined.—*continued.*

Date of Experiments.	Number of Specimens.	Number of Samples tested.	Weights. Put on.	Weights. Total.	Sample between Length of Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							NAKED WIRES.—UNWELDED.											
19 Dec.	32	1	1·00	4·50	50·00	·20	Steel	One	14	·075	·089	—	—	—	—	—	—	Continued.
			1·00	5·50		·20	"	"	"	"	"							
			1·00	6·50		·40	"	"	"	"	"							
			·50	7·00		·45	"	"	"	"	"							
			·25	7·25		·50	"	"	"	"	"							Broke fair. Per-centage of elongation :— ½ Breaking strain = ·30 Breaking strain = 1·00
19 Dec.	32	2	·50	·50	50·00	·00	Steel	One	14	·075	·089	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·05	"	"	"	"	"							Sent from Messrs. Webster and Horsfall.
			1·00	2·50		·10	"	"	"	"	"							Weights put on at intervals of 1½ min.
			1·00	3·50		·15	"	"	"	"	"							
				3·62		·17	—	—	—	—	—							
			1·00	4·50		·21	"	"	"	"	"							
			1·00	5·50		·26	"	"	"	"	"							
			·50	6·00		·31	"	"	"	"	"							
			·50	6·50		·36	"	"	"	"	"							
			·50	7·00		·50	"	"	"	"	"							
			·25	7·25		—	"	"	"	"	"							
							NAKED WIRES.—WELDED.											
20 Dec.	32	1	·50	·50	50·00	·00	Steel	One	14	·075	·089	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·32 Breaking strain = 1·00
			1·00	1·50		·05	"	"	"	"	"							Naked wire, welded.
				1·62		·06	—	—	—	—	—							Sent from Messrs. Webster and Horsfall.
			1·00	2·50		·10	"	"	"	"	"							Weights put on at intervals of 1½ min.
			·50	3·00		·16	"	"	"	"	"							Broke instantly weight touched the scale at the weld.
			·25	3·25		—	"	"	"	"	"							Per-centage of elongation :— ½ Breaking strain = ·12 Breaking strain = ·32
20 Dec.	32	2	·50	1·50	50·00	·00	Steel	One	14	·075	·08	—	—	—	—	—	—	Naked wire, welded.
			1·00	1·50		·05	"	"	"	"	"							Sent from Messrs. Webster and Horsfall.
			·50	2·00		·07	"	"	"	"	"							Weights put on at intervals of 1½ min.
			·50	2·50		·10	"	"	"	"	"							Broke at the weld.
			·25	2·75		·12	"	"	"	"	"							Per-centage of elongation :— ½ Breaking strain = ·10 Breaking strain = ·24
			·25	3·00		—	"	"	"	"	"							
							NAKED WIRES.—UNWELDED.											
19 Dec.	33	1	·50	·50	50·00	·00	Steel	One	13	·085	·118	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·05	"	"	"	"	"							Sent from Messrs. Webster and Horsfall.
			1·00	2·50		·09	"	"	"	"	"							Weights put on at intervals of 1½ min.
			1·00	3·50		·12	"	"	"	"	"							
			1·00	4·50		·14	"	"	"	"	"							
				4·75		·15	—	—	—	—	—							
			1·00	5·50		·19	"	"	"	"	"							
			1·00	6·50		·24	"	"	"	"	"							
			1·00	7·50		·30	"	"	"	"	"							
			1·00	8·50		·39	"	"	"	"	"							
			·50	9·00		·45	"	"	"	"	"							
			·50	9·50		—	"	"	"	"	"							
							NAKED WIRES.—WELDED.											
20 Dec.	33	1	·50	·50	50·00	·00	Steel	One	13	·085	·118	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·30 Breaking strain = ·90
			1·00	1·50	—	—	"	"	"	"	"							Sent from Messrs. Webster and Horsfall.
			1·00	2·50	—	—	"	"	"	"	"							Weights put on at intervals of 1½ min.
			·50	3·00	—	—	"	"	"	"	"							
			·25	3·25	—	—	"	"	"	"	"							
			·25	3·50	—	—	"	"	"	"	"							
			·25	3·75	—	—	"	"	"	"	"							
			·25	4·00	—	—	"	"	"	"	"							
			·25	4·25	—	—	"	"	"	"	"							Broke at the weld.
20 Dec.	33	2	·50	·50	—	—	—	—	—	—	—	—	—	—	—	—	—	Naked wire, welded.
			1·00	1·50	—	—	—	—	—	—	—	—	—	—	—	—	—	
			1·00	2·50	—	—	—	—	—	—	—	—	—	—	—	—	—	
			·50	3·00	—	—	—	—	—	—	—	—	—	—	—	—	—	
			·25	3·25	—	—	—	—	—	—	—	—	—	—	—	—	—	
			·25	3·50	—	—	—	—	—	—	—	—	—	—	—	—	—	
			·25	3·75	—	—	—	—	—	—	—	—	—	—	—	—	—	
			·25	4·00	—	—	—	—	—	—	—	—	—	—	—	—	—	
			·25	4·25	—	—	—	—	—	—	—	—	—	—	—	—	—	
			·25	4·50	—	—	—	—	—	—	—	—	—	—	—	—	—	
			·25	4·75	—	—	—	—	—	—	—	—	—	—	—	—	—	
			·25	5·00	—	—	—	—	—	—	—	—	—	—	—	—	—	Broke about 1½ inch from weld. Elongation of these samples not taken. Wire very much bent close to welds.
							NAKED WIRES.—UNWELDED.											
20 Dec.	34	1	·50	·50	50·00	·00	Steel	One	12	·097	·150	—	—	—	—	—	—	Naked wire.
			1·00	1·50		·02	"	"	"	"	"							Sent from Messrs. Webster and Horsfall.
			1·00	2·50		·04	"	"	"	"	"							Weights put on at intervals of 1½ min.
			1·00	3·50		·06	"	"	"	"	"							
			1·00	4·50		·09	"	"	"	"	"							
				5·125		·11	—	—	—	—	—							
			1·00	6·50		·13	"	"	"	"	"							
			1·00	6·50		·14	"	"	"	"	"							
			1·00	7·50		·17	"	"	"	"	"							
			1·00	8·50		·23	"	"	"	"	"							
			·50	9·00		·25	"	"	"	"	"							
			·50	9·50		·33	"	"	"	"	"							
			·25	9·75		·39	"	"	"	"	"							
			·25	10·00		·46	"	"	"	"	"							
			·25	10·25		—	"	"	"	"	"							Broke fair. Per-centage of elongation :— ½ Breaking strain = ·32 Breaking strain = ·96

APPENDIX No. 10.—FALMOUTH AND GIBRALTAR TELEGRAPH—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Length of Sample between Clamps.	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Round each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							NAKED WIRES.—UNWELDED.											
20 Dec.	34	2	·50	·50	50·00	·00	Steel	One	12	·097	·150	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50	·03	·03	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	2·50	·04	·02	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	3·50	·06	·02	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Weights put on at intervals of 1½ min.
			1·00	4·50	·09	·03	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
				5·25	·11	—												
			1·00	6·50	·19	·03	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	6·50	·14	·02	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	7·50	·17	·03	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	8·50	·20	·03	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	9·00	·23	·03	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	9·50	·26	·03	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	9·75	·29	·03	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·00	·34	·05	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·25	·42	·08	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	10·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·22 Breaking strain = ·34
							NAKED WIRES.—WELDED.											
20 Dec.	35	1	·50	·50	—	—	Steel	One	12	·097	·150	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	2·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	3·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Weights put on at intervals of 1½ min.
			·50	4·00	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	4·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	5·00	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	5·25	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	5·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	5·75	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·00	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Broke at weld.
			·25	6·25	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
20 Dec.	33	2	·50	·50	—	—	〃	〃	〃	〃	〃	〃	—	—	—	—	—	
			1·00	1·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	2·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	3·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	4·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	5·00	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	5·50	—	—	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
																		Broke at the weld.
																		Elongation of these samples not taken. Wire very much bent close to welds.
							NAKED WIRES.—UNWELDED.											
21 Dec.	36	1	·50	·50	50·00	·00		One	14	·070	·078	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50	·05	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	2·50	·11	·06		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
				3·375	·15	—		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	3·50	·16	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Weights put on at intervals of 1½ min.
			1·00	4·50	·23	·07		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·50	5·00	·28	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	5·25	·30	·04		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	5·50	·32	·03		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	5·75	·35	·03		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·00	·40	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·25	·45	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·50	·55	·10		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·75	—	—		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·30 Breaking strain = 1·00
21 Dec.	36	2	·50	·50	50·00	·00		One	14	·070	·078	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50	·05	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	2·50	·10	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
				3·25	·13	—		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Weights put on at intervals of 1½ min.
			1·00	3·50	·15	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	4·50	·22	·07		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	5·50	·31	·09		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	5·75	·35	·04		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·00	·38	·03		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·25	·46	·08		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			·25	6·50	—	—		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·26 Breaking strain = ·92
21 Dec.	36	3	·50	·50	50·00	·00												
			1·00	1·50	·05	·05												
			1·00	2·50	·10	·05												
				3·25	·13	—												
			1·00	3·50	·15	·05												
			1·00	4·50	·22	·07												
			1·00	5·50	·32	·10												
			·25	5·75	·35	·03												
			·25	6·00	·40	·05												
			·25	6·25	·45	·05												
			·25	6·50	—	—												Broke fair. Per-centage of elongation :— ½ Breaking strain = ·26 Breaking strain = ·90
29 Dec.	37	1	·50	·50	50·00	·00		One	13	·087	·122	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50	·05	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	2·50	·09	·04		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	3·50	·13	·04		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	Weights put on at intervals of 1½ min.
			1·00	4·50	·15	·03		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	5·50	·19	·04		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	6·50	·24	·05		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	
			1·00	7·50	·32	·08		〃	〃	〃	〃	〃	〃	〃	〃	〃	〃	

App. No. 10.

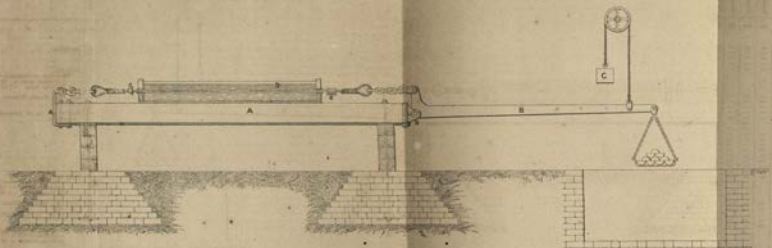
Appendix No. 10.—Falmouth and Gibraltar Telegraph—*continued.*

Results of Experiments made by Messrs. Gisborne and Forde and C. W. Siemens, upon the Strength of Steel and Iron Wires (Welded and Unwelded), and Hemp Strands, Separate and Combined—*continued.*

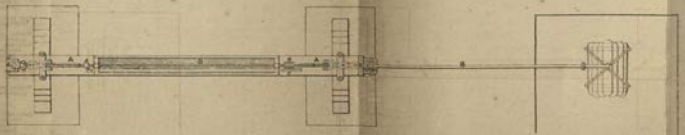
Date of Experiments.	Number of Specimen.	Number of Sample tested.	Weights. Put on.	Weights. Total.	Sample between Clamps. Length of	Elongation.	DESCRIPTION OF SAMPLE. Wire. Steel or Iron.	Wire. No. of Wires.	Wire. Gauge. Birmingham Wire Gauge.	Wire. Gauge. Decimals of Inch.	Wire. Weight per Fathom. Single Wire.	Wire. Weight per Fathom. Strand.	Hemp. No. of Strands. Bound each Wire.	Hemp. No. of Strands. Total.	Hemp. Length of Lay.	Hemp. Weight per Fathom.	Weight per Fathom, Wire and Hemp combined.	REMARKS.
1859:			*Cwts.*	*Cwts.*	*Ins.*	*Ins.*		*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	*No.*	*No.*	*Ins.*	*lbs.*	*lbs.*	
							NAKED WIRES.—UNWELDED.											
29 Sept.	37	1	·50	3·00	50·00	·38	Patent Steel.	One	13	·087	·122	—	—	—	—	—	—	Continued.
			·25	3·25		·42		"	"	"	"	—	—	—	—	—	—	
			·25	3·50		·46		"	"	"	"	—	—	—	—	—	—	
			·25	3·75		·53		"	"	"	"	—	—	—	—	—	—	
			·25	9·00	—	—		"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·30 Breaking strain = 1·05
29 Sept.	37	2	·50	·50	50·00	·00	Patent Steel.	One	13	·087	·122	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50		·04		"	"	"	"	—	—	—	—	—	—	
			1·00	2·50		·08		"	"	"	"	—	—	—	—	—	—	Weights put on at intervals of 1½ minutes.
			1·00	3·50		·11		"	"	"	"	—	—	—	—	—	—	
			1·00	4·50		·15		"	"	"	"	—	—	—	—	—	—	
				4·62														
			1·00	5·50		·20		"	"	"	"	—	—	—	—	—	—	
			1·00	6·50		·25		"	"	"	"	—	—	—	—	—	—	
			1·00	7·50		·32		"	"	"	"	—	—	—	—	—	—	
			·50	8·00		·40		"	"	"	"	—	—	—	—	—	—	
			·25	8·25		·44		"	"	"	"	—	—	—	—	—	—	
			·25	8·50		·49		"	"	"	"	—	—	—	—	—	—	
			·25	8·75		·55		"	"	"	"	—	—	—	—	—	—	
			·25	9·00		·65		"	"	"	"	—	—	—	—	—	—	
			·125	9·125		·73		"	"	"	"	—	—	—	—	—	—	
			·125	9·250	—	·10		"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·30 Breaking strain = 1·50
22 Dec.	38	1	·50	·50	50·00	·00	Salter's Steel.	One	16	·064	·065	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50		·05		"	"	"	"	—	—	—	—	—	—	Weights put on at intervals of 1½ minutes.
				1·73		·06												
			1·00	2·50		·11		"	"	"	"	—	—	—	—	—	—	
			1·00	3·56	—	·18		"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·12 Breaking strain = ·22
22 Dec.	38	2	·50	·50	50·00	·00	Salter's Steel.	One	16	·064	·065	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50		·06		"	"	"	"	—	—	—	—	—	—	
				1·63		·07												Weights put on at intervals of 1½ minutes.
			1·00	2·50		·15		"	"	"	"	—	—	—	—	—	—	
			·25	2·75		·19		"	"	"	"	—	—	—	—	—	—	
			·25	3·00		·23		"	"	"	"	—	—	—	—	—	—	
			·25	3·25	—	—		"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·14 Breaking strain = ·46
22 Dec.	38	3	·50	·50	50·00	·00	Salter's Steel.	One	16	·064	·065	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50		·06		"	"	"	"	—	—	—	—	—	—	
				1·625		·08		"	"	"	"	—	—	—	—	—	—	Weights put on at intervals of 1½ minutes.
			1·00	2·50		·15		"	"	"	"	—	—	—	—	—	—	
			·25	2·75		·19		"	"	"	"	—	—	—	—	—	—	
			·125	2·875		·20		"	"	"	"	—	—	—	—	—	—	
			·125	3·000		·22		"	"	"	"	—	—	—	—	—	—	
			·125	3·125		·24		"	"	"	"	—	—	—	—	—	—	
			·125	3·250		·25		"	"	"	"	—	—	—	—	—	—	
			·125	3·375	—	—		"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·16 Breaking strain = ·50
29 Dec.	39	1	·50	·50	50·00	·00	Salter's Steel.	One	13	·083	·111	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50		·01		"	"	"	"	—	—	—	—	—	—	
				2·25		·08												Weights put on at intervals of 1½ minutes.
			1·00	2·50		·09		"	"	"	"	—	—	—	—	—	—	
			1·00	3·50		·15		"	"	"	"	—	—	—	—	—	—	
			1·00	4·50	—	—		"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·16 Breaking strain = ·30
29 Dec.	39	2	·50	·50	50·00	·00	Salter's Steel.	One	13	·083	·111	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50		·05		"	"	"	"	—	—	—	—	—	—	
				2·25		·09												Weights put on at intervals of 1½ minutes.
			1·00	2·50		·10		"	"	"	"	—	—	—	—	—	—	
			1·00	3·50		·15		"	"	"	"	—	—	—	—	—	—	
			·25	3·75		·16		"	"	"	"	—	—	—	—	—	—	
			·25	4·00		·18		"	"	"	"	—	—	—	—	—	—	
			·25	4·25		·22		"	"	"	"	—	—	—	—	—	—	
			·25	4·50	—	—		"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·18 Breaking strain = ·44
29 Dec.	39	3	·50	·50	50·00	·00	Salter's Steel.	One	13	·083	·111	—	—	—	—	—	—	Sent from Messrs. Webster and Horsfall.
			1·00	1·50		·04		"	"	"	"	—	—	—	—	—	—	
				2·25		·08												Weights put on at intervals of 1½ minutes.
			1·00	2·50		·09		"	"	"	"	—	—	—	—	—	—	
			1·00	3·50		·14		"	"	"	"	—	—	—	—	—	—	
			·25	3·75		·16		"	"	"	"	—	—	—	—	—	—	
			·25	4·00		·19		"	"	"	"	—	—	—	—	—	—	
			·25	4·25		·23		"	"	"	"	—	—	—	—	—	—	
			·25	4·50		·30		"	"	"	"	—	—	—	—	—	—	Broke fair. Per-centage of elongation :— ½ Breaking strain = ·16 Breaking strain = ·65

MACHINE USED IN MESSRS GISBORNE & FORDE AND C.W. SIEMENS'

EXPERIMENTS FOR TESTING THE ELONGATION & BREAKING STRAIN OF TELEGRAPH CABLES.



SECTIONAL ELEVATION



PLAN

APPARATUS

USED FOR MEASURING THE ELONGATION & BREAKING-STRAIN OF SINGLE STRANDS OF WIRE.

Scale ½ of an Inch to a Foot.

APPENDIX No. 10—*continued.*

App. No. 10.

Machine used for testing the elongation and breaking strains of telegraph cables.

MACHINE used by Messrs. GISBORNE and FORDE, and C. W. SIEMENS for Testing the Elongation and Breaking Strain of TELEGRAPH CABLES.*

THIS machine consists of a strong beam A, resting on suitable supports and firmly secured to the ground. To one end of the beam is bolted a strong wrought iron angle plate *a*, carrying a hook for the purpose of attaching the cable to be tested, while the other end is provided with a suitable bracket *b*, which serves as a fulcrum to the weighing lever B.

This lever B, which is in the proportion of 1 : 10, carries on one extremity a scale pan, and, on the other, the second hook for attaching the cable. The scale pan, as well as the lever, are balanced by the counter weight C. The cable itself passes through the trough D, filled with water, for the purpose of testing it electrically, and also to observe the amount of shrinkage and other changes in cables covered wholly or partly with hemp. The two openings through which the cable passes are filled up with hemp, packing lightly tightened down by wedges.

The scale *c*, used for measuring the elongation of the cable passes through a pipe fixed in the trough, and is loosely connected on one side to a clamp *d*, fixed on the cable itself. The connexion is made by means of a circular flange on the clamp, which is fitted into a notch on the scale so as to allow the cable to twist itself without altering in any way the position of the scale. For the same reason a small cylinder *e*, is fixed on the cable so as to slide along the divided part of the scale, and this cylinder being divided as a vernier with circular divisions it is evident that the elongation can be read off with great accuracy, although the cable may twist itself while the weights are put on. It will be seen from the arrangement above described for moving the scale, that the elongation indicated by it is entirely independent of any stretching that may take place in the chains, joints, or other parts of the machine; nor can it be influenced by the slipping of the ends of the cable.

In proceeding to test a cable a small weight is first put into the scale for the purpose of straightening it; the vernier is then adjusted, and the weights put on at timed intervals. It will be seen from the drawing that the proportion of leverage will always remain as 1 : 10 in whatever position the lever may be.

To measure the elongation and breaking strain of single strands of wire, the simpler apparatus, shown in the drawing, is used. The wire is suspended at the top on a round beam F, by being twisted several times round it, and is fixed in the same manner at its lower extremity to a round block of wood G, which carries the scale pan.

A scale H is attached to the top of the wire, and a vernier *i* to the lower part, sliding along the divided portion of the scale H.

APPENDIX No. 11.

App. No. 11.

FALMOUTH AND GIBRALTAR CABLE.

Falmouth and Gibraltar telegraph cable.

Method adopted in manufacturing and testing.

MEMORANDUM OF METHOD adopted in manufacturing and testing the Government Cable designed to be laid between Falmouth and Gibraltar, about 1,100 knots direct distance.

IN May 1859, it was decided by Her Majesty's Government to lay down a telegraph cable between Falmouth and Gibraltar.

Messrs. Gisborne and Forde were appointed engineers to the Government, to carry out this undertaking, being already engineers to the Red Sea and India telegraph.

The core of the Red Sea telegraph, for lengths of 500 miles, is composed of a seven wire copper strand, weighing 180 lbs. per knot, covered with two coats of gutta-percha and two of Chatterton's compound, weighing per knot 212 lbs., making the total weight of the core 392 lbs. per knot; this was designed to work at the rate of 10 words per minute, through a circuit of 500 miles; the result proved that 12 words per minute could be sent through that length.*

In the case of the Falmouth and Gibraltar cable it was decided that from five to seven words per minute would be a fair practical speed. It was therefore designed to consist of a seven copper wire strand weighing 400 lbs. per knot, covered with three coverings of gutta-percha and three of Chatterton's compound, equal in weight to 400 lbs. per knot; the total weight of the core is therefore about 800 lbs. per knot, and measures about half an inch outside diameter.

Before finally deciding on this core the Government referred to the late Mr. Robert Stephenson and other eminent engineers, and their unanimous opinion was that the core was suitable for the purpose and superior to anything that had hitherto been designed.

The following tabular statement will prove interesting to show the progress made in this all important portion of a submarine cable. It shows the comparative weight and size, as well as the rate of speaking of the three longest cables that have been made:—

TABLE I.

Name of Line.	Date.	Length of Cable.		Weight and Size of Core.				Speed of Working.		Observations.
		Total from end to end.	In longest Sections.	Copper per Knot.	Insulator per Knot.	Total weight of Core per Knot.	Outside diameter of Core.	Through whole Line or longest Section.	Reduced to 500 miles circuit.	
		Knots.	Knots.	lbs.	lbs.	lbs.	Inches.	Words per minute.	Words per minute.	
Atlantic Telegraph -	Jan. 1857	2,000	2,000	107	261	368	·34	Doubtful.	8*	* As compared with the conductor of the Red Sea cable; the speed of working should be eight words per minute through 500 knots.
Red Sea and India Telegraph - -	Aug. 1858	3,548	730	180	212	392	·30	6·5	14	
Falmouth and Gibraltar Telegraph	May 1859	1,260 including about 15 per cent. slack.	1,260	400	400	800	·50	5·	31·7	

The Atlantic cable was tested for insulation by galvanometer readings, independent of temperature, and not reduced to any standard of resistance. The resistance of this core given in the following table has been reduced by the method adopted for the Red Sea and Gibraltar cables, as will be hereafter explained, the temperature being 60° Fahr. The tests were made at the Gutta-Percha Company's works.

* It may be interesting here to mention that previous to deciding on the Red Sea core, it was tested with and without Chatterton's compound, and the former invariably proved superior to the latter in the ratio of 2½ : 1. The Atlantic core was covered with three coatings of gutta-percha without any compound.

App. No. 11.

Falmouth and Gibraltar telegraph cable.

Method adopted in manufacturing and testing.

TABLE II.

Name of Telegraph.	Date when commenced.	Insulation Resistance.		Copper Resistance.		Ratio of Resistance of Gutta Percha as compared with the Conductor.	
		Per Knot.	For longest Section.	Per Knot.	For longest Section.		
		Millions of Siemens' Units.	Siemens' Units.	Siemens' Units.	Siemens' Units.	Per Knot.	Longest Section.
Atlantic Telegraph -	Jan. 1857	10	5,000	13·5	27,000	740,740 to 1	·185 to 1
Red Sea and India Telegraph -	Aug. 1858	80	41,096	8·3	6,059	3,614,444 to 1	6·8 to 1
Falmouth & Gibraltar Telegraph -	May 1859	330	261,904	3·65	4,599	90,400,000 to 1	57 to 1

This table shows the great progress made in the mode of putting on the insulator, which in all cables hitherto laid consists of more or less layers of gutta-percha with or without intermediate coatings of Chatterton's compound.

On making final arrangements for superintending the manufacture of the Falmouth and Gibraltar cable, the engineers specified that they should have the entire control of the cable, both during manufacture and laying, and that the most perfect system of electrical tests should be established in order that every portion of the cable might be thoroughly investigated, and its electrical condition fully ascertained as the work progressed.

The experience gained in testing the Red Sea and India telegraph cable during construction and laying, made it apparent that the system of electrical tests adopted by Messrs. Siemens, Halske, & Co. and the delicate instruments they manufacture for the purpose, were eminently successful in thoroughly investigating the various phenomena produced by atmospheric and terrestrial magnetism, humidity, temperature, &c., and which have hitherto rendered the investigation of the absolute resistance of the conductor and insulator so uncertain and unsatisfactory. The Government under the advice of their engineers, secured the services of Messrs. Siemens, Halske, & Co. as electricians.

The insulating covering of submarine wires is necessarily liable to defects, caused either by accident or imperfect manufacture, and the insulated wire has therefore always been tested electrically before sending it out.

The first method of testing was introduced by Messrs. Siemens & Halske in 1847, in preparing the wire for the underground lines then about to be laid down in Prussia.

It consisted of passing the coated wire through a bath of acidulated water in connexion with one pole of an induction apparatus, the body of the observer forming part of the circuit, which was completed by the second pole of the induction apparatus being connected with the line wire. It will be readily perceived that so long as the coating of the wire in passing through the bath was comparatively perfect, no induced current could pass from the wire to the bath, but the instant a fault occurred the high tension-induced currents would pass through the body of the observer producing a very sensible effect upon him.

For a final and more delicate test the coils of coated wire were then completely immersed, and their perfect state of insulation ascertained by means of delicate galvanometers.

Subsequently, galvanometer tests alone have been generally substituted. Each mile of insulated wire before leaving the gutta-percha works was subjected to two kinds of tests. The "*continuity test*" by metallic circuit including a battery and a sine or tangent galvanometer, and the "*insulation test*" by connecting one pole of a powerful battery to the copper of the insulated line wire, and connecting the outer covering (or water surrounding the coil of insulated wire) with the other pole of the battery, including a delicate sine galvanometer in the circuit. These tests were repeated at the sheathing works, both by the contractors and on behalf of the purchasers of the cable, and a record was kept of the deflections obtained.

During submersion of long submarine cables their electrical supervision has generally been entrusted to independent electrical engineers possessed of the mathematical and physical knowledge required to guide the paying-out engineers in his operations.

In the case of most of the Mediterranean cables, of the Red Sea and Indian, and of the Batavia, and Singapore cables, Messrs. Siemens, Halske, & Co. were employed in this capacity.

The system of testing hitherto employed is defective in many important points. The tests at the gutta-percha works and at the contractors' works having been entrusted to persons unconnected with the electricians finally employed in submerging the cable, the latter were debarred from the opportunity of acquiring an intimate knowledge of each part of the cable, or of carrying out a complete system of tests in which each operation should be the foundation of success. But supposing even that the results of the early tests were at the disposal of the electrician directing the final operations, they would have been of no avail to him, because these results consisted in angles of deflection of galvanometers which are subject to daily variations, and which were seldom compared with a standard. The continuity tests had been taken by a different description of instrument from the insulation tests, rendering it impossible to form a correct estimate of the comparative conductivity of the insulated conductor and the insulating medium. No account had been taken in the tests during manufacture of changes of temperature and of the time during which the current had been active before each observation was taken, although both temperature and time exercise a very large influence upon the results obtained.

In taking charge of the electrical department of the Falmouth-Gibraltar telegraph cable, Messrs. Siemens, Halske, & Co. have for the first time had the opportunity of carrying into effect their complete system of testing. The mathematical deductions upon which this system is based will be found in the Appendix, under the title of "Outline of the Principles and Practice involved in dealing with the Electrical Conditions of Submarine Electric Telegraphs," by Werner and C. W. Siemens, read before the British Association at Oxford, on the 3rd July 1860.

The chief object aimed at in these tests is to obtain strictly comparative results. For this purpose a unit of resistance is established by which the conductivity of both the copper and the insulating medium is expressed in definite and strictly comparative numbers. This has the advantage that when the separate coils are united in a continuous cable, the records of the tests of the first short lengths form a standard of comparison for the electrical condition of the whole, in comparing the total resistance of both the conductor and the insulating medium with the sum of the resistances previously obtained in testing each coil separately, due allowance being made for changes of temperature.

Between the limits of 40 degrees and 75 degrees Fahr. the conductivity of the insulating covering of the Falmouth-Gibraltar cable is found to increase in the ratio of 1 to 7. This increase, however, is by no means constant; it was therefore thought necessary to test the coils at the works of the Gutta-Percha Company at an uniform temperature of 75 degrees Fahr.

This comparatively high degree of standard temperature has the advantage that it is seldom exceeded in nature, and that the conductivity being seven times greater at that temperature than at the winter temperature of 40 degrees, the effect of minute faults upon the measuring instruments will also be proportionately exaggerated.

In order to insure uniformity of temperature, the coils to be tested at the gutta-percha works, being two knots in length and wound upon an iron drum, were placed for 24 hours in tanks containing water regulated to 75 degrees. This length of time was found to be absolutely necessary in

order to insure uniformity of temperature throughout the coil.

The coils were then removed into pressure tanks containing water of the same temperature, and tested both for conductivity and insulation, in the first instance under a vacuum of 25° on Bourdon's gauge, and afterwards under a pressure of 600 lbs. per square inch, with a view to forcing the water into any cavities or fissures that might exist.

The application of hydrostatic pressure sensibly decreases the conductivity of gutta-percha covering when perfectly sound, but in slightly defective coils no increase, and even decrease, of the insulating property is produced. A clue is thus obtained to ascertain otherwise inappreciable defects.

These precautions, however, do not yet suffice to obtain truly comparative results.

Gutta-percha, unlike a metal conductor, offers a variable resistance to the current according to the length of time it has been applied; and to avoid the errors arising from this cause, the principle has been adopted to take observations at definite periods of time after the action of the galvanic current has commenced, for which purpose minute glasses are employed.

As the effect of the electric action remains in the gutta-percha for some length of time, care has also to be taken that the gutta-percha has returned to its natural unchanged state before subsequent tests are applied.

The results thus obtained are expressed in units of resistance.

The measuring instruments employed consist of combinations of variable resistance coils of silk-covered German silver wire, arranged in the manner of a Wheatstone's bridge, in which the two permanent branches are also made changeable in order to obtain a larger range of measurements.

The gutta-percha resistance of very short lengths of cables surpassing the limits of the arrangement just described, is obtained by means of a very delicate sine galvanometer or a Weber's magnetometer, the deflections of which are reduced to units of resistance by means of a special formula, the constant of the instrument and the electro-motive force of the testing battery being measuredat frequent intervals.

It is now well understood that insulated conductors can only be properly tested when under water. Therefore the coils at the gutta-percha works were placed immediately after their manufacture under water in the canal, and a rough test was taken to find out any large defects. They were then wound upon iron drums and placed in the warming cisterns, thence into pressure tanks, and tested accurately in the manner before mentioned. The annexed Forms A. and B.* show the principle on which these tests at the gutta-percha works were recorded. Form A. contains the conductivity measurements of the copper wire; Form B. those of the insulating covering. Each coil has its own number recorded in col. 8; the observed resistance of the copper wire is noted down in col. 10. This observation is reduced to the uniform length of 1,000 yards in col. 11, and to a standard temperature of 20 degrees centigrade in col. 12. These records of the comparative results of the conductivity of each portion of the cable serve, during and after completion of the cable, as a basis for all subsequent tests, and are of the greatest practical value.

The Roman figures in Form B. give the tests taken before the pressure was applied, and the *Italic* figures mark those taken under pressure.

To the measurements of the resistances of the insulating coverings those of charge and discharge are added, as a further control to the preceding results, and for the sake of appreciating eccentricities of the wire, coils not giving the proper results in all these tests were set aside. The minimum standard of the gutta-percha resistance for the Gibraltar cable had been fixed at one hundred millions of units of resistance per knot, at the temperature of 75 degrees Fahr. The perfect coils were put back again into the canal until they were taken to Messrs. Glass, Elliot, & Co.'s works at Greenwich. Having arrived there they were at once placed again under water, until required to be covered with hemp and iron.

After leaving the sheathing machine the cable as completed was to be coiled and kept in water-tight tanks. During the manufacture of the cables as well as after their completion they were tested daily. Form C. shows how these tests were recorded.

* Pages 5 and 6.

App. No. 11. Falmouth and Gibraltar telegraph cable.

Method adopted in manufacturing and testing.

The numbers of the coils composing the cable are noted down in column 9; the observed resistance of the copper wire is given in column 13, and the reduced resistance to the standard temperature in column 14. Column 15 gives the resistance of the same length of cable, which follows from the tests taken at the gutta-percha works, and which ought to coincide with the measured resistance to prove the satisfactory condition of the conductor.

The measured resistances of the gutta-percha covering when both positive and negative currents are applied are given in degrees in columns 18 and 19. The resistance in units is given in column 21, and the resistance per knot in column 22. The tests of the charges and discharges of the cables are given in columns 23 to 27.

The tanks at Messrs. Glass, Elliot, & Co.'s works proved unfortunately defective, and in consequence of the cable becoming partially exposed to atmospheric influence spontaneous generation of heat was observed.

Warned by this observation, Messrs. Siemens, Halske, & Co. constructed certain instruments, called by them resistance thermometers, which they placed at intervals between the layers of the cable when coiled on board ship, for the purpose of ascertaining at all times the temperature of the interior of the mass, in order that any dangerous accumulation of heat might be checked in time.

These resistance thermometers are based on the well-ascertained fact that the conductivity of copper wire increases in a simple ratio inversely with its temperature. They consist of silk-covered copper wire, wound upon a metal rod, about 18 inches long, and offering at the temperature of 20 degrees Celsius, a resistance of about 100 units. The wire is covered for protection by india-rubber and gutta-percha, and hermetically sealed.

The two ends of each thermometer are led into the electrician's room on board, by means of insulated conducting wires, and the resistance of each coil is ascertained by a special measuring apparatus.

As the resistance of such a thermometer would undergo an increase of 0·21 units of resistance, if its temperature should rise one degree Fahr.; and as the system for measuring the resistance permits of appreciating one tenth or even one hundredth part of a unit, it is not difficult to calculate the temperature of the cable at different points throughout its mass with the greatest accuracy.

The first tests taken on board the S.S. "Queen Victoria," after the cable in the ship's hold had been kept dry for 12 consecutive days, proved that the cable had heated internally to above 80 degrees Fahr., although mercury thermometers applied externally only gave a maximum temperature of 60 degrees.

The rate of increase of temperature was found to be about 3 degrees Fahr. per day; and considering that gutta-percha softens at 92 degrees Fahr., it follows that this cable might have been rendered unfit for use if the heating had been allowed to proceed many days longer. This danger would unquestionably have been avoided, if the original plan of placing the cable in water-tight tanks on board ship had been enforced. The temperature of the cable could only be reduced by daily watering, a circumstance which delayed the departure of the expedition.

Form D. shows a record of the tests of the cable on board the ship "Queen Victoria" while in this condition.

Columns 1, 2, and 3 give the date and time of observation. Columns 4 to 10 contain the regular insulation tests of the cable. Columns 11 and 12 the continuity resistances in metallic circuit and with earth. Column 13 gives the calculated average temperature according to the observed copper resistance; columns 14 to 17 contain the readings of three resistance thermometers and the temperature calculated therefrom; column 18 contains general remarks.

In order to prevent this dangerous generation of heat and the destruction of the iron covering by the joint action of air and water upon it, Messrs. Siemens, Halske, & Co. proposed to place strips of zinc at regular intervals between the layers of the cable, which by galvanic action with the iron have been proved by experiment to prevent to a great extent the oxidation of the more negative constituent.

The cable having been tested in this manner during all the stages of its manufacture, has to be tested subsequently in the same manner during submersion.

The chief care during the submersion is to detect at once the slightest change in the insulation, in order that the paying-out machinery may be stopped instantly. The following is the plan here pursued.

A clock-work arrangement at the land station is made to put the cable by rotation to earth, to the poles of a battery and to insulation.

App. No. 11.

Falmouth and Gibraltar telegraph cable.

Method adopted in manufacturing and testing.

On board the ship a balance of resistances is always in connexion with the line. The electrician, by keeping the balance simply in equilibrium, is enabled to ascertain alternately the resistances of insulation and continuity, according to the connexion made by the contact arrangement on land.

The attendant of the station likewise observes these two data and transmits them periodically to the ship. If these four tests differ materially they indicate the existence of a fault the position of which can be calculated from the data obtained.

Means are provided to repeat these tests after the line is completed, in order to watch and record its electrical condition daily, by which means slight changes in the condition of the line will be observed, and proper measures adopted in time to avoid serious stoppages.

Prior to deciding upon the form of the outer covering, the Government, at the request of their engineers, sanctioned a series of experiments to determine the relative proportions between strength and weight of various forms of outer covering and the amount of elongation due to known strains.

The results of these experiments are given in Appendix No. 10.

The core to be covered in this case is about half an inch in diameter, which is but little less than the outside diameters of the Atlantic and Red Sea cables. The maximum depth of water between Falmouth and Gibraltar is about 2,700 fathoms.

The outer covering for this great depth became a matter of the utmost importance, and the result of the experiments prove that a combination of steel and hemp possesses lightness and strength with less tendency to elongate than any form of covering hitherto adopted; although this form of covering was not eventually carried out on this cable in consequence of its destination being altered to Rangoon and Singapore, yet it has been tested in a cable laid between Toulon and Algiers for the French Government, which was made similar to that designed by Messrs. Gisborne and Forde, and manufactured under their superintendence.

The following specification was, upon all these data, drawn up and approved of by the Government, and tenders were invited for the covering of the core.

Falmouth and Gibraltar Telegraph.

Specification for the Manufacture and Laying of a Submarine Telegraph between some Point on the British Isles and Gibraltar.

The cable is to be made according to the following specification:—

Specification.

1. Shore End, No. 1.

Core { Copper 7-wire strand, weighing per knot	400	lbs.
Gutta percha and compound	400	„
Total	7·15	cwt.
Best Petersburg clean hemp yarn serving, steeped in best Archangel tar	10·18	cwt.
12 wires, best selected charcoal iron, No. 3 gauge	120·62	„
Total weight per knot	137·95	„
	= 6·897	tons.

Shore End, No. 2.

Core same as above	7·15	cwt.
Best Petersburg clean hemp yarn serving, steeped in best Archangel tar	5·60	„
12 wires, best selected charcoal iron, No. 5 gauge	85·81	„
Total weight per knot	98·56	„
	= 4·928	tons.

Deep Water, No. 1.

Core same as above	7·15	cwt.
Best Petersburg clean hemp yarn serving, steeped in best Archangel tar	2·00	„
18 wires, best selected charcoal iron, No. 11 gauge	33·56	„
Total weight per knot	42·71	„
	= 2·135	tons.

Deep Water, No. 2.

Core same as above	7·15	cwt.
Core to receive a thin serving of best Petersburg clean hemp yarn well saturated with best Archangel tar	1·78	„
Outer covering to consist of steel and hemp yarn strand, the latter to be steeped in best Archangel tar, and twisted round each iron wire in the following proportions, viz.:—Hemp yarn strands steeped in tar	6·00	„
12 steel wires, No. 14 guage	11·37	„
Total weight per knot	26·30	„
	= 1·315	tons.

Specimens of all the above-mentioned cables are to be seen at Messrs. Gisborne and Forde's offices, No. 6, Duke Street, Adelphi.

In case it should be deemed advisable to cover the shore ends of the cable Nos. 1 and 2, and the deep water portion No. 1, with hemp and tar or marine glue, or other covering to protect the iron wires from decay, the contractor will be required to cover them according to the specification of the engineer, and at a price to be settled between the Government and the contractor, or in case of difference by Mr. Robert Stephenson.

2. The gutta-percha covered wire or core is to be supplied to the contractor by the Government at the Gutta-Percha Company's works, in accordance with the specification of the engineer, but the contractor will have to satisfy himself that the core at the Gutta-percha works is in accordance with a standard knot to be supplied by the engineer, and will have to bear all expenses attending its removal to the contractor's works, and be held responsible for any mechanical or other injury the core may receive after it is handed over to him. The contractor to supply good and sufficient iron drums capable of containing two knots in length of core. The core must be forwarded from the Gutta-percha works to those of the contractor upon these drums and be so placed in the pressure tank for testing.

3. The core to be subject in every instance to the approval of the contractor, the engineer, and the electrician, and to be tested in pressure tanks capable of being exhausted of air. Each two knots shall be placed in the apparatus, the air shall then be exhausted to not less than 25 inches on the vacuum gauge, after which the pressure of the water is to be raised to 1,000 lbs. to the square inch, which pressure is to remain on not less than ten minutes.

4. Any part of the core in which defects in manufacture may be discovered when tested under pressure shall be returned to the Gutta-Percha Company and replaced by them at their own cost.

The core after being tested under pressure must be sent to the serving machine, and it is desirable that the core shall be kept immersed in water until it is taken to be tested under pressure.

5. The length of cable required will be as follows, viz.:—

60 knots	shore ends, No. 1 to	50	fathoms depth.
60 „	„ „ No. 2 „	100	„ „
940 „	deep water No. 1 „	600	„ „
360 „	„ „ No. 2 „	2,600	„ „

These distances include 20 per cent. for slack.

6. In the case of No. 2 deep-water cable, the manufacture of it must be delayed until after the 31st December 1859, and in the interim the Government reserve to themselves the right to alter the class of covering should they desire to do so, it being understood that whatever covering is finally adopted, the cable shall be able to bear 5,000 fathoms of its own weight in water, with a maximum elongation of one per cent.

In case the covering is altered so as to necessitate the employment of a greater or lesser quantity of the same material, or a cheaper or more costly material, the difference in price is to be settled between the contractors and the engineer, or in case they differ, the matter is to be referred to the arbitration of Mr. Robert Stephenson, whose decision is to be final.

7. As the cable progresses the finished portion in each tank is to be tested under water, and the engineer and the electrician, or any person they may appoint, are to have the right of inspecting and testing the cable and materials during manufacture, and while laying, in such manner and at such times as they may think fit, so as not to interfere with the progress of the work.

8. In joining the different lengths of core, the contractor will be required to use Chatterton's compound immediately over the conductor and between each coating of gutta-percha; the joints to be made in every respect similar to samples to be seen in Messrs. Gisborne and Forde's office. In making the joints in the outer covering, the iron or steel must be most carefully and neatly welded or joined together as required by the engineer; breaking joint at least five feet, and the outside hemp yarn covering carefully interwoven and spliced by hand, so that the whole shall be nearly as strong as the portion not jointed.

9. The breaking strain of No. 2 deep-sea sample, made with steel wire and hemp, must not be less than five tons, and it must be capable of bearing a strain of 50 cwts. with no greater elongation than 0·75 per cent. of its length.

10. The engineer or the electrician shall be at liberty during the manufacture, and at any time, to take any portion of the cable for the purpose of testing it, and satisfying themselves that the cable is being made according to the specification.

11. The contractor is to supply two first-class screw steamers of sufficient tonnage, including coals, crew, and all supplies and wages, and fit them out with all necessary machinery, instruments, and appliances for laying the cable and picking it up; each ship to be supplied with four buoys of different dimensions, two of which must be capable of buoying the cable 2,500 fathoms if necessary.

Five thousand fathoms of five to seven inch hawsers to be also put on board of each ship, as well as six grappling irons and six mushroom anchors with 2,000 fathoms of four-inch hawsers. When putting the cable on board every knot in length must be carefully marked with a leather label, upon which the number of knot must be legibly branded.

The direction in which the cable shall be laid will be as near as possible on the line marked on the chart by the engineer.

12. The contractors shall lay the cable and maintain it for 30 consecutive days in good working order, to the satisfaction of the engineer, in respect of insulation and capability of transmitting at least five words per minute upon Siemens' improved recording instrument, or such other instrument as may at the time be approved of by the engineer.

13. The contractor shall at his own expense connect the shore ends of the cable with the land stations and instruments, but in case the aggregate length of these exceed a statute mile, the extra length will be paid for in accordance with a price per statute mile, to be stated in the tender.

The wire to be used in the land connexions of this telegraph to be of the best description of iron (No. 6 Birmingham wire gauge), fixed on to Siemens' insulators, specimens of which may be seen in the engineer's office.

The poles are to be of well-seasoned larch, round, with a smooth surface, straight, and perfectly sound, to be in length from 18 to 20 feet, and not less than five inches in diameter at top; the bottom portion from six to seven feet in length, to be properly charred, and then to receive a good coating of the best coal tar. They are to be placed perpendicularly and firmly at least four and a half feet into the ground, and from 40 to 80 yards apart, according as the line is straight or curved. Straining poles and insulators must be placed at intervals of half a mile, as well as at each end of the land connexions.

The insulators are to be fixed to the poles in a permanent and workmanlike manner, and the wire is to be carefully hung and well jointed according to sample in the engineer's offices.

Houses or any fixed points may be substituted for poles, subject to the approval of the engineer or a person appointed by him to lay out the direction of the land connexions.

Proper lightning conductors and testing posts at each end of the cable shall also be provided by the contractor.

14. The materials and workmanship as well as the ships, the machinery, and all appliances are to be of the very best description, and to be subject to the approval of the engineer, whose decision in all cases is to be final and binding on the contractor.

15. The cable is to be finished and placed on board ship, and all preparations of every kind are to be completed by the 1st June 1860.

The Government will supply two steamers, if required, to assist the vessels in the operation of laying the cable.

16. The contractors shall, at the cost of the Government, insure the cable against risk of damage by sea and fire, to the full amount of the advances made, the policies to be effected in the name of Her Majesty's Government, or any person they may appoint, and to be placed in the hands of the solicitor of the Treasury or other person appointed by the Government.

17. The payments for the cost of covering the cable to be made monthly for the number of nautical miles finished in accordance with Clause 5, and certified by the engineer, less five per cent., to be retained until the cable shall have been certified as having been laid and maintained in working order, in accordance with Clause 12.

The payment for laying the cable to be made after the cable has been certified by the engineer to have been laid and maintained in working order in accordance with Clause 12.

The contractor to give security to the amount of 20,000*l.* to the satisfaction of the solicitor to the Treasury, that the cable shall be safely laid and maintained in working order, in accordance with Clause 12, which will be forfeited in case of failure.

Any quantity of cable which may prove to be requisite beyond the lengths specified in Clause 5, shall be supplied at the contractor's cost.

Any surplus cable remaining after the submerged portion is taken over by the Government shall be the property of the contractor, but the engineer shall be satisfied that the quantity of slack paid out is sufficient to prevent any injurious strain upon the cable.*

18. A formal contract, with proper securities for its completion, will have to be entered into between Her Majesty's Government and the contractor whose tender is accepted.

(Signed) GISBORNE and FORDE,
6, Duke Street, Adelphi, W. C.

Sept. 1, 1859.

When the destination of this cable was changed, the hemp and steel covering specified for the very deep sea, was not carried out, the maximum depth between Rangoon and Singapore being under 100 fathoms. The rest of the cable was so far advanced in manufacture that it had to be adopted, although not designed for this locality, nor perhaps is it the most economical or best form that might have been designed originally for this eastern line.

Various causes prevented this cable being shipped in time for laying before the setting in of the monsoon, and its destination has been again changed, the proposal being now to lay it as a coast line from Malta to Alexandria.

(Signed) GISBORNE and FORDE.
Jan. 16, 1861. SIEMENS and HALSKE.

* This Clause was subsequently altered, and now provides that all surplus cable shall remain the property of the Government.

App. No. 11.

Falmouth and Gibraltar telegraph cable.

Registry of tests (continuity). Form A.

Appendix No. 11—*continued.*

Form A.—Falmouth and Gibraltar Telegraph.

Electricians' Department.

Registry of Tests applied to Copper Wire (Continuity).

Time.			Temperature.				Coils Tested.		Resistance of Copper Wire.			Remarks.
			Of Appar.		Of Wire.					Per 1,000 Yards. $\left(\frac{C}{L} \cdot 1000\right)$		
Month.	Day.	Hour.	Cent.	Fahr.	Cent.	Fahr.	No.	Length. L.	Total. C.	Obs.	At 20° Cent.	
1	2	3	4	5	6	7	2	9	10	11	12	13
			°	°	°	°		Yards.	Units.	Units.	Units.	
October	2	,,	65	18	75	24	1,089	1,992	7·37	1·86	1·83	All these Coils were tested for Continuity after having been tested for Insulation. They had not been presented before to be tested singly, as usual, which will not, however, be of any consequence.
							1,090	1,985				
							1,091	1,995				
							1,092	1,994				
,,	4	,,	63	17	75	24	1,093	1,972	7·12	1·83	1·80	
							1,094	1,990				
							1,095	1,981	7·32	1·86	1·83	
							1,096	1,945				
							1,097	1,971	7·20	1·78	1·74	
							1,098	2,082				
							1,099	1,991	7·10	1·79	1·75	
							1,100	1,983				
,,	5	,,	63	17	75	24	1,101	1,979	7·35	1·86	1·83	
							1,102	1,981				
							1,103	2,004	7·30	1·86	1·83	
							1,104	1,929				
							1,105	2,005	7·20	1·84	1·81	
							1,106	1,966				
							1,107	1,998	7·29	1·82	1·71	
							1,108	1,976				
,,	6	,,	65	18	75	24	1,109	1,999	7·20	1·82	1·79	
							1,110	1,985				
							1,111	1,959	7·26	1·83	1·80	
							1,112	1,984				
							1,113	1,847	6·75	1·78	1·74	
							1,114	1,937				
							1,115	1,969	7·18	1·82	1·70	
							1,116	1,972				

Form B.—Falmouth and Gibraltar Telegraph.

Registry of tests of insulation. Form B.

Electricians' Department.—Registry of Tests of Insulation.

N.B.—The *Italic* figures refer to the tests in the pressure tanks.

Date.		Temperature.				Number		Length. l	Number of Cells. n	Const. of Instru. i	Insulation.			Observed.			Reduced.			Disch. 1—Ch.		Remarks.
		Room.		Water.		of Test.	of Coils.				¢ +	¢ −	Resistance per knot. $\frac{\sin.p'}{\sin.¢} \cdot \frac{l}{2059}$	Charge. Ψ	Discharge. 1st	Discharge. 2nd	$\sin.\frac{\Psi}{2}\sqrt{\frac{2029}{n\,l}}\,10^{4}$ Charge.	$\sin.\frac{\Psi}{2}\sqrt{\frac{2029}{n\,l}}\,10^{4}$ 1st Dis.				
Month.	Day.	F.	C.	F.	C.																	
1	2	3	4	5	6	7	8	9	10	11	13	13	14	15	16	17	18	19		20		21
		°	°	°	°			Yards.					Millions of Units.	°	°	°						
Oct.	2	65	18	75	24	383	1089/90	3980	100	13·3	25·3	21·7	113	9·5	2·4	—	18·45	4·66	0·747		24	
											20·6	*21·3*	*120*	*9·3*	*2·0*	—	*18·46*	*4·64*	*0·695*			*
,,	,,	,,	,,	,,	,,	384	1091/92	3950	,,		24·5	23·1	113	9·8	2·5	—	19·03	4·86	0·745		,,	
											22·8	*22·8*	*122*	*9·8*	*2·9*	—	*19·03*	*4·64*	*0·697*		,,	
,,	,,	,,	,,	,,	,,	385	1089/90	3960	,,		24·4	20·8	117	9·5	2·6	—	18·45	5·05	0·726		,,	
						386	1091/92	3950			24·7	21·6	114	9·8	2·5	—	19·03	4·86	0·745		,,	†
,,	4	68	17	75	24	387	1093/94	3881	,,	13·4	22·1	23·6	116	9·5	2·4	—	18·68	4·72	0·747		,,	
											20·0	*20·5*	*128*	*9·5*	*2·3*	—	*18·68*	*4·51*	*0·705*		,,	
,,	,,	,,	,,	,,	,,	388	1095/96	3924	,,	,,	22·1	23·8	117	9·5	2·3	—	18·46	4·48	0·768		,,	
											20·6	*20·5*	*122*	*9·5*	*2·0*	—	*18·46*	*4·64*	*0·694*		,,	
,,	,,	,,	,,	,,	,,	389	1089/90	3960	,,	,,	23·8	25·3	109	9·8	2·3	—	18·88	4·24	0·776		,,	
											20·3	*20·3*	*120*	*9·8*	*2·3*	—	*18·88*	*4·38*	*0·725*		,,	‡
,,	,,	,,	,,	,,	,,	390	1091/92	3952	,,	,,	24·8	25·6	106	9·0	2·3	—	18·68	4·44	0·707		,,	
											21·2	*23·2*	*120*	*9·0*	*2·0*	—	*18·68*	*4·20*	*0·707*		,,	
,,	,,	,,	,,	,,	,,	391	1097/98	4053	,,	,,	21·2	21·2	128	10·1	3·5	—	19·01	4·03	0·883		,,	
											19·7	*19·0*	*140*	*10·1*	*3·8*	—	*19·01*	*3·46*	*0·683*		,,	
,,	,,	,,	,,	,,	,,	392	1099/100	3973	,,	11·8	19·9	21·6	126	10·0	3·1	—	19·31	4·26	0·690		,,	
											17·5	*18·3*	*148*	*10·0*	*3·6*	—	*19·31*	*3·73*	*0·680*		,,	
,,	5	63	17	75	24	393	1101/2	3957	,,		21·6	22·8	105	9·1	2·6	—	19·59	5·09	0·714		,,	
											19·4	*20·2*	*116*	*9·1*	*2·9*	—	*19·59*	*4·43*	*0·668*		,,	
,,	5	63	17	75	24	394	1103/4	3933	,,	,,	22·2	23·3	102	9·0	2·2	—	19·79	4·84	0·755		,,	
											20·1	*20·2*	*118*	*9·0*	*2·5*	—	*19·99*	*4·50*	*0·722*		,,	
,,	,,	,,	,,	,,	,,	395	1105/6	3971	,,	,,	21·8	23·8	105	9·1	2·3	—	19·83	5·07	0·714		,,	
											19·6	*20·2*	*118*	*9·1*	*3·0*	—	*19·83*	*4·64*	*0·670*		,,	
,,	,,	,,	,,	,,	,,	396	1107/8	3974	,,	,,	22·2	23·2	114	9·2	2·8	—	20·02	5·10	0·695		,,	
											19·0	*19·2*	*118*	*9·2*	*3·0*	—	*20·02*	*4·54*	*0·674*		,,	
,,	6	65	18	75	24	397	1109/10	3949	,,	13·3	23·0	25·3	101	9·3	2·0	—	19·55	4·21	0·785		,,	
											21·3	*23·4*	*118*	*9·3*	*2·6*	—	*19·55*	*3·47*	*0·720*		,,	
,,	,,	,,	,,	,,	,,	398	1111/12	3953	,,	,,	23·6	24·6	102	9·3	2·2	—	19·63	4·66	0·763		,,	
											20·9	*21·5*	*114*	*9·3*	*2·4*	—	*19·63*	*3·99*	*0·748*		,,	
											24·3	24·5	109	9·3	2·3	—	19·55	4·83	0·733		,,	
,,	,,	,,	,,	,,	,,	399	1109/10	3949	,,		24·0	24·5	100	9·3	2·2	—	19·63	4·96	0·763		,,	
,,	,,	,,	,,	,,	,,	400	1111/12	3928	,,		23·7	24·0	97	9·0	2·1	—	20·06	4·80	0·761		,,	
,,	,,	,,	,,	,,	,,	401	1113/14	3784	,,	12·1	*20·6*	*21·5*	*109*	*9·0*	*2·2*	—	*20·06*	*4·01*	*0·755*		,,	
											23·2	24·4	101	9·0	2·1	—	19·27	4·60	0·766		,,	
,,	,,	,,	,,	,,	,,	402	1115/16	3941	,,	,,	*21·1*	*21·5*	*118*	*9·0*	*2·6*	—	*19·27*	*3·67*	*0·744*		,,	

* We ordered these coils to be retained to be tested again, as the tests made with the negative current do not show the proper improvement when tested under pressure. The coils had been exposed to strong currents by the G. P. O. a little previous to our tests, which circumstances will have caused this irregularity.

† Tests repeated after the pressure had been taken off.

‡ Second tests of the coils Nos. 1089/1092. The first had been made on the 2nd of October. The coils not having been exposed to any current for 24 hours previous to our tests of this day, gave now the proper results.

§ Tests repeated after pressure.

‖ Retained on account of its insulation being below the standard of 100.

APP. No. 11.

APPENDIX No. 11—*continued.*

Rangoon and Singapore telegraph cable.

FORM C.—RANGOON AND SINGAPORE (LATE FALMOUTH AND GIBRATAR) TELEGRAPH.

Registry of tests during manufacture. Form C.

ELECTRICIANS' DEPARTMENT.—REGISTRY OF TESTS DURING MANUFACTURE OF CABLES.

Date. Month.	Day.	Temperature. Room. F.	Room. C.	Water. F.	Water. C.	Number Of Test.	Of Cable.	Length. Addit. Increase No.	Addit. Increase Lgth.	Total.	W	Obs.	Resistance of Wire. r, at 29°	Sum C at 20°	No. of Elem. n	Const. of Instrum. ρ	Resistance of Insulation. ρ +	ρ −	ρ Dif.	IR = sin ρ / sin ρ₂	Per Knot. Ir = IR . l	Charge. Obs.	Charge. Red.	Discharge. Obs.	Discharge. Red.	Loss. Disch. On.	Loss. 1 — Disch. On.	Electromotive force of Battery.	REMARKS.
1	2	3	4		6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
									Yds	Knots			Units.	Units.						Millions	Millions								
Oct.	2	64	17·7	56	13·3	911	1	897	1841	141·44	00/000	504·6	518·3	516·1	12	11·7	5·6	6·0	0·4	2·67	368	15·0	16·1	9·0	9·7	·398	11·3		
	2	65	18·3	57	13·8	922	,,	,,	,,	,,	,,	505·4	518·3	,,	,,	11·7	5·5	5·8	0·3	2·67	369	15·0	16·6	9·0	10·0	·398	11·5		
	4	66	18·8	66	13·8	935	,,	,,	,,	,,	,,	505·3	519·3	,,	,,	11·7	5·5	5·9	0·5	2·53	351	15·0	17·5	11·0	12·0	·265	11·2		
	5	66	18·8	57	13·8	944	,,	,,	,,	,,	,,	505·1	517·7	,,	,,	14·1	7·5	7·1	0·4	2·67	368	15·0	14·0	8·5	8·0	·402	11·8		
	6	63	20·0	57	13·8	955	,,	,,	,,	,,	,,	504·7	517·3	,,	,,	11·0	5·6	6·2	0·6	2·49	344	15·0	18·0	11·0	13·2	·265	11·4		*
Oct.	2	64	17·7	56	13·3	912	2 &7	,,	,,	87·16	00/000	312·5	320·9	318·1	12	11·7	4·3	4·4	0·1	3·56	316	10·0	17·0	5·0	8·6	·500	11·3		
	3	65	18·3	56	13·3	923	,,	,,	,,	,,	,,	312·7	321·1	,,	,,	11·2	4·3	4·3	0·0	3·51	306	9·0	15·9	6·0	10·5	·333	11·5		
	4	66	18·8	56	13·3	934	,,	,,	,,	,,	,,	311·3	320·6	,,	,,	11·1	4·2	4·3	0·1	3·42	298	10·0	15·5	6·0	11·1	·399	11·2		
	5	66	18·8	57	13·8	945	,,	,,	,,	,,	,,	311·0	318·8	,,	,,	14·1	4·2	4·9	0·0	3·96	345	10·2	15·2	5·0	7·5	·569	11·8		
	6	63	20·0	57	13·8	956	,,	,,	,,	,,	,,	310·7	313·5	,,	,,	11·0	3·9	4·2	0·3	3·92	316	10·0	19·1	5·5	10·5	·449	11·4		
Oct.	3	64	17·7	,,	,,	913	3	,,	,,	130·78	00/000	478·6	,,	477·4	12	11·7	11·5	10·0	0·5	1·45	160	15·0	15·2	6·0	6·1	·399	11·3		
	4	66	18·3	,,	,,	924	,,	,,	,,	,,	,,	475·2	,,	,,	,,	11·2	10·1	10·6	0·5	1·46	162	15·0	15·6	6·0	6·3	·399	11·5		
	5	66	18·8	,,	,,	935	,,	,,	,,	,,	,,	475·2	,,	,,	,,	11·1	9·5	10·3	0·8	1·48	193	15·0	16·5	7·0	7·7	·532	11·2		
	6	66	18·8	,,	,,	946	,,	,,	,,	,,	,,	476·2	,,	,,	,,	14·1	10·5	10·6	0·1	1·85	241	14·9	13·1	7·0	6·2	·529	11·8		
	6	63	20·0	,,	,,	957	,,	,,	,,	,,	,,	474·9	,,	,,	,,	11·0	9·3	10·2	0·9	1·51	193	15·0	16·9	7·0	7·9	·332	11·4		†
Oct.	2	64	17·7	,,	,,	914	4	,,	,,	166·17	00/000	600·5	,,	606·3	12	11·7	9·0	10·2	1·2	1·62	209	18·0	16·0	10·0	9·9	·443	11·3		
	3	65	18·3	,,	,,	925	,,	,,	,,	,,	,,	600·9	,,	,,	,,	11·2	9·6	10·0	0·4	1·53	257	18·0	16·5	10·0	9·2	·443	11·5		
	4	66	18·8	,,	,,	936	,,	,,	,,	,,	,,	600·2	,,	,,	,,	11·1	9·3	10·0	0·7	1·62	252	18·0	17·4	10·0	9·7	·443	11·2		
	5	66	18·8	,,	,,	947	,,	,,	,,	,,	,,	599·6	,,	,,	,,	14·1	10·0	10·4	0·4	1·91	317	18·0	13·9	10·0	7·8	·443	11·8		
	6	63	20·0	,,	,,	958	,,	,,	,,	,,	,,	600·1	,,	,,	,,	11·0	8·8	10·1	1·3	1·66	269	18·0	17·9	11·0	9·9	·443	11·4		
Oct.	2	64	17·7	56	13·3	915	5a	,,	,,	39·12	00/000	141·2	145·0	142·8	12	11·7	3·4	3·8	0·1	4·48	175	4·5	17·1	3·0	11·4	·333	11·3		
	3	65	18·3	56	13·3	926	,,	,,	,,	,,	,,	141·1	144·9	,,	,,	11·2	3·4	3·5	0·1	4·67	179	4·5	17·6	3·0	11·5	·333	11·5		
	4	66	18·8	56	13·3	937	,,	,,	,,	,,	,,	140·3	144·1	,,	,,	11·1	3·2	3·1	0·1	4·76	187	4·5	18·6	3·0	12·4	·333	11·2		
	5	66	18·8	57	13·3	948	,,	,,	,,	,,	,,	140·2	143·8	,,	,,	14·1	2·7	3·0	0·3	4·75	264	4·5	14·9	3·2	9·9	·356	11·8		
	6	63	20·0	56	13·3	959	,,	,,	,,	,,	,,	140·1	143·9	,,	,,	11·0	3·6	2·5	1·1	4·06	182	4·5	19·1	3·0	12·7	·333	11·4		
Oct.	2	64	17·7	56	13·3	916	5b	,,	,,	57·15	00/000	182·9	186·9	186·6	12	11·7	3·6	2·8	0·2	5·73	293	6·0	17·4	3·0	8·8	·500	11·3		
	3	65	18·3	56	13·3	927	,,	,,	,,	,,	,,	182·6	187·2	,,	,,	11·2	2·6	2·6	0·0	5·79	296	5·5	16·5	3·0	9·0	·454	11·5		
	4	66	18·8	56	13·3	938	,,	,,	,,	,,	,,	182·1	186·7	,,	,,	11·1	2·4	2·7	0·3	5·70	293	5·7	18·0	3·0	9·6	·476	11·2		‡
	5	66	18·8	57	13·8	949	,,	,,	,,	,,	,,	182·2	186·8	,,	,,	14·1	3·1	3·6	0·1	6·35	325	6·1	15·2	2·9	7·3	·517	11·8		
	6	63	20·0	56	13·3	960	,,	,,	,,	,,	,,	181·9	186·8	,,	,,	11·0	2·4	2·5	0·1	5·99	306	6·0	19·5	4·0	13·9	·333	11·4		
Oct.	2	64	17·7	56	13·3	917	5c	,,	,,	72·69	00/000	261·7	268·7	266·3	12	11·7	4·1	4·2	0·1	3·73	271	8·0	16·4	4·0	8·2	·500	11·3		
	3	65	18·3	56	13·3	928	,,	,,	,,	,,	,,	260·0	268·5	,,	,,	11·2	4·0	4·6	0·6	3·51	255	8·0	16·9	4·0	8·5	·500	11·5		
	4	66	18·8	56	13·3	939	,,	,,	,,	,,	,,	259·9	267·4	,,	,,	11·1	4·1	4·5	0·4	3·38	246	8·0	17·8	4·0	9·0	·500	11·2		
	5	66	18·8	57	13·8	950	,,	,,	,,	,,	,,	260·0	267·4	,,	,,	14·1	5·2	5·7	0·6	3·56	259	8·0	14·2	3·5	6·2	·602	11·8		
	6	63	20·0	56	13·3	961	,,	,,	,,	,,	,,	260·5	267·5	,,	,,	11·0	4·0	4·0	0·0	3·66	267	8·0	18·3	4·0	9·2	·600	11·4		

* Well under water. † Pumped over at intervals during the daytime, but not during the night.

‡ Nos. 5a and 5b invariably under water, and 5c under water at least twice per day; and in the interim perhaps 1 foot or 1·14 exposed to the air.

FORM D.—S. S. "QUEEN VICTORIA," FOREHOLD BOTTOM CABLE.

Form D.

Total Length, 189·59 Knots. Copper Resistance at 20° Cel. 692 Units.

Date. Month.	Day.	Hour.	Resistance of Gutta Percha in Millions of Units: taken after 1 Minute. Total.	1 Minute. Per Knot.	3 min.	8 min.	13 min.	18 min.	23 min.	Resistance of Copper Wire— with Earth connexion.	Resistance of Copper Wire— without Earth connexion.	Temperature according to Resistance of Cable. Fahrenheit.	Resistance Thermometers. No. 5. 45 Knots from Bott. end, 100·9 Units at 20° Cels. Resistance.	No. 5. Temp. Fahr.	No. 6. 113 Knots from Bott. end, 102·1 Units at 20° Cels. Resistance.	No. 6. Temp. Fahr.	No. 7. 163 Knots from Bott. end, 98·81 Units at 20° Cels. Resistance.	No. 7. Temp. Fahr.	REMARKS.
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
		p.m.										°							
December	1	3.0	0·237	60·99	0·337	0·337	0·330	—	—	719·4	720·0	84	102·9	77·0	101·4	64·4	102·8	86·0	Cable kept dry.
,,	,,	5.0	0·232	54·33	0·310	0·320	0·320	0·320	0·320	719·4	720·0	84	—	—	—	—	—	—	Water pumped over the cable from 6.30 p.m. till 10.30 p.m. irregularly.
,,	,,	5.30	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	
,,	2	a.m. 11.0	0·280	53·0	0·300	0·305	0·305	0·302	0·301	721·5	721·8	86	102·5	75·2	103·3	73·4	102·4	84·4	
,,	,,	p.m. 1.0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Water pumped over continually from 10 p.m.
,,	,,	10.0	0·345	64·4	0·337	0·446	0·460	0·465	0·465	715·8	716·0	83·4	104·2	82·8	100·1	59·0	98·0	64·4	Ditto ditto.
,,	3	a.m. 3.0	0·583	109·35	—	—	—	—	—	696·0	697·0	70·2	100·8	67·7	99·5	58·4	97·7	62·1	
,,	,,	p.m. 2.0	0·598	113·38	—	—	—	—	—	689·0	690·0	68·8	100·0	64·0	100·0	58·8	98·3	65·8	
,,	,,	2.0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Ice put on till 9 p.m., and water pumped over cable from 10.90 p.m. till 11.30 p.m. (1.0)
,,	,,	10.30	0·730	138·0	0·750	0·770	0·800	0·800	0·800	—	692·0	68·0	101·5	70·8	99·6	54·2	97·8	62·6	
,,	,,	11.30	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Pumping stopped without orders from our staff.
,,	4	a.m. 5.0	0·749	142·0	0·750	0·770	0·768	0·770	0·770	—	—	67·4	101·4	70·3	98·7	52·7	97·7	63·6	Queen Victoria leaves for Plymouth. No water pumped over the cable.
,,	,,	11.0	0·621	117·6	0·670	0·720	0·730	0·730	0·730	—	691·0	73·4	100·7	67·2	98·9	53·6	98·4	65·8	
,,	,,	11.0	—	—	—	—	—	—	—	—	700·0	—	—	—	—	—	—	—	Water pumped over the cable for half an hour.
,,	,,	p.m. 6.0	0·692	112·2	—	—	—	—	—	719·0	720·0	84·0	101·5	70·8	99·0	54·1	96·2	56·3	Pumping stopped by Capt. Lambert so as not to endanger the ship.
,,	,,	8.0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Water pumped over from 8.0 p.m. with several interruptions till 10.0 p.m., when it was stopped by Captain Lambert so as not to endanger the ship.

APP. No. 11.

Rangoon and Singapore telegraph cable.

APPENDIX No. 11—*continued.*

FORM D.—S. S. "QUEEN VICTORIA," FOREHOLD BOTTOM CABLE—*continued.*

Date.			Resistance of Gutta Percha in Millions of Units: taken after						Resistance of Copper Wire—		Temperature according to Resistance of Cable, Fahrenheit.	Resistance Thermometers.						Remarks.
			1 Minute.		3 min.	8 min.	13 min.	18 min.	23 min.	with Earth connexion.	without.		No. 5. 45 Knots from Bott. end, 100°9 Units at 20° Cels.		No. 6. 113 Knots from Bott. end, 102°1 Units at 20° Cels.		No. 7. 163 Knots from Bott. end, 98°81 Units at 20° Cels.	
Month.	Day.	Hour.	Total.	Per Knot.									Resistance.	Temp. Fahr.	Resistance.	Temp. Fahr.	Resistance.	Temp. Fahr.	
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
December	4	p.m. 11.0	0·610	115·7	0·675	0·700	—	—	—	—	692·0	68·0	101·0	68·5	98·5	51·8	97·0	66·0	
"	5	a.m. 1.0	—	—	—	—	—	—	—	—	700·0	73·4	—	—	—	—	—	—	No water pumped over the cable.
"	"	3.0	0·508	113·4	—	—	—	—	—	—	710·0	70·8	101·5	70·8	98·8	53·2	97·5	62·1	
"	"	10.45	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Water pumped over the cable from 10.45 a.m. till 11.30 a.m., when it was stopped by Captain Lambert so as not to endanger the ship.
"	"	p.m. 12.30	0·598	113·4	0·637	0·700	0·711	0·745	0·775	700·0	700·0	73·4	101·0	73·8	99·3	58·4	98·2	65·4	No water pumped over the cable.
"	"	11.0	0·598	113·4	0·605	0·648	0·669	0·680	0·680	701·0	701·0	73·9	101·0	72·6	99·6	56·7	98·0	64·1	
"	6	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	No tests applied on the 6th, the testing room being filled with water in consequence of which the instruments were deranged.
"	7	a.m. 11.0	0·599	113·6	0·610	0·649	0·678	0·680	0·680	702·0	703·0	75·2	101·0	72·6	98·7	52·8	98·5	66·7	No water pumped over the cable; chief officer declined doing so, as there was 3 feet 1 in. water in the hold.

Mr. Reid's patent pressure tank. *See Plate No. 2.*

MR. REID'S DESCRIPTION of his PATENT PRESSURE TANK, used in testing Cables during Manufacture.

HAVING some years ago employed, at different times, several modes of testing telegraph wire for submarine purposes, such as immersing it in the sea at different depths, &c. this plan I found a great improvement over the common method that was practised at the Gutta-percha works, where it was tested in the canal with seldom more than 12 inches of water over the coil of wire.

The plan of testing in the sea, however good, has some disadvantages, such as stormy weather, the danger of having the wire injured on board ship, or in the transit to and fro, &c. I have therefore adopted the following plan, which for simplicity and giving a fair impartial test of wire to any extent required, seems to provide for all contingencies that may arise in making telegraph cables effective.

First, I take a strong iron vessel capable of containing five miles of core; it is made air and water tight, and is fitted with a number of air-tight stuffing boxes, for the purpose of passing the ends of the wire out from the cylinder and connecting them with a galvanometer. The vessel is also fitted with gun-metal valves at top and bottom, for the purpose of exhaustion and for filling and discharging the water from the cylinder. This vessel weighs in round numbers seven tons.

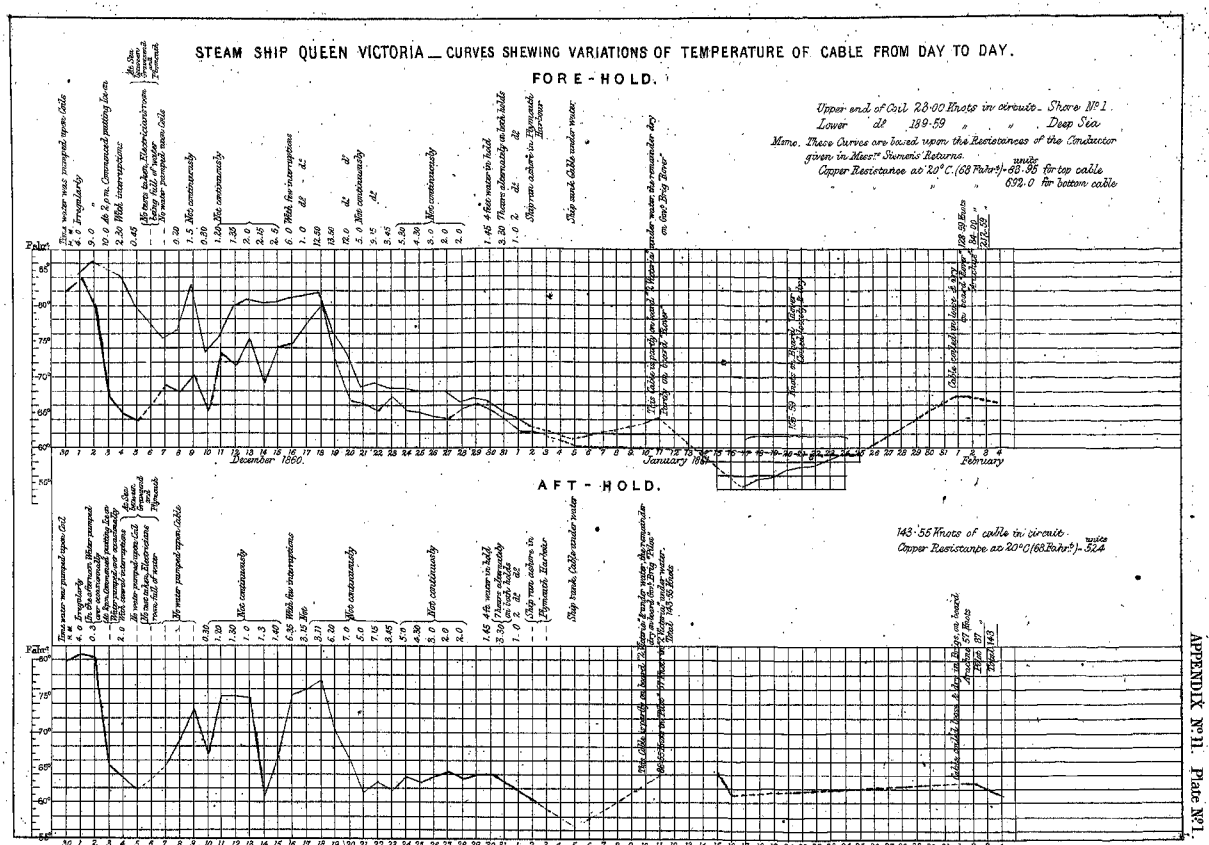
To explain the working fully, I fill the cylinder from the top with as much wire as it will contain, bringing the ends of the several coils or drums through the stuffing boxes; I then put on the cover of the cylinder and bolt it down, having a ring of Indian rubber for a joint. The air-pump, which is worked by steam power, is then applied, and a vacuum formed by exhausting all the air contained in the cylinder and also that of the gutta-percha or other core. Hitherto gutta-percha has only been employed, and it is found that in the process of manufacture a considerable quantity of air has been mixed up with it, forming what is called air bubbles, and sometimes to be in places porous and spongy; in all such places the cavities are filled with air which at times have nearly reached the conductor, and in many cases must have proved fatal to the telegraph. To remedy this defect the object of the air-pump is to remove all the minute particles of air. When the vacuum is considered perfect, as shown on the vacuum gauge, the exhaustion pipe is closed and the valve opened for the admission of water, which at once fills the cylinder and occupies the space in the gutta-percha formerly filled with air. At this stage of the proceedings the galvanometer is carefully noted and the deflection recorded. I now bring the hydraulic pump into action and exert a pressure equal to 600 pounds on the square inch, and after this pressure has remained for 10 or 15 minutes the galvanometer is again recorded, and if it remains quiescent you may consider the cable safe for a depth of 250 fathoms; the pressure is still continued till it equals a column of water whose height is equal to the depth of the sea where the cable is to be laid, adding a sufficient margin to the pressure to make it perfectly safe, on the same principle as testing a beam or girder that is to carry a given weight, the pressure must exceed this weight, or its safety becomes endangered. If we suppose the cable destined for the Atlantic, and the depth to be 2,000 fathoms, equal to 12,000 feet, the pressure applied in this case would be 5,295 pounds on the square inch, equal to 2 tons 7 cwt. 1 qr. 3 lbs. When this pressure has remained on for 15 minutes, again note the galvanometer, and if still quiescent your cable is sound, and may safely be immersed to the required depth. When the wire is removed from the cylinder it must be carefully wound off one drum on to another passing through the hands of a careful operator, who will find a slight indentation on the surface where the pressure has acted on a crevice containing air. He must pass a thin sharp knife under this indentation and reach the bottom; if only slight it may be tooled up and restored; if it is a serious flaw it must be cut out, and the cable proceeded with. The further advantage of this process of testing is, that the flaws and defects are all discovered at an early stage of the cable's progress, and before the expensive process of covering the cable with iron or steel wire has commenced, and the detection becomes more difficult. The engineer has now the satisfaction of knowing that he has a perfect core, and that when laid in the ocean's deep it will not be subjected to any more pressure than has already been upon it, and that if carefully laid and free from mechanical injury, the cable will work satisfactorily and fulfil all the conditions that are anticipated from it.

The great and crying evil with all deep-sea telegraphs hitherto made was the careless and hurried manner they were executed, without a proper amount of inspection and supervision.

I may observe, that some people are of opinion that a cable to be perfect ought to be tested by the above-named process after it has received its iron or hemp covering. This can be done while passing it on board of ship; it only involves a great amount of time and additional expense.

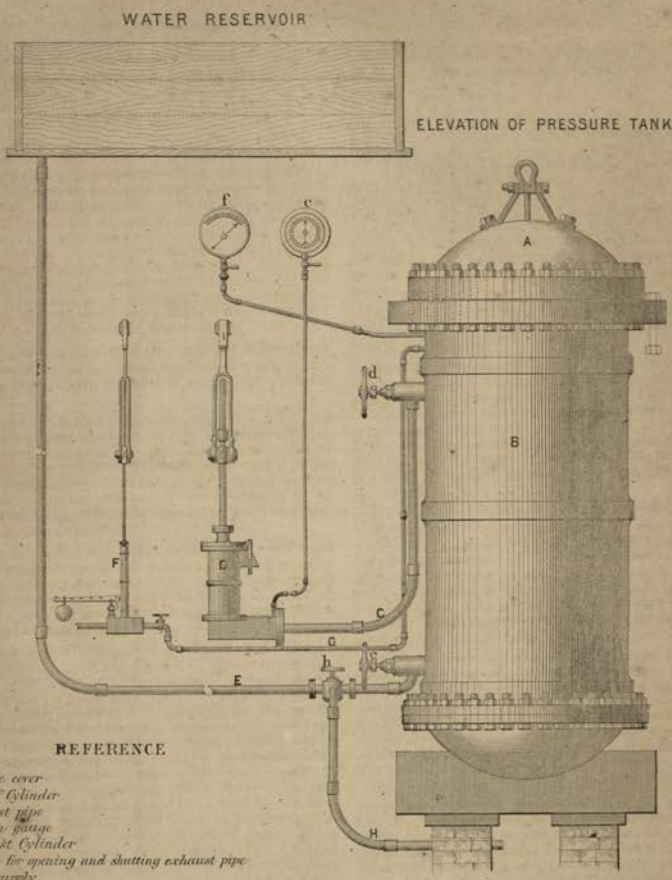
The cable constructed for the Government and about to be laid from Singapore, and the one for the French Government from Toulon to Algiers, I consider the best cables that have yet been made. No pains or expense have been spared to render them effective. They have been very carefully tested, and every coil on which rested the least suspicion has been rejected; and if carefully laid will redeem the credit of this country from the many failures that are recorded against submarine telegraphy.

January 1861.


STEAM SHIP QUEEN VICTORIA — CURVES SHEWING VARIATIONS OF TEMPERATURE OF CABLE FROM DAY TO DAY.
FORE-HOLD.
Upper end of Coil. 28·00 Knots in circuit. Shore № 1.
Lower do 189·59 " " Deep Sea.
Mem. These Curves are based upon the Resistances of the Conductor given in Messrs Siemens's Returns.
Copper Resistance at 20° C. (68 Fahr°) = 63.95 for top cable
692.0 for bottom cable
Fahr°
December 1860.
January 1861
February
AFT-HOLD.
143·55 Knots of cable in circuit.
Copper Resistance at 20°C (68 Fahr°) = 524

Day & Son, Lith. to the Queen.

REID'S APPARATUS FOR TESTING TELEGRAPH WIRES.

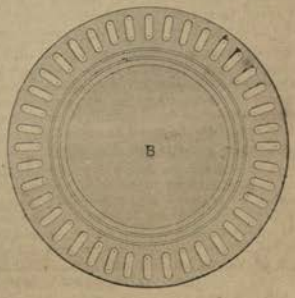


REFERENCE

- A *Moveable cover*
- B *Body of Cylinder*
- C *Exhaust pipe*
- c *Vacuum gauge*
- D *Exhaust Cylinder*
- d *Handle for opening and shutting exhaust pipe*
- E *Water supply*
- e *Handle for opening and shutting water supply pipe*
- F *Hydraulic pump for pressure*
- f *Pressure gauge*
- G *Pressure pipe*
- H *Waste pipe*
- h *Handle for opening and shutting waste pipe*

PLAN OF CYLINDER COVER

PLAN OF CYLINDER



APPENDIX No. 11—*continued.*

App. No. 11.
Rangoon and Singapore telegraph cable.

REPORTS on the SPONTANEOUS GENERATION of HEAT in the RANGOON and SINGAPORE (late FALMOUTH and GIBRALTAR) CABLE, by WM. ALLEN MILLER, M.D., and Messrs. SIEMENS, HALSKE, and Co.

1.

King's College, London,
December 3, 1860.

MY LORDS,

REFERRING to your letter bearing date October 1, 1860, in which you requested me to examine into the cause of the heating of the electric cable (proposed to be laid down between Rangoon and Singapore) during the process of manufacture, I, in accordance with your instructions, placed myself in communication with Mr. Gisborne, who has given me every facility for inspecting the cable during the process of its manufacture, and for conducting the necessary experiments. I have also received full particulars respecting the tests of the electric conductivity of the cable, and of the conditions in which the heating was observed.

From the information thus obtained, and from various experiments of my own, I have been enabled to arrive at a definite conclusion as to the cause of the heating which was observed.

These heating effects appear to have been due not to any permanent chemical change, either in the composition of the insulating coating of the gutta-percha, nor in that of the serving of hemp and tar, but were owing simply to the effects of oxydation upon the iron, at the ordinary summer temperature of the air, produced by the moistening of the cable with the water of the river, the slightly brackish nature of which increased the effect.

It appears that the cable, when manufactured, was coiled in tanks, where it was at first wholly submerged in water. Gradually, however, the tanks began to leak as the weight of water increased, and eventually the water was drawn off, leaving the mass of the cable thoroughly soaked in water, but nowhere submerged.

From the mode of stowage necessarily adopted, an enormous surface of metal was exposed to the air in a comparatively small compass. The moisture necessary to the oxydation was gradually supplied by the capillary action of the hemp sewing as fast as it evaporated from the metallic surface, under conditions most favourable to the production of oxydation. This oxydation is always attended with a rise of temperature, but under ordinary circumstances the heating is overlooked, as the mass of air around carries off the heat as fast as it is produced. Here, however, the conditions were exactly such as would be required for its accumulation, viz., a sufficient access of air to promote oxydation, with an effectual prevention of a rapid change of the mass of air; and a speedy rusting of the metal, attended with a considerable rise of temperature throughout the mass, was the unavoidable result. The process of rusting is very slow at temperatures not exceeding 50°, but it increases rapidly as the external temperature is augmented beyond this point.

In order for the future to avoid any similar heating of the electric cables during their manufacture, it will be sufficient either to avoid wetting the metallic coating at all during the process of making, or if it be found needful to immerse the cable in water, to take care that it be always afterwards kept entirely submerged.

The oxydation of the iron wire coating takes place at temperatures above 50°, but only when both air and moisture have free access to the surface of the metal.

The method of cooling the cable which was resorted to in this instance, by frequently sprinkling it with cold water, although effectual as a temporary expedient, is attended with an increased rusting of the metal; and if it were discontinued during the warm weather, it would inevitably lead to a renewal of the heating in a still more rapid degree. I return Mr. Gisborne's reports.

I am, &c.

(Signed) WM. ALLEN MILLER, M.D.

To the Right Honourable
the Lords of the Committee
of Privy Council for Trade.

2.

King's College, London,
December 12, 1860.

Dr. Miller's reports on spontaneous generation of heat in cable.

SIR,

I HAVE the honour to acknowledge the receipt of a copy of a letter, dated December 11, 1860, addressed by Sir W. B. O'Shaughnessy to J. Danvers, Esq.

In reply to your request that I would express my opinion upon the suggestion of Sir William as to the cause of the heating of the electric cable on board the "Victoria" steamer, I would remark that that gentleman states that he has not had an opportunity of seeing the cable in question.

Had he done so, I think it not improbable that he would have come to the same conclusion upon the subject as myself.

From the experiments which I have made, and the results of which I reported to you in August last, I am able to confirm the statement of Sir W. O'Shaughnessy, that gutta-percha under certain circumstances slowly absorbs oxygen, and might even become heated in the process. This oxidation, however, takes place under free contact with the air, is favoured by an elevated temperature, and, as I have particularly pointed out, is especially influenced by exposure to light.

If I understand rightly, in the case alluded to by Sir W. O'Shaughnessy, the gutta-percha cable was entirely unprotected by any outer covering. If so, there is no analogy between that case and the present one; for in the cable at present on board the "Victoria" the gutta-percha is covered with hemp soaked in tar, and this coating is again covered with iron wire. The gutta-percha is therefore in the dark, and in a great measure excluded from the action of the air.

If Sir W. O'Shaughnessy had examined the cable he would have observed,—1st, that there was a considerable oxidation of the iron covering; 2nd, that this oxidation was in great measure confined to the *exposed* surface of the iron wire, and that where the surface of the wire was in contact with the hemp serving it was scarcely rusted. This oxidation of the wire is abundantly sufficient to produce the heating in question.

Remembering also that the oxidability of iron in a moist condition far surpasses that of gutta-percha, Sir W. O'Shaughnessy would probably agree with me in thinking that the oxygen of the air would expend itself upon this freely exposed iron, and could not therefore reach the deeper and well-protected gutta-percha. Still more, he would also have seen, on examining the gutta-percha that in reality *no such change from oxidation had occurred.*

I satisfied myself upon this point, as of course the oxidability of gutta-percha itself was, from my own recent experiments, present to my mind.

The remedy suggested by Sir W. O'Shaughnessy, viz.—the immediate coiling of the cable into water with the view of recoiling it on shipboard into water-tight tanks for the voyage, is exactly the same as that which I proposed to yourself as soon as I was informed that the heating on shipboard had occurred. There is no doubt that it is the true remedy, and that it would be perfectly effectual.

I understand, however, owing to the delay which would be needful in order to enable suitable tanks to be constructed, that such a course would entail the loss of the present season for laying the cable, and that with a view to attempt to obviate this great inconvenience and expense, it is proposed to uncoil the cable, tar it, and recoil it.

I have considerable doubt as to the ultimate success of this expedient; but I see no objection to the trial of this *as an experiment*, provided that the freshly coiled cable be exposed for ten days or a fortnight to a careful series of tests, with the view of detecting immediately any rise of temperature should it occur when the cable is duly recoiled into the hold. Even should the process fail in preventing the heating, the loss incurred would be limited to the expense of tarring and recoiling.

App. No. 11. Rangoon and Singapore telegraph cable.

If, however, the temperature be still found to rise after the tar has been applied, the sailing of the ship should be stopped at all hazards. The cable, in such a case, must be recoiled into water, and kept there until vessels with suitable water-tight tanks for its conveyance have been prepared.

I have, &c.
(Signed) WM. ALLEN MILLER.

To Captain Galton, F.R.S.
&c. &c. &c.

Dr. Miller's report on spontaneous generation of heat in cable.

NOTE OF APPENDIX to DR. MILLER'S REPORT on the HEATING of the RANGOON ELECTRIC CABLE.

I have been informed that a knot of this cable weighs 2½ tons, or about 2 tons per mile. Experiments upon the heat given out during the oxidation of iron show that for every pound of iron oxidized, it would raise two tons of the metal 15° Fahrenheit.

Each mile of wire during the oxidation of 1 lb. of the wire covering its surface would therefore be raised about 15°.

In order to form an approximate estimate of the quantity of iron that had undergone oxidation during the heating observed in the course of its manufacture whilst on the premises of Messrs. Glass and Elliot, I detached as much as possible of the oxide of iron from the surface of a piece of the cable. I found that a sample of this cable, cut at my request at the beginning of October 1860 from the centre of the heated coil, and representing a fair average sample of its condition, furnished about 66 grains of oxide of iron, the length of this portion being two feet.

This oxide would amount to 24·9 lbs. per mile, which would be equivalent to nearly 17½ lbs. per mile of metallic iron which had undergone oxidation, a quantity amply sufficient to account for the heating of the cable observed.

The rusting of the iron, it may be stated, was entirely confined to the external surface of the iron wire the parts of the wire in contact with the tarred hemp having perfectly preserved their brilliant metallic surface and aspect.

(Signed) WM. ALLEN MILLER.

Messrs. Siemens and Halske's report on the spontaneous generation of heat in the cable.

3.—REPORT on the SPONTANEOUS GENERATION of HEAT in the RANGOON and SINGAPORE CABLE at GREENWICH.

It was remarked, towards the beginning of June 1860, that some of the tanks at the sheathing works of Messrs. Glass, Elliott, and Co., at Greenwich, began to leak. The cables deposited in these tanks could not, therefore, be kept entirely under water, and some irregularities in the indications in our electrical observations became manifest. This was communicated, by letter dated 22d June 1860, to Messrs. Gisborne and Forde, who, guided chiefly by the advice of the Gutta Percha Company, ordered these cables to be kept entirely dry, and since nothing more was to be added to their length, our tests were reduced to a mere control test once or twice per week. On the 17th August, when the usual weekly test was applied, it was found that the resistance of the gutta-percha of cable No. 5 had fallen to 85 millions of units. By the copper resistance it was also found that the table had attained a mean temperature of from 79–80° Fahrenheit, corroborating the reduced resistance of the gutta-percha. On 20th August, cable No. 5 was cut into three different pieces, each of which was tested separately. The length of each piece not being then exactly known, no very definite results could be obtained. In order to ascertain these lengths, sections A and B, cable 5, were coiled over into dry tanks, where they remained from the 4th to 12th September. Section C, cable 5, was paid over to the same dry tank gradually during this period.

The annexed three sheets contain the tabulated summary of our tests. The observed copper resistances in column 13 gave a clue to the real average temperatures of the cables; their resistances at the temperature of 20° C. being known. The temperatures calculated from these resistances will be found in column 16.

Some of the indications of the resistance of the copper wire are too high and not to be relied upon, because discharge currents of the cable, caused by the previous tests of Messrs. Glass and Elliott, influenced our tests. The differences between the positive and the negative current given in column 20 prove the existence of these discharge currents.

Section A, lying at the bottom of the tanks, has been affected during the time it has been kept dry to the greatest extent, the temperature of this cable reaching to 23·1° C., and the resistance of the gutta-percha decreasing to 57 millions of units.

Section B being kept dry during the same period, and placed between the other two, attained a temperature of 23° C., and its gutta-percha resistance was found to be 91 millions of units.

Section C being irregularly wet and dry during the time it was coiled over, reached a temperature of between 16·8° and 19° C., the resistance of the gutta-percha varying between 115 and 301 millions of units.

Section A, at the bottom, had therefore changed most.
Section B, the middle section, less.
Section C, on the top, least.

On the afternoon of the 11th, water was pumped over all the cables deposited in the same tanks in the above order.

The water, previous to being pumped over the cable, had a temperature of 14·4° C., which however rose to 17·8° C. when the tank was full, as measured by a mercury thermometer.

The calculated temperatures of the three cables were, on the 12th, as follows:—

Section a. = 19·5° Centigrade.
„ b. = 18·0° „
„ c. = 17·9° „

During the time from 12th September to 13th October, the cables were continually kept under water, except on the 9th October.

The temperature of the cables during this period as ascertained by electrical tests, which are given in column 16, will be seen to fall gradually, exceeding nevertheless, always the measured temperature of the water, which in its turn exceeds its temperature when entering the tank.

The first being = 14·1°, the other 13·7°.

The average temperatures of the three sections during this period are as follows:—

Section.	Calculated Temperature.	Measured Temperature of Water in the Tank.	Measured Temperature of Water in the other Tank.
a.	17·1	14·1	13·7
5 b.	15·9	14·1	13·7
5 c.	15·38	14·1	13·7

Since these cables, 5 a., 5 b., 5 c., have been kept continually under water, cable 5 a. has undergone some changes not attributable to the variations of the temperature of the water alone.

It is, therefore, still under the strictest observations in order to come to a decision whether this cable has suffered permanently by the generation of heat that took place.

Cables 5 b. and 5 c. however do not give rise to such a doubt, as their regular tests show no other differences than such as are accounted for by variations in the temperature of the water.

We do not pretend to decide what the chemical causes may be that give rise to a generation of heat when such cables after having been wet, become gradually dry, but we are convinced by our tests that such changes of temperature do take place. These should be carefully guarded against, lest by an accumulation of heat the gutta-percha should be softened and the metallic core move from its central position. Unless the temperature should at any time have exceeded 90° we do not apprehend that any permanent injury can have been done to the cable. The maximum average temperature that has been observed in the present instance does not exceed 80° Fahrenheit, and the cables may be pronounced uninjured unless the heating action should have been more energetic, locally, of which however there is no proof at present.

(Signed) SIEMENS, HALSKE, & Co.

London, 31st October 1860.

APPENDIX No. 11—*continued.*

App. No. 11.
Rangoon and Singapore telegraph cable.
Registry of tests.

RANGOON AND SINGAPORE TELEGRAPH.—ELECTRICIANS' DEPARTMENT.—REGISTRY OF TESTS DURING MANUFACTURE OF CABLES.

Date.	Temperature. Room.	Temperature. Water. F.	Water. C.	Temperature of Water in the Tanks.	Number of Cable.	Total Length.	Resistance of Wire. Obs.	Resistance of Wire. Sum C at 20°.	Calculated Temperature.	Resistance of Insulation. g. +	g. −	g. Diff.	Per Knot. $Ir=IR.l$.	Loss. Disch.	Remarks.
	F.	° F.	° C.			Knots.	Units.	Units.	°	°	°	°	Millions.	ch.	
Aug. 20	65	61	16·	..	5a. lying at bottom of sand.	39·12	142·5	142·8	19·5	2·8	3·0	0·2	91	·600	Water pumped over from the 19th August to 4th September.
21	..	..	..	..		..	142·6	..	19·55	2·3	2·3	0·0	159	·750	
										22·0	22·0	0·0			
Sept. 4	62	..	..	..		..	142·5	..	19·5	22·0	22·0	0·0	123	·750	
5	61	..	..	..		..	142·8	..	20·0	27·7	27·7	0·0	101	·500	
6	60	..	..	..		..	143·1	..	20·5	2·5	2·6	0.1	107	·501	
7	62	..	..	..		..	143·1	..	20·5	3·4	3·6	0·2	75	·625	Cable kept dry till the afternoon of the 11th September.
8	63	..	..	..		..	144·1	..	22·3	4·0	4·2	0·2	66	·750	
10	64	..	..	..		..	143·9	..	22·0	4·2	5·1	0·9	57	·750	
11	66	..	..	..		..	144·6	..	23·1	3·4	3·8	0·4	72	·625	
12	66	64	17·8	14·4		..	142·5	..	19·5	2·3	2·5	0·2	117	·500	From the 12th September the cable was completely under water, except on the 9th October.
13	64	63	17·2	14·4		..	142·3	..	19·1	2·4	2·5	0·1	106	·500	
14	65	62	16·5	14·4		..	141·9	..	18·4	1·5	1·9	0·4	153	·500	
15	64	60	15·5	14·4		..	141·9	..	18·4	1·6	2·2	0·6	136	·500	
17	64	57	14·0	13·8		..	141·4	..	17·6	..	..	..	..	..	
18	68	58	14·4	13·8		..	141·3	..	17·4	4·8	5·2	0·4	104	·800	
19	70	57	14·0	14·4		..	141·9	..	18·4	3·8	4·0	0·2	123	·758	
20	71	57	14·0	13·8		..	141·3	..	17·4	5·5	4·0	1·5	105	·778	
21	70	57	14·0	13·8		..	141·2	..	17·2	5·5	3·8	1·7	108	·778	
22	69	57	14·0	13·8		..	141·2	..	17·2	2·7	3·0	0·3	181	·555	
24	63	58	14·4	13·3		..	141·0	..	17·0	2·6	2·8	0·2	184	·555	
25	64	58	14·4	13·3		..	140·8	..	16·5	2·8	3·0	0·2	171	·555	
26	64	58	14·4	13·3		..	141·0	..	19·9	4·0	4·1	0·1	143	·555	
27	62	58	14·4	13·3		..	144·3	..	See co-	5·2	4·5	0·7	119	·555	The copper resistances taken on the 27th, 28th, and 29th are not to be depended upon, as the cable was insufficiently discharged at the time.
28	60	57	14·0	13·3		..	143·7	..	lumn of	4·4	4·3	0·1	122	·555	
29	59	56·5	13·5	13·3		..	142·0	..	remarks.	3·5	3·7	0·2	161	·555	
											°				
Oct. 1	..	56	13·2	13·3		..	140·5	..	16·0	..	..	..	..	..	
2	64	56	13·2	13·3		..	141·2	..	17·2	3·4	3·5	0·1	175	·333	
3	65	56	13·2	13·8		..	141·1	..	17·0	3·4	3·5	0·1	179	·333	
4	66	56	13·2	13·3		..	140·3	..	15·6	3·0	3·1	0·1	187	·333	
5	66	56	14·3	13·8		..	140·3	..	15·6	2·7	3·0	0·3	264	·336	
6	68	56	13·3	13·3		..	140·1	..	15·3	3·6	2·5	1·1	182	·336	
8	66	56	13·3	12·8		..	140·1	..	15·3	2·1	2·3	0·2	297	·336	
9	65	..	..	..		..	141·0	..	16·9	3·0	3·2	0·2	212	..	No water in the tank.
10	64	58	14·5	12·8		..	140·4	..	15·4	2·0	2·2	0·2	289	..	
11	63	56	13·2	12·8		..	140·6	..	15·8	2·0	2·2	0·2	296	..	
12	66	54	12·0	12·2		..	140·1	..	15·3	2·0	2·2	0·2	318	..	
13	60	53	11·7	12·2		..	139·6	..	15·0	1·9	2·0	0·1	309	..	
Average temperatures			14·1	13·7					17·1						

London, 31st October 1860. (Signed) SIEMENS, HALSKE, and Co.

APP. No. 11.

APPENDIX No. 11.—*continued.*

RANGOON AND SINGAPORE TELEGRAPH.—ELECTRICIANS' DEPARTMENT.—REGISTER OF TESTS DURING MANUFACTURE OF CABLES.

Date.	Temperature. Room.	Temperature. Water.		Temperature of Water in the Tanks.	Number of Cable.	Total Length.	Resistance of Wire. Obs.	Resistance of Wire. Sum C at 20°.	Calculated Temperature.	Resistance of Insulation. γ_t. +	γ_t. ±	γ_t. Diff.	Per Knot. $Ir = IR.l$	Loss. Discharge.	Remarks.
	F.	F.	C.			Knots.	Units.	Units.		°	°	°	Millions.		
Aug. 20	65	64	17·8	..	5 b.	51·15	185·3	186·6	18·4	3·0	3·5	0·5	250	..	Water pumped over the cable from 19th August till the 4th September.
21	65	64	..	..	placed between 5 a. and 5 c.	..	184·5	..	17·3	2·8	3·3	0·5	157	..	
Sept. 4	62	..	..	..		..	184·7	..	17·5	17·0	17·5	0·5	237	·555	Kept dry from the 4th till the afternoon of the 11th September.
5	61	..	..	..	..	..	184·6	..	17·4	17·8	18·0	0·2	206	·446	
6	60	..	..	..	..	..	185·8	..	19·0	2·0	2·3	0·3	182	·556	
7	62	..	..	..	..	..	186·8	..	20·0	2·4	2·7	0·3	135	·555	
8	65	..	..	..	..	..	187·5	..	21·1	2·5	2·7	0·2	135	·667	
10	64	..	..	..	..	..	189·2	..	23·0	3·8	3·6	0·2	106	·778	
11	66	..	..	..	..	..	189·9	..	..	3·0	3·6	0·6	91	·877	The cable completely under water from the 12th September except on the 9th October.
12	66	64	17·8	14·4	..	..	186·6	..	20·0	2·0	2·1	0·1	180	·555	
13	64	63	17·2	14·4	..	..	185·0	..	18·0	1·7	2·0	0·3	183	·555	
14	65	62	16·5	14·4	..	..	184·5	..	17·4	1·5	1·8	0·3	200	·434	
15	64	60	15·5	14·4	..	..	184·4	..	17·3	1·8	2·0	0·2	183	·533	
17	64	57	14·0	13·8	..	..	184·0	..	17·0	..	..	..	..	..	
18	68	57	14·0	13·8	..	..	184·0	..	17·0	2·8	2·8	0·0	243	·500	
19	70	58	14·5	14·4	..	..	184·6	..	17·5	2·7	2·7	0·0	235	·600	
20	71	57	14·0	13·8	..	..	183·5	..	16·1	2·6	2·6	0·0	251	·400	
21	70	57	14·0	13·8	..	..	183·4	..	16·1	2·5	2·7	0·2	253	·600	
22	69	57	14·0	13·8	..	..	184·4	..	17·3	2·6	2·7	0·1	255	·600	
24	63	58	14·4	13·3	..	..	183·3	..	15·9	2·4	2·5	0·1	265	·452	
25	64	58	14·4	13·3	..	..	183·2	..	15·8	2·2	2·4	0·2	281	·454	
26	64	58	14·4	13·3	..	..	183·1	..	15·7	2·5	2·7	0·2	291	·454	
27	62	58	14·4	13·3	..	..	183·3	..	15·9	2·7	3·0	0·3	266	·454	
28	60	57	14·0	13·3	..	..	184·0	..	17·0	2·5	2·7	0·2	302	·500	
29	59	56	13·3	13·3	..	..	183·1	..	15·7	2·5	2·6	0·1	297	·500	
Oct. 1	..	56	13·3	13·3	..	..	182·5	..	15·1	..	..	..	293	·500	
2	64	56	13·3	13·3	..	..	182·9	..	15·5	2·6	2·8	0·2	293	·500	
3	65	56	13·3	13·8	..	..	182·6	..	15·2	2·6	2·6	0·0	296	·454	
4	66	56	13·3	13·3	..	..	182·1	..	14·3	2·4	2·7	0·3	292	·473	
5	66	57	13·8	13·8	..	..	182·2	..	14·2	3·1	3·0	0·1	325	·517	
6	68	56	13·3	13·3	..	..	181·9	..	14·0	2·4	2·5	0·1	306	·333	
8	66	56	13·3	12·8	..	..	181·9	..	14·0	2·3	2·5	0·2	356	·356	
9	65	..	..	12·8	..	..	183·1	..	15·7	3·1	3·2	0·1	264	·416	No water in the tank.
10	67	58	14·4	12·8	..	..	182·6	..	15·2	2·4	2·5	0·1	317	·500	
11	63	56	13·3	12·8	..	..	182·2	..	14·2	2·6	2·6	0·0	312	·500	
12	60	54	12·2	12·2	..	..	182·2	..	14·2	2·4	2·4	0·0	347	·500	
13	60	53	11·7	12·2	..	..	181·5	..	13·8	2·2	2·3	0·1	350	·500	
Average temperatures		14·1		13·71					15·9						

London, 31st October 1860. (Signed) SIEMENS, HALSKE, and Co.

Appendix No. 11—*continued.*

Rangoon and Singapore Telegraph.—Electricians' Department.—Registry of Tests during Manufacture of Cables.

Date.	Temperature. Room.	Temperature. Water.		Temperature of Water in the Tanks.	Number of Cable.	Total Length.	Resistance of Wire. Obs.	Resistance of Wire. Sum C at 20°.	Calculated Temperature.	Resistance of Insulation. g. +	g. −	g. Diff.	Per Knot. Ir = IR.l.	Loss. 1 — Discharge.	Remarks.
	F.	F.	C.												
	°	°	°			Knots.	Units.	Units.		°	°	°	Millions.		
Aug. 20	63	61	16·	..	5 c. laying on the top of the other cables.	72·69	263·1	266·3	17·	4·0	4·6	·6	115	·79	Water pumped over from 19th August to 4th September.
21	..	..	..	..		..	265·2	..	19·	4·0	4·5	·5	118	..	
Sept. 4	62	..	..	..		..	262·7	..	16·8	28·0	30·0	·2	158	·499	From the 4th to the 11th the cable was paid over to the new tank, and till the 17th kept sometimes without water, from which day it was always under water. Except on the 9th Oct.
5	62	..	..	..		..	263·9	..	17·8	29·9	30·0	·1	150	·680	
6	60	..	..	..		..	263·9	..	17·8	2·7	2·9	·2	162	·599	
7	62	..	..	..	..	..	263·9	..	17·8	2·2	2·2	·0	218	·532	
8	62	..	..	..	..	..	263·9	..	17·8	2·4	2·6	·1	301	·599	
10	64	..	..	..	..	..	263·3	..	17·0	2·5	2·6	·1	195	·327	
11	66	..	..	..	..	..	263·5	..	17·4	2·3	2·6	·3	195	·532	
12	66	64	17·8	14·4	..	..	264·1	..	17·9	2·3	2·4	·1	208	·465	
13	64	63	17·2	14·4	..	..	263·9	..	17·8	2·2	2·5	·3	206	·652	
14	65	62	16·5	14·4	..	..	..	..	..	2·7	3·1	·2	200	·465	
15	64	60	15·5	14·4	..	..	..	..	..	..	..	..	167	·399	
17	64	57	14·	13·8	..	..	..	..	..	..	..	..	..	..	
18	68	57	14·8	13·8	..	..	262·4	..	16·3	4·4	4·8	·4	211	·625	
19	70	58	14·5	14·7	..	..	262·5	..	16·3	4·0	4·3	·3	213	·500	
20	70	57	14·	13·8	..	..	263·2	..	17·2	4·7	5 1	·4	190	·625	
21	70	57	14·	13·8	..	..	262·7	..	16·8	4·0	4·5	·5	221	·500	
22	69	57	14·	13·8	..	..	262·9	..	16·5	4·1	4·3	·2	224	·500	
24	63	58	14·4	13·3	..	..	262·5	..	16·7	3·8	4·0	·2	237	..	
25	64	58	14·4	13·8	..	..	262·4	..	16·65	3·9	4·0	·1	234	..	
26	64	58	14·4	13·8	..	..	261·7	..	15·8	4·2	4·5	·3	248	..	
27	62	58	14·4	13·3	..	..	264·7	..	18·5	4·3	4·9	·6	235	..	The copper resistances measured on the 27th and 28th are not to be depended upon, as the cable was insufficiently discharged when taken.
28	60	57	14·	13·3	..	..	262·9	..	16·9	4·1	4·6	·5	257	..	
29	59	56	13·3	13·8	..	..	261·9	..	15·9	4·1	4·4	·3	254	..	
Oct. 1	64	56	13·3	13·3	..	..	261·3	..	15·5	..	..	..	..	..	
2	64	56	13·3	13·8	..	..	261·7	..	15·8	4·1	4·2	·1	271	..	
3	65	56	13·3	13·3	..	..	260·0	..	014·5	4·0	4·1	·1	255	..	
4	66	56	13·3	13·3	..	..	260·9	..	14·9	4·1	4·5	·4	246	..	
5	66	57	14·	13·8	..	..	260·9	..	14·9	5·2	5·7	·5	259	..	
6	68	56	13·2	13·3	..	..	260·5	..	14·7	4·0	4·0	·0	267	..	
8	66	56	13·3	12·8	..	..	260·7	..	14·8	4·0	4·0	·0	292	..	
9	65	56	13·3	12·8	..	..	260·9	..	14·9	4·3	4·6	·3	269	..	No water in the tank.
10	64	58	14·5	12·8	..	..	260·0	..	14·5	4·2	4·6	·4	255	..	
11	63	56	13·2	12·8	..	..	260·2	..	14·6	3·0	3·3	·3	292	..	
12	60	54	12·	12·2	..	..	259·7	..	14·3	3·2	3·7	·5	334	..	
13	60	53	11·7	12·2	..	..	259·3	..	14·2	3·5	3·5	·0	321	..	
Average temperatures			14·1	13·7					15·38						

London, 31st October 1860. (Signed) Siemens, Halske, and Co.

App. No. 11.

Appendix No. 11—*continued.*

4.—Fore-hold Bottom Cable.—Queen Victoria.

Total length, 189·59 Knots . . . . Resistance at 20° Cels. = 692 Units.

Date.	Hour.	Resistance of Gutta Percha per Knot in Millions of Units.	Resistance of Copper of Total Length in Units of Resistance.	Average Temp. of Cable calculated by its measured Copper Resistance.	Resistance Thermometer. No. 5, situated 45 Knots from the bottom end of Cable. Resist. at 20° Cels. = 100·9 Units.		No. 6, situated 113 Knots from the bottom end of Cable. Resist. at 20° Cels. = 102·1 Units.		No. 7, situated 163 Knots from the bottom end of Cable. Resist. at 20° Cels. = 98·81 Units.		Remarks.
					Measured Resist.	Temp. Fahr.	Measured Resist.	Temp. Fahr.	Measured Resist.	Temp. Fahr.	
				°		°		°		°	
Nov. 10	—	553	—	—	—	—	—	—	—	—	Before coiling the cable on board, it was laying in a tank at Greenwich, being pumped over with water occasionally. Temperature of the water in tank = 79° Fahrenheit.
„ 21	—	199	—	—	—	—	—	—	—	—	Cable being completely coiled on board and kept dry.
„ 30	7 p.m.	—	—	—	102·4	75·2	101·0	62·6	102·2	84·2	Cable kept dry.
Dec. 1	2 p.m.	61	720	84	102·9	77	101·4	64·4	102·8	86·0	
„ 1	6 p.m.	54	720	84	—	—	—	—	—	—	
„ 2	11 a.m.	53	721·8	86	102·5	75·2	103·3	73·4	102·4	84·6	Water was pumped over the cable irregularly from 6.30 p.m. to 10.30 p.m. on the previous evening.
„ 2	10 p.m.	64	716	83·4	104·2	82·8	100·1	59	98	64·4	Water was pumped over the cable from 1 p.m. continually.
„ 3	6 a.m.	100	697	70·2	100·8	67·7	99·5	56·7	97·5	62·1	

Description of a Resistance Thermometer.—By Mr. C. W. Siemens.

Description of a resistance thermometer.

In consequence of the observed tendency of the Rangoon and Singapore cable to heat after passing from water into atmospheric influence, I was desirous to ascertain the temperature of the coil at regular intervals throughout the mass after it had been coiled on board, in order that timely remedies might be applied to check the accumulation of heat in any part. Recent events have proved that this precautionary measure had not been adopted in vain, for the temperature in the interior of the fore-hold cable on board the "Queen Victoria" had reached the alarming point of 86° before the temperature at the surface of the mass had quite reached 60°. The instrument I designed for this purpose may be appropriately termed a "resistance thermometer."

This instrument is based upon the principle "that the "specific resistance of copper to the galvanic current "varies in a definite and constant ratio with the tempera-"ture of the conductor;" the rate of variation has been determined by "Arndtsen" to amount to ·21 per cent. of increase of resistance per degree Fahrenheit increase of temperature.

The instrument consists of a rod of iron about 18 inches long, covered with several layers of insulated copper wire, covered externally with sheet india-rubber, and a metallic protecting tube, hermetically sealed at the ends. The two ends of the coil of wire are continued, by insulated conductors, into the observatory, the instruments themselves being deposited at intervals between the layers of cable in the hold of the vessel. The resistance of these spiral conductors, including the larger connections leading to the instrument room, had been accurately ascertained beforehand at the standard temperature, and noted down in units of resistance, the instrument employed for this purpose being our ordinary resistance-coil testing apparatus. In taking the resistance of these coils at any future time, and in comparing the results of these observations with the original measurement at the standard temperature, the increase or decrease of temperature in degrees of Fahrenheit is obtained, by simply multiplying the increase or decrease of resistance in per cents of the total resistance with $\frac{1}{21} = 4{\cdot}8$.

The result of these determinations is more uniform, and may be depended on for correctness, to a tenth part of a degree Fahrenheit.

Thermometers on this principle would, I think, be found extremely useful in meteorological observations, and in experiments to determine the temperature of the ocean at various depths, being the only thermometer hitherto devised which can be read at a point considerably distant from the position of the instrument.

C. W. Siemens.

London, 31st December 1860.

APPENDIX No. 12.

OUTLINE of the PRINCIPLES and PRACTICE involved in dealing with the ELECTRICAL CONDITIONS of SUBMARINE ELECTRIC TELEGRAPHS.—By WERNER and C. W. SIEMENS.

Principles and practice involved in dealing with the electrical conditions of submarine electric telegraphs, by Werner and C.W.Siemens.

THE failures of the more extensive lines of submarine electric telegraphs, which have hitherto been but too frequently experienced, have become manifest almost invariably by a gradual decrease of insulation. In repairing these lines, it has generally been found that the gutta-percha has become disintegrated by the electrolytic action of the currents employed in working the line in places where the thickness of insulating material had been originally considerably below the average, owing to some mechanical injury, or, more frequently, owing to a cavity in the material, forced into by the water, or to an excentric position of the conductor.

In such places where the insulating covering of gutta-percha has been of uniform and sufficient thickness, no disintegration or partial destruction of the material is observable, even after the line has been worked for many years. The rapidity with which the work of destruction in faulty places proceeds depends entirely upon the intensity and duration of currents employed in working the line. Faults are produced proportionately more rapidly in long lines, owing to the greater resistance of the metallic conductor. Their progress can be retarded in working the lines with *feeble* and *alternating currents*, but it cannot be arrested entirely, and it may be laid down as an axiom that "*so "long as any thin places are allowed to remain in the gutta-"percha covering of a submarine conductor, so long will their "insulation fail by slow degrees.*"

It is, therefore, a matter of first importance to prevent, if possible, all irregularity in the insulating covering. The material employed should be perfectly homogeneous; it should be put upon the wire in several coatings, closely adhering to one another; air bubbles should be strictly avoided, and the concentricity of the entire coating be insured by the use of very perfect machinery and strict avoidance of stoppages during the process of covering, to prevent a softening of the several coatings by heat.

Great improvements have of late been effected in the process of covering electric conductors with gutta-percha and intermediate layers of compound called "Chatterton's mixture," which may be estimated by the fact that the covering of the Rangoon and Singapore cable, now in process of manufacture, insulates fully ten times better than the covering of the Red Sea and India cable did before it was laid.

This marked improvement is due to the greater care taken by the Gutta Percha Company in the manufacture, under a system of stringent electrical tests, which we are charged by the British Government to apply. The object of these tests is, in the first place, to ascertain the specific conductivity of each mile of the copper conductor, in order that all below a certain fixed standard may be rejected.

An inquiry into the extraordinary variations in the conductivity of the *copper of commerce* has been made the subject of a careful investigation by Dr. Mathiessen for the British Government, which will probably shortly be published.

In practice we find that the best selected copper employed for telegraphic conductors varies as much as twenty per cent. in its conductivity and that the *purer copper* conducts the best.

The conductivity tests of each mile of an insulated conductor are very necessary, not only to reject the faulty material but also to obtain a complete record of the conductivity of each portion of the cable when completed, without which it is not possible to determine afterwards by galvanic tests and calculations the precise position of a fault.

The more difficult and most important tests are those of the conductivity of the insulating material of *each mile* of insulated conductor, for it is not sufficient to find out any palpable fault or leakage but to appreciate excentricities, cavities, or other minor defects in the coating, and to reject what falls below the standard of conductivity of the insulating material in its most perfect condition.

It was necessary for the purpose to determine in the first place the specific conductivity of the material which experience has proved to be sufficiently uniform at constant temperatures.

The effect of temperature upon the conductivity of gutta-percha and other insulators has lately been fully investigated by the Scientific Telegraph Committee of the British Government, whose report is however not yet published.

It suffices for our present purpose to state that between the limits of 41° and 80° Fahrenheit we found the conductivity of the insulating covering of the Rangoon and Singapore Cable to increase nearly in the ratio of 1 : 7. The ratio of this enormous increase is however by no means constant, and in the absence of very elaborate and reliable experimental results we thought it advisable to test at a uniform temperature of 75° Fahrenheit (20 Cents.) This comparatively high degree of temperature has the advantage that it is seldom exceeded naturally, and that the conductivity being seven times greater at that temperature than at the winter temperature of 41°, the effect of minute faults upon the measuring instrument will also be proportionately exaggerated.

In order to insure uniformity of temperature the coils to be tested are placed twenty-four hours in tanks containing water regulated to 75°, they are then removed into the testing tank of the same temperature, which is hermetically closed, and hydraulic pressure of at least 600lb. per square inch applied, in order to force the water into the cavities or fissures that may present themselves. It is a remarkable fact, which is borne out by observation upon cables in process of submersion, that the application of hydrostatic pressure sensibly decreases the conductivity of gutta-percha, which however increases again slightly above the former ratio when the pressure is relieved.

In slightly defective coils the increase of external pressure produces, on the contrary, no increase, or even a decrease of insulating property, and a clue is thus obtained to ascertain otherwise inappreciable defects. The methods usually employed of measuring the conductivity and insulation of conductors in degrees, by simple galvanometer tests, would be insufficient for the purposes here intended.

It was necessary to express the conductivity of both the conductor and the insulating covering by simple numerical expression in units of resistance.

Unit of resistance adopted for expressing the degrees of conductivity of the conductor and the insulating material.

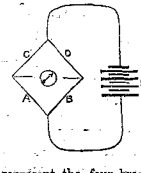
The unit of resistance we have adopted is that of a column of mercury 1 metre in length and of 1 square millimetre sectional area, taken at the freezing point of water. The advantages of this unit have been fully set forth by Mr. Werner Siemens in a treatise published in "Poggendorff's Annalen, vol. 110."

In expressing the degrees of conductivity of both the wire and the insulating medium in definite units of resistance we obtain not only the advantage of a more accurate comparison between the results of different indication, but subsequently when the separate coils are united with a single cable, we have an admirable means of judging its electrical condition if we compare the total resistances of both the conductor and insulating medium with the sum of the resistances previously obtained in testing each coil separately, due allowance being made, of course, for change of temperature.

But the principal advantage derived from this system of measuring consists in the facilities it affords in determining the position of a fault in the cable *while* it is being laid and *after* submersion.

In carrying this system into practice, we construct in the first place coils of definite resistance, which are capable of being combined in such a manner that we can vary the total resistance between the limits of 1 unit and 10,000.

By inserting these alterable resistances into one branch of a Wheatstone's bridge, the resistances of the copper or insulating covering of a cable of considerable length can be ascertained. If, however, it is required to ascertain resistances beyond the limits of the resistance coils, we adopt another arrangement on the principle of the Wheatstone's bridge, which consists in making the two permanent branches of the same also changeable.



A B C D represent the four branches of this arrangement, A C and B D being in connexion with the galvanometer, A B and C D the terminals of a battery.

App. No. 12.

Principles and practice involved in dealing with the electrical conditions of submarine electric telegraphs, by Werner and C. W. Siemens.

No current will pass through the instrument when the relation $\frac{A}{B} = \frac{C}{D}$ exists. But as in Wheatstone's arrangement A is always equal to B, the unknown resistance D is equal to the resistance C. A scale containing resistance coils from 1 — 10,000 units would therefore only allow us to ascertain resistances not exceeding these limits, but C and A being each composed of 3 variable coils of 10, 100, and 1,000 units respectively, we are enabled to measure any resistance between 0·01 and one million units with the same degree of accuracy. By means of this arrangement we measure the resistances of copper wire of any length and the insulation resistance of long cables within the limits of correctness of 0·2 per cent.

For the insulation tests of short pieces of cables or of longer cables of better insulating materials, such as india-rubber and *Wray's* mixture, such method is no longer applicable, because resistance coils of such diversity of dimensions as would be necessary could not be used with the sufficient accuracy, chiefly because the greater battery power that would be required would heat the smaller branches of the arrangement and *thus increasing* the resistance affect the result very considerably.

It was therefore necessary to turn to another method for ascertaining the value in units of the insulation resistances of short pieces of cable, say one knot in length. We employ in such cases a very sensitive sine galvanometer, or if the room permits of it, a Weber's reflecting galvanometer of 40,000 turns, and a magnetic reflector.

By means of an adjusting magnet the sensibility of this instrument can be varied between the limits of 1 and 100.

The astatic condition of the needles of the sine galvanometer being subject to changes, the constant of the instrument should be verified repeatedly while testing.

For the reading of this instrument in degrees, we substitute units of resistance by means of the following formula :—

$$R = \frac{\sin \phi}{\sin \varphi^1} \, n$$

in which R is the insulation resistance, ϕ the angle of deflection, φ_1 the constant of the instrument, n the number of elements employed.

For the derivation of this formula, see Appendix No. 1.

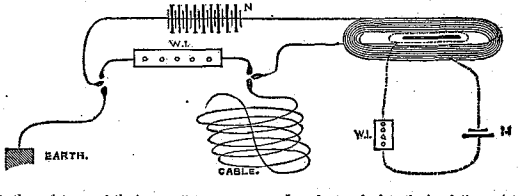
This method is applicable only for measuring great resistance between certain narrow limits. During the progress of the cable at the sheathing works, the insulation resistance gradually decreases, and the instrument would very soon be too sensitive. It could be made less sensitive, it is true, but in resorting to this it would no longer be possible to appreciate correctly the value of the resistance of the last coil added to the cable.

It was, therefore, necessary to resort to a means of maintaining the original degree of sensitiveness of the measuring instrument, while the total resistance gradually decreases.

For this purpose the coils of the sine galvanometer employed are surrounded by an additional coil of comparatively few turns, through which the current of a constant small battery continually passes.

The insulation current passes through the wire of the instrument, but is counteracted by the current in opposite direction in the outer coils, which is so regulated by means of a resistance coil, that no deflection of the galvanometer needle can be observed.

In adding to the length of the cable, the resistance coil in the outer circuit of the instrument has to be diminished by stopping till the equilibrium of the needle is restored; and the value of the alteration of the resistance coil being known in units, this number has only to be multiplied by the fixed proportion of the relative power of the outer and inner coil upon the needles to produce the correct result.



If W represents the resistance of the inner coil,
W^1 the resistance coil put into the inner circuit,
m, the number of cells of the battery of the inner circuit,
w, the resistance of outer coil,
w', the resistance coil put into the outer circuit.
n, the number of cells of the outer battery circuit, and
k, the number indicating the proportion of the effect of the outer and inner coil on the needle, we have,

$$\frac{n}{w + w^1} \quad k = \frac{m}{W + W^1} \; ; \text{ or}$$

$$k = \frac{m\,(w + w_1)}{(W + W_1)\,n} \, .$$

If instead of w the unknown resistance of the cable is introduced into the circuit, and by arranging the resistance w till the needle is perfectly at zero, the following equation is established,

$$\frac{M}{X + W^1} = \frac{N}{V_1 + w} \, . \, k \; ; \text{ or}$$

$$x = \frac{M\,(V + w)}{N \, . \, k} - W_1$$

or by introducing for k its value from above,

$$x = \frac{M\,n}{N\,m} \cdot \frac{W + W}{w + w_1} \cdot (V + w) - W_1$$

The chief advantage of this arrangement consists in the unchanged sensibility of the instrument, since the whole strength of the insulation current acts upon the needle, which nevertheless is brought back always to zero.

In measuring the insulation resistance of short cables the resistance of the galvanometer coils (W) may in practice be neglected, and the following more simple formula may be adopted :—

$$x = \frac{M}{N} \cdot \frac{V}{K}$$

The value of K is independent of the sensibility of the needle, and need only be determined once for all.

The tests are thus reduced to a very simple and easy method.

Methods for ascertaining insulation and copper resistances o cables of all lengths and forms.

In order to calculate the insulation resistance of insulated wires from the specific conductivity of the material used, and vice versâ, we employ the following formula :—

$$W = \frac{C \, . \, \log. \text{nat.} \frac{R}{r}}{2\,\pi\,\lambda}$$

The derivation of this formula has been given by Mr. Werner Siemens in Poggendorff's Annalen of 1857, vol. 102, and will be found in Appendix 2 of this paper.

The foregoing methods suffice to ascertain insulation and copper resistances of cables of all lengths and forms; they do not comprise, however, the test necessary to determine their inductive capacities.

Recent experiments hereafter given prove that the specific inductive capacity of insulating materials is more to be relied upon for permanency than their specific conductivity, the inductive capacity is, moreover, independent of local defects in the insulating covering, being dependent chiefly upon the general geometrical form of the insulator. In ascertaining, therefore, the inductive capacity of a length of cable, as compared with a standard Leyden jar, and in comparing this result with the total capacity due to the material employed, a means is obtained of ascertaining with great certainty whether the material is disposed throughout its length in equal thickness round the conductor, or whether the wire lays partly excentric. A knowledge of the inductive capacity of a cable is, moreover, absolutely necessary, in order to determine the position of a break in the conductor when the broken end remains insulated.

According to Faraday's conception, the inductive action is communicated, say from the interior electrified covering of a Leyden jar to the exterior, from atom to atom, through the dialectric. In our case the jar is represented by the cable, the inner covering of which is formed by the surface of the copper wire, the exterior by the water.

The laws which apply to the motion of heat and electricity in conductors are accordingly *directly* applicable to electro-induction, which may be expressed by the conductivity

multiplied by a constant varying with the nature of the insulating material.

Formula for representing the inductive capacity of any insulated wire.

Starting from this point of view the inductive capacity of any insulated wire will be represented by the formula

$$K = \frac{I\,C\,2\pi l}{\text{log. nat.}\,\frac{R}{r}},$$

in which the inductive capacity I takes the place of the specific conductivity λ of the previous formula. The unit measure of inductive capacity is assumed to be the capacity of a Leyden jar of two square plates of the unit of measure in breadth, and placed at the same distance apart.

Professor W. Thomson has obtained the same formula in a direct and most elegant manner, which differed from that of Mr. Werner Siemens in the value of the constant, proving that he started with another unit. Mr. Werner Siemens' method has been fully developed in Poggendorff's Annalen, vol. 102.

In dealing with cylindrical jars, or with cables, this formula may be written more simply thus;—

$$K = C\,\frac{I\,.\,C}{\log\frac{R}{r}}$$

In our experiments the inductive capacity of a Leyden jar is measured by the deflection of a galvanometer needle. If the deflection of the needle is caused by a current of very short duration, the quantity of the electricity passing through the galvanometer is equal to

$$K = \sin\frac{a}{2}\,\frac{}{E}$$

Instrument used for giving a steady deflection of the needle.

In practice it is found to be very difficult to read with sufficient accuracy the sudden deflection of a needle, and we prefer for practical use an instrument which we have placed before the section, enabling us to obtain a rapid succession of charging or discharging currents which in passing through the galvanometer produce a steady deflection of the needle, capable of being read with great accuracy. The value of these deflections is calculated by means of the following formula* :—

If A is the angle through which the sine galvanometer has to be turned to bring the needle to zero, C the number of charges or discharges per second, E the electromotive power of the battery, we have—

$$K = \frac{\sin a}{E}\,C$$

or if K_1 is the unit capacity of a jar and a the corresponding angle of readjustment of the instrument we have (if the number of discharges per second remains the same)—

$$K : K_1 = \sin a : \sin a_1$$
$$K = \frac{K_1 \sin a}{\sin a_1}$$

By permission of the British Government, we have been enabled to test the Government experimental cables by this method.

The results of these experiments which are given in the tables of the Appendix No. 3 show satisfactorily the accuracy of the methods employed. They also prove that the formula employed in calculating the specific inductive capacities which Professor W. Thomson and Mr. Werner Siemens obtained in entirely different ways, can be relied upon in practice.

Specific induction of differently covered wires.

The specific *induction* of all gutta-percha covered wires is shown to be nearly the same and to be entirely independent of its specific *conductivity*, while india-rubber, and its compounds, are far inferior in its specific induction to gutta-percha. The specific induction of gutta-percha being taken as a unit, that of india-rubber is equal to 0·7 only, and that of Wray's mixture = 0·8.

We have still to make mention of those methods which have been frequently resorted to of late of ascertaining by means of sensible electrometers the decrease of tension in a heavily charged cable when left to itself.

If E represents the tension of a galvanic battery in communication with the cable, as observed by a sine electrometer, y the remaining tension in the cable after an interval of time t, K the capacity, and v the resistance of the insulator, there will be, according to the law of Ohm, after the interval t a current of discharge $= \frac{y}{w}$, by which the tension is decreased during the time dt by dy. Hence we obtain the equation

$$-K\,.\,dy = \frac{y}{w}\,dt$$
$$-\frac{dy}{y} = \frac{dt}{Kw}$$
$$C - ln\,y = \frac{t}{Kw}$$

and since $t = 0$

$$y = E$$
$$lg\,n\,\frac{e}{y} = \frac{t}{Kw}$$

or
$$\frac{E}{y} = \frac{t}{lKw}$$

and
$$y = \frac{E}{\frac{t}{lKw}}$$

In a regular cable

$$K = J\,\frac{2l\pi}{ln\frac{R}{r}}$$
$$W = \frac{ln\,\frac{R}{r}}{\lambda 2\pi l}$$

or
$$KW = \frac{J}{\lambda}$$

and therefore
$$ln\,\frac{E}{y} = \frac{t\lambda}{J}$$

and

$$\lambda = \frac{T\,.\,ln\frac{E}{y}}{t} \qquad \text{(V.)}$$

and therefore

$$\lambda\;;\;J = \text{log. nat.}\,\frac{E}{y}\;;\;t$$

This method is well adapted for ascertaining the specific resistances of insulating materials, and to compare the insulation of two similar cables, even when no instrument capable of exact measurement is at hand. It suffices to observe the times required for the reduction of the original tensions to a given fraction. As the proportion $\frac{E}{y}$ although unknown is in each case the same, it is obvious from the former formula that

$$\frac{t_\lambda}{y} = \frac{t_1\lambda_1}{y}$$

and

$$\frac{\lambda}{\lambda_1} = \frac{t_1}{t}$$

where λ and t represent specific conductivities and times occupied in both experiments.

This result is independent of any excentricity of the wire in its insulating covering. The method is therefore well adapted for determining the specific resistance of materials, but as it is necessary to ascertain whether the wire is throughout the cable, concentric with the insulator, this method cannot be exclusively used.

Besides this process requires considerable time in testing well insulated cables. Again, another objection to its exclusive use arises from the possibility of slight faults in long cables passing unappreciated, as the loss of tension through such faults will be exceedingly small as compared with the whole charge.

We therefore prefer to determine the loss of tension not by an electrometer, but by measuring the charge a, and after the lapse of one minute the discharge b by the galvanometer needle. We then have the loss of quantity or tension during the minute

$$L = 1 - \frac{b}{a} \qquad \text{(VI.)}$$

In order to associate this formula with the system previously developed, it is only necessary to remark that $\frac{b}{a}$ is equal to $\frac{y}{E}$.

The cable having been tested from the earliest stage of its manufacture (in lengths of one knot) subsequently during the joining and covering of the cable, and finally during the paying out. These tests must strictly control each other, and must consequently be recorded systematically. The chief care during the submersion of the cable should be to detect at once the slightest change in its insulation, in order that the paying out machinery may be stopped instantly. It sometimes happens, however, that

* In this case the amount of charge is represented by a constant deflection, and therefore by sin a, whilst above, where it was given by one swing of the needle, it is equal to sin $\frac{a}{2}$.

App. No. 12.

Principles and practice involved in dealing with the electrical conditions of submarine electric telegraphs, by Werner and C.W. Siemen.

a fault does not appear immediately on submersion. It is therefore necessary, if a fault appears, to calculate its exact place before taking any other steps to remove it. In order to do this effectually, it is necessary to test the cable from both ends, *i.e.*, from the ship and from the land station, as the determination from one side gives only the maximum distance.

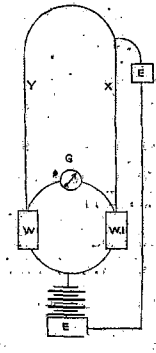
In paying out submarine cables, we pursue the following plan of testings:—

Mode of testings in paying out electric cables.

A clockwork arrangement at the land station is made to put the cable by rotation to earth, to the poles of a battery, and to insulation.

On board the ship there is constantly a bridge of resistances in connexion with the line. Whilst the electrician keeps the bridge in equilibrium, he is enabled to ascertain alternately the resistances of insulation and continuity.

The attendant of the station likewise observes the two datas, and transmits them telegraphically to the ship. If these four tests differ materially they indicate thereby the existence of a fault, the position of which can be calculated from the dates obtained. This method of observing the conditions of the cable, although very fatiguing to the electrician employed, has been found to answer perfectly well in paying out the Indian lines.



During the paying out of the Aden-Kurrachee section by Messrs. R. S. Newall and Co., we were by this means enabled to observe faults on five different occasions, which could then be removed without delay.

Our methods for determining the place of a fault are as follows:—

1st. When both ends of the cable are at hand let x and y represent the respective distances from each end of the cable to the fault, l the length of the whole cable, G a galvanometer, and W, W_1, two, graduated resistance coils. Then if W and W_1 are so adjusted that the galvanometer needle is perfectly quiet the place of the fault is given by the formula—

$$x = \frac{l\,w}{W \times W_1} \qquad \text{(VII.)}$$

This method has already been published by Werner Siemens (the Zeitschrift des Deutsch Oesterreichschen Telegraphen; Verems, 1857) having been used by us with perfect success ever since 1849.

In dealing with a single submerged line this method is no longer applicable. Let C denote the resistance of the length of the cable, x and y the resistances from each end to the fault, Z that of the fault itself and a_1 and b_1 resistances observed from each end respectively; the further end being insulated, a and b the same, whilst the further end is to earth. We have then by means of Ohm's law the following equations:—

$$1.\ C = x + y$$
$$2.\ a_1 = x + Z$$
$$3.\ b_1 = y + Z$$
$$4.\ a = x + Z \cdot \frac{y}{Z \times y}$$
$$5.\ b_1 = y + Z \cdot \frac{x}{Z \times x}$$

By eliminating Z and y the resistance x is found by the following expressions:—

$$x = a_1 - b_1 + \frac{C}{2} \qquad \text{(VIII.)}$$

$$x = a \cdot \frac{c-b}{a-b}\left(1 - \sqrt{\frac{b}{a}\cdot\frac{c-a}{c-b}}\right) \qquad \text{(IX.)}$$

$$\text{or } \frac{x}{y} = \sqrt{\frac{a}{b}\cdot\frac{c-b}{c-a}} \qquad \text{(X.)}$$

$$x = a\,\frac{\sqrt{(a_1-a)(c-a)}}{\sqrt{(a_1-a)(c-a)}}$$

If the cable was not perfectly well insulated before the fault under consideration appeared, the values a and b supply the means for determining an average resistance γ of the previous leakages. This resistance γ together with the final readings of insulation a_2, b_2, gives the place of the fault as follows:—

$$x = a_1 - \gamma\sqrt{\frac{a_1-a_2}{b_1-b_2}} \qquad \text{(XI.)}$$

In all these measurements the battery power must be so regulated as to keep the polarisation at the faulty place uniform. This is to be accomplished by determining preliminarily the place of the fault, then by regulating in the final measurement the number of cells so as to send from each side an equally powerful current through the fault, taking care not to take the observations till the polarisation has reached its maximum. We attach considerable importance to the last formula, which alone enables us to determine the situation of new faults in old defective cables, if only its previous electrical condition is known. This knowledge is unfortunately wanting in respect of nearly all the cables that have hitherto been laid. In the instance of the Rangoon and Singapore cable, we propose to furnish each station with a complete testing apparatus, and to cause daily tests to be instituted upon the cable, when laid, of its electrical conditions in each section.

Records of these observations should be forwarded *daily* to the chief electrician in charge of the line, who will then have the means at his disposal to watch the rise and gradual progress of faults, and to apply the remedy at the proper moment, and with a certain knowledge of the position and magnitude of every defect.

Considering the circumstance that owing to great care, the conductor of the Rangoon and Singapore cable is fully ten times more perfectly insulated than the best conductor hitherto submerged, we confidently expect that the result in practice, will also greatly exceed that of previous experience, still the insulating material employed remains the same, and is, therefore, liable to be affected by the same causes of failure.

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Appendix No. 13.

Electrical conditions of the Red Sea telegraph.

Report on the Electrical Conditions of the Red Sea Telegraph, by Siemens, Halske, and Co.

On the 28th of last April, after the arrival of the Imperador at Suez, the conditions of that portion of the cable which it brought were very carefully examined on board the ship; and, on the 21st of May, the portion of the cable in the Imperatrice was submitted to a similar examination in the port of Suakin.

The results of these tests are given in the following Table:—

Table I.

Cable.		Length.	Date of Test.	Resistance per knot	
				of Wire.	of Gutta Percha.
On board the	No.	Knots.		Units.	Millions.
Imperador	I.	399	28 April	8·6	26·0
(Suez Bay	II.	418	„	8·6	28·4
Imperatrice	III.	455	21 May	6·0	13·1
(Suakin	IV.	429	„	6·4	12·5

The unit of resistance which we have adopted in all our experiments and calculations is equal to that of a prism of mercury one metre in length by one square mille-metre sectional area. This is always to be understood when the values of resistance are given in units.

According to Table I. the resistance of the conducting wire of the cable on board the Imperador (Nos. I. and II.) was found to be equal to 8·6 units per knot at 80° F. temperature.

The experiments which were previously made at Birkenhead showed a resistance of 9·3 units per knot at 15c. When the end of the cable which was not connected with the battery was insulated, the resistance to the current passing through the gutta percha was nearly equal to 67,000 units.

The resistance which one knot of the gutta-percha covering opposed to the electrical current was, in Cable I.=26 millions, in No. II.=28 millions. The mean, therefore, =27 millions of units. The ratio of conductivity of the cable per knot to the conductivity of the gutta-percha

covering was, therefore, in the cable contained in the Imperador before submersion, 25,000,000 : 8·6, or as 3 millions to 1, nearly.

The resistances in Cables III. and IV. on board the Imperatrice were less favourable. The difference may be explained partly by the considerably increased temperature of the cable; as it is well known that the conductivity of gutta percha increases rapidly with an increasing temperature. That this was the case here is demonstrated by the fact that the resistance of gutta percha became nearly twice as great during and after submersion. The resistances of the gutta percha of the Cables I., II., III., and IV., reduced to that of one knot, was, at their departure from Birkenhead, supposing that of Cable I.=100, as follows :—

No. I. = 100
„ II. = 103
„ III. = 87
„ IV. = 81

We have been limited, in this computation, to a comparison between the cables themselves, as the instruments used at Birkenhead had not been compared with those at Suez, and in consequence of our method of comparing the resistances not having, at that time, been fully developed.

The respective ratios of these resistances coincide with those taken at Suez.

It is difficult to explain with entire satisfaction the far smaller resistance to conduction of cables III. and IV., *i.e.*, 6·2 instead of 8·6. This difference is partly due to a less perfect degree of insulation of the conducting wire, which would permit a considerable amount of the current to escape through the gutta-percha covering instead of passing through the whole length of the wire; but this alone does not nearly account for the difference.

During and after Submersion.

During and after submersion.

A continued series of tests were made while the process of submersion was going on, for the purpose of noting all the alterations which occurred, during that time, in the electrical condition of the cable. This was done in such a way that the place and the amount of any fault which might occur could, at any time, have been determined.*

The process of paying out was three times interrupted in consequence of accidental defects in insulation.

The first two occasions happened soon after the departure from the port of Cossire, and the third at a distance of 180 knots from Suakin, towards Aden. They were repaired by raising and cutting out the defective pieces.

Finally, a defect of considerable magnitude was observed, soon after submersion, in the Aden line, the distance of which was calculated to be within ten knots from Aden. This cable was raised to a length of seven knots, and the defect discovered and repaired.

The accompanying diagrams, of which a special explanation is annexed, show the alterations in the resistances of wire and gutta percha during and after the submersion, and also during the four weeks following.

The irregularities observed in these diagrams are chiefly due to the difficulty of making correct tests on board ship, while in motion; in addition to which it was necessary to take into account, as much as possible, the southerly increase of the intensity of the earth's magnetism in the Red Sea, and particularly a considerable alteration in the direction of the attractive force, exercised on the needle by an iron ship laden with an iron cable, which varied with every change in her course, and with the continued lessening of the disturbing cause, as the cable was paid out.

* Since defects in insulation rarely occur immediately after submersion (sometimes not in the space of days or even weeks), it is always absolutely necessary to determine, by calculation, the distance of a fault from the ship before the paying out is interrupted, and any attempt made to repair the injured part.

In order to be constantly supplied with data for confidently determining the distance of any fault which might appear, it is, in all cases, requisite to make tests of the resistance, as well of insulation as of continuity, at either end of the line. Such tests will afford data for four different determinations of the faulty place; two of which can be used for determining the place and magnitude of the fault through the imperfect insulating of the gutta percha; and the remaining two for proving the truth of these calculations. It will be seen by comparing the values arrived at from the different tests for determining the faulty place whether there exists only one or several defects. Other valuable data on these points may be gained in testing for loss of current simultaneously at both ends of the cable.

* These resistances are likewise shown in the following Table :—

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Electrical condition of the Red Sea telegraph.

TABLE II.

Line.	No. of Cable.	Length.	Date of Test.	Test from South.	Test from North.
		Knots.		Millions.	Millions.
After submersion.					
Suez-Cossire	I.	272·5	4 May	27·1	25·4
Cossire-Suakin	I. & II.	534	17 May	30·4	32
Suakin-Aden	III. IV.	637	4 June	22·8	21
Four weeks after submersion.					
Suez-Cossire	1.	- -	10 June	34·1	33·8
Cossire-Suakin	I. & II.	- -	„	- -	—
Suakin-Aden	III. & IV.	- -	5 July	27·9	16·7

Since the resistances of the wire had not been remarkably changed, those values were omitted from the Table, and instead of which we have put together the values for resistance of gutta percha, as they have been derived from tests at the two ends of each line.

In the second part of Table II., the spaces for the Cossire-Suakin line we have been obliged to leave blank. The resistances of insulation being small, in consequence of a large fault, could not be considered as resistance of gutta percha; and, therefore, could not be calculated per knot. However, tests have been made and put down on the accompanying diagrams.

On the whole it will be seen from Table II., as well as by inspection of the diagrams, that the resistance opposed by the gutta percha has continually increased during the process of paying out, as well as during the four weeks following. This change, so favourable to the insulation, was very considerable in the Suakin-Aden cable; and the fact of the high temperature, to which the cable on board the Imperatrice was exposed, being detrimental to its insulation, is fully confirmed.

The close correspondence of the tests of resistance, which were taken at the north and south ends of each cable, proves that they may be considered so far free from defects. This is not quite the case with the Suakin-Aden line, in which there is obviously a small defect near Suakin; it is, however, too small to be determined with any degree of certainty; and even if it were to increase to an extent a hundred times greater than it is at present, it would not be of sufficient magnitude, in any way, to interfere with the working of the cable; it was, therefore, in the Table, considered as a defect in the quality of gutta percha over the whole length.

Very different from this, however, is the state o the Cossire-Suakin line, which, although it remained in excellent condition until the 22nd May, a defect in the insulation was discovered then, which gradually increased until the 28th June, since which date it has remained nearly of the same magnitude.

The electrician at Suakin being ill at the time, Dr. Esselbach was unable, until the beginning of July, to make tests of the currents and resistances, in order to determine the position of the defect.

From these tests, and from those made at Cossire it will be seen that a defect of 850 units of resistance exists at a distance of 135 knots from Suakin. This calculation of the place of the defect has been corroborated by the diagrams showing the currents and resistances during the paying out which prove that the defect existed already during submersion. On the morning of the 16th May, soon after the place where, according to calculation, the defect exists was passed, a sudden leakage through the insulating covering was observed, as will appear by inspection of the four curves; but it was too small to justify us in stopping the paying out, and, after a few hours it entirely disappeared. It is, therefore, probable that an air-bubble, previously enclosed in the gutta percha, may have been broken by the hydrostatic pressure, and a small cavity opened thereby which had reached to the conducting wire for a while, but had been subsequently closed by a continuation of the same or an increased pressure. For a period of eight days following this, perfect insulation was re-established, when the water must have again penetrated to the conducting wire. The nearly constant resistance of this defect gives room for hope that during the next few

App. No. 13. Electrical condition of the Red Sea telegraph.

months it may again disappear in consequence of the formation of hydrated oxide of copper at the opening. This will most probably be the case also with the fault in the Suakin–Aden line particularly as it is nearly 100 times smaller than the former, provided the conducting wire is enclosed centrically in the insulating medium, at the place of the fault; but it must be left to time to decide this. The defect in the Cossire-Suakin line exercises no detrimental influence whatever on the present working condition of the cable; *i. e.* the celerity and certainty with which messages can be transmitted. And so far is this true, that even if the fault were five times greater than it is, it could not affect the utility of the cable to any serious extent. The fault which was observed in the Aden–Suakan line (resistance about 20 units) near Aden, and which was consequently at least 45 times greater than the one above-mentioned, was inconvenient to the speaking through it. Correspondence could, however, even under these circumstances, be sustained with considerable certainty. It ought to be remarked that a defect in a line near to a station, as was the case here is of less importance, in an electrical sense, than a fault of equal magnitude midway between the stations.

We may state in conclusion, as the result of our investigations aided by the accompanying diagrams.

1st. That, only excepting the two defects above mentioned, the insulation of all the cables composing the line is in first rate condition so far as we are enabled to judge by comparison with former cable layings.

2nd. That the conductivity of the conducting wire is such as could only have been obtained by using a very pure quality of copper.

3rd. That the Suez-Cossire line is without defect.

4th. That the Cossire-Suakin line contains a defect (the resistance of which is 850 units) about 135 knots from Suakin. This defect did not in its state on the 5th July, when the cable was handed over to the charge of the company, in any way retard speaking through the line, and even if it should have greatly increased since, would only retard the speed of transmission to an inconsiderable extent. But it is possible the defect may in time disappear.

5th. That a very slight defect exists in the Suakin-Aden line, which will probably disappear, and which, even in the event of its remaining as it is, does not interfere in the least degree with the working of the cables.

Should anything given in the above Report and its accompanying diagrams seem to the reader to be insufficiently considered, we must remark, that believing our attention chiefly due to the task of showing the main features of the cable's condition, we have not entered into details of minor importance, which we should have done had the Report been intended for a specially scientific purpose.

Explanation of the Diagrams.

Explanation of the diagrams.

The diagrams are arranged in the numerica Porder I., II., III., with an additional indicating letter A, B, C.

The figures I., II., III., refer to the 3 sections,

I. Suez-Cossire.
II. Cossire-Suakin.
III. Suakin-Aden.

whilst the additional letter to each of these figures indicates the special regard in which the respective diagrams show the conditions of the cables.

A, During submersion and following month.
B, Resistance during submersion.
C, Strength of current during submersion.

Since the quality of a cable is to be judged by the ratio of resistances which its conducting wire and the gutta-percha covering relatively oppose to the passage of the electric fluid, it was our chief object to observe the variations in these two resistances which could be deduced.

1. By observed changes of the intensity of the current.

2. Directly, by comparing resistance coils with the cable, and altering them until they produce upon the current an effect equal to that produced by the cable.

The experiments made in the Red Sea convinced Mr. Werner Siemens, that the latter method is, in practice, by far the more preferable; as both instrument and battery reductions, which are always tedious and uncertain are avoided by its adoption. In the Red Sea both methods were applied. The green and red curves represent the resistance of the gutta percha; the blue and black, the resistance of the conducting wire. Taken in another view, the blue and green curves show the state of the cable observed from the north end; those in black and red as observed from the south end of the cable, or on board the ship during the paying out, which was done in a direction from north to south.

The three diagrams A, represent the conditions of the three sections during submersion and the month following, according to daily observations. The successive days are laid down as points of the base, the perpendiculars above them indicating the resistances observed on those days.

The most favourable ratio of the resistances of the gutta percha to that of the conducting wire will be seen in the diagrams representing the resistances of the Suez-Cossire line (I. A). The other diagrams show the decrease of the insulation in the inverse ratio of the increasing length of cable, as was natural.

The ratio between these two resistances appears somewhat too favourable for the length of the cable in the Aden-Suakin section (III. A). But this may, perhaps, be due to the resistances being chiefly computed from observed intensities which deviate occasionally as much as one-sixth of their real values.

The insulating covering of the Suakin-Cossire cable (II. A), since the defect, opposes scarcely more resistance to the passage of the electric current transmitted from Cossire than the conducting wire, and to that sent from Suakin still less. From this it might erroneously be concluded that in its passage, the electric current is wholly lost through leakage; but this could not be so as, according to the law of Ohm, only part of the current would be lost and the remainder pass along the line, in the inverse ratio of the resistances. At the commencement of June direct tests showed a loss of $\frac{1}{3}$ at most; and we believe that, even in its worse state, the loss will not amount to more than $\frac{2}{3}$.

The diagrams B and C are special diagrams, shewing the state during submersion according to hourly observations. B and C have not been constructed for Section I., for the reason given in the special explanation to that Section.

The diagrams B contain the resistances during the paying out, as being the more obvious.

The respective lengths of cable paid out are taken as the base, while the perpendiculars erected thereon represent the observed resistances, namely, of wire on a twice larger scale, of gutta percha on a scale four times as small compared with diagram A.

Since we limited ourselves, during the paying out, to tests of current more than of resistance, we have added these diagrams, representing the observed strength of current in the same manner as the resistances to which they are reciprocal; but laying down the currents through gutta percha on an eight-fold scale compared with the current through copper.

I.—Section from Suez to Cossire.

Cable I. (Imperador.)

Section from Suez to Cossire.

Diagram I. A.—The insulation became worse until the 16th of May, at which date its good condition was perfectly re-established by repairing the land line at Suez, and it remained so until the time of the rupture of the cable on the 12th June. The values indicated within inverted commas were obtained after the repairs on the 14th July were effected, the cable being, at that time, already in the hands of the Red Sea company.

Special diagrams (B and C), showing the hourly observations made during the two days of paying out, have not been constructed, as the submersion was effected without accident, and as, in any event, it would have been impracticable, in consequence of the defective insulation of the land line at Suez, to depend upon the observed variations.

II.—Section from Cossire to Suakin.

Remainder of Cables I. and II. (Imperador.)

Section from Cossire to Suakin.

The general diagram shows two interruptions of the paying out directly after departure, on the 10th and 12th May, the result of defective insulation.

After our departure for the third time, the hourly observed resistances remained regular; the resistance of the gutta percha increasing gradually as the cable left the ship.

On the 16th at eight o'clock, a.m., there was observed on board the ship, as well as at Cossire, a decrease of insulation, accompanied by considerable disturbances of the magnetic needle. The defect thus indicated was not of sufficient magnitude to justify the interruption of the submersion, and disappeared altogether after the lapse of a few hours.

An extensive decrease of insulation appeared on the 22nd May, after the cable was laid, and the ships had left Suakin for Aden. Experiments subsequently made (ignorant, however, of the fault above mentioned) to determine the distance of the defect, placed it at 135 knots from Suakin, that is to say, at about the spot where the disturbances alluded to above occurred. In the beginning of July the resistance of this fault was equal to 850 units.

APPENDIX No. 13—*continued.* App. No. 13.

Electrical condition of the Red Sea telegraph.

Section from Suakin to Aden.

III.—SECTION FROM SUAKIN TO ADEN.

Cables III. and IV. (Imperatrice).

The Diagram III. B shows that during the submersion the resistance of gutta percha became nearly doubled.

A confirmed leakage, not far from the ship, showing itself on the morning of the 23rd May (III. A, B, C), the paying out was interrupted. On withdrawing part of the cable and removing the faulty piece the conducting wire was found to be enclosed excentrically in the gutta percha covering. This having been set right by taking out the excentric portion and rejoining the cable, the submersion was proceeded with, and no interruption of any importance afterwards occurred.

On the 26th May, at 4 o'clock P.M. (III. B and C), the entire submersion of Cable III. was effected, and it was connected with Cable IV., in consequence of which the intensity of the insulation current was doubled, and, reciprocally, that of the passing current reduced to about one-half its former amount.

Speaking through a cable when coiled up being inconvenient, on account of the extra current, we made a puncture and connected our instrument to this intermediate point of the conducting wire, which when done left the insulation current unchanged; but increased the current passing through the conductor according to the law of resistances, as was to be expected.

At 3 o'clock tests were applied to the whole length of the cable, as the paying out had so far proceeded as to nearly reach the punctures; and (off Adeu) at 7.40 A.M., the superfluous cable having been cut off, the final observations were made.

In order to have at a single view all the major variations of the cable, independent of the altered circuit, an additional piece has been attached to this part of the Diagram III. C, showing the tests reduced to the length of Cable III. which had just been paid out.

After laying the deep-sea cables, and when the shore ends were completed, a serious defect near Aden, which rendered speaking through the cable difficult, was discovered. The resistance following it was 20 metres. In order to take up and remove the injured piece, the Imperatrice was dispatched and successfully accomplished her task.

It is improbable that the sudden rise seen to occur in the red curve (of 10th June) is referrible to any sudden improvement in insulation, although the disturbance (seen in the oscillations of the needle) increase with the rising curve and may be estimated therefore as considerable. The green line, which has slightly fallen, indicates the small defect mentioned at the conclusion of the Report.

In general the *resistances* given in these diagrams are slightly in advance, for the reasons given above, and the reduced resistances in the Table (Appendix I.) are more to be depended on.

SIEMENS AND HALSKE.

Electrical conditions of the Aden and Kurrachee section of the Red Sea telegraph.

REPORT on the ELECTRICAL CONDITIONS of the ADEN-KURRACHEE SECTION of the RED SEA CABLE, shortly before, during, and for One Month after its Submersion, or in January, February, and March 1860.

The S.S. "Imperador," with portion of the cable on board, arrived at Aden on the 10th December 1859, and at Kurrachee on the 24th. The "Imperatrix" joined the expedition at Kurrachee on the 11th January 1860.

The mode of testing the Indian cable was the same as devised by Mr. Werner Siemens for testing the Red Sea and Singapore-Batavia lines; it consists in measuring the resistance of the conductor, as well as of the insulating materials by means of resistance coils capable of accurate adjustment.

During submersion the cables were tested alternately from the ship and from the station; the latter reporting the results obtained to the electricians on board during short intervals permitted by the contact arrangement of the clockwork employed in our method of testing.

The conditions of the several sections of the line previous to, and shortly after submersion, and at the termination of the period of guarantee are given in the annexed Table No. 1 and A, B, and C.

The Table No. I. shows at a glance the good condition of the line up to the day of our final tests.

Table No. II. records the tests and interruptions that took place during the submersion of the cables, the figures given in this Table are the average of a series of tests extending over six hours. Column No. 10 contains the numbers given to the several faults that occurred, and were removed, being six in all, not including a breakage of the cable that took place in the harbour of Muscat in consequence of a gale before the vessel started.

The appearance of the faults 2 to 5 was very sudden, they all were found situate close to the ship, and were removed by hauling in the last part of the cable paid out, and by cutting it out.

The fault No. 6 was an old one in the cable laid down in June 1859 between Aden and Maculla. The position was according to tests fixed to be within 20 knots of Maculla. These 20 knots were taken on board, and the fault was found to be exactly in the middle. A new piece of cable was laid down to replace these 20 knots.

The instruments for the transmission of messages in the stations Kurrachee, Muscat, Helania, and Aden were put up in the same manner as in the stations of the Red Sea line, and a complete testing apparatus for ascertaining the condition of the line by regular tests was added.

The construction of the telegraphic instruments is such as to enable the manipulator to use them either as terminals, intermediate, or translation instruments.

During the period of the guarantee, when our assistants were at the stations, the messages were always sent by translation or transmission.

Through the Kurrachee-Muscat, the Muscat-Helania, and the Helania-Maculla section (that is before the last was joined on to the Aden-Maculla piece) an average speed of ten words per minute was attained.

Through the entire length from Aden to Helania six words per minute were sent without difficulty.

(Signed) SIEMENS, HALSKE, & Co.

London, Nov. 15, 1860.

TABLE I.—RECORD of TESTS of the ADEN-KURRACHEE CABLE shortly before, immediately after its Submersion, and at the Termination of the Period of Guarantee.

No.	Name of Ship or Section of the Line.	Date.	No. of Cable.	Length in Knots.	Resistance of the Copper Wire. Measured from the				Resistance of the Gutta-percha. Measured from the			
					East End Top End.	West End Bottom End.	At an Average.	Per Knot.	East End Top End.	West End Bottom End.	At an Average.	Per Knot.
1	2	3	4	5	6	7	8	9	10	11	12	13
			A. BEFORE LAYING.									
1	Imperador - -	7th Jan.	I.	443	3605	3603·5	3604·2	8·34	106,675	105,500	106,087	47,218,000
2	" - -	4th "	II.	457	3781	3804	3792·5	8·3	88,500	88,500	88,650	40,513,000
3	Imperatrix - -	26th "	I.	421	4043	4044	4044·5	8·235	72,500	72,466	72,533	35,610,000
4	" - -	" "	II.	443	3034	3653	3683·5	8·314	69,633	69,633	69,633	30,840,000
			B. AFTER LAYING.									
5	Kurrachee—Muscat	10th "	Imperador (I. & II.)	515	4056	4054·25	4055	7·873	272,843	236,000	254,421	131,024,300
6	Muscat—Helania -	4th Feb.	Imperatrix (I. & II.)	406·6	3973	3958·0	3966	7·937	162,700	150,000	156,350	77,806,000
7	Ship—Helania- -	12th "	Imperador (II.) & Imperatriz (II.)	735	5729	5320	5522·5	7·544	91,500	85,000	88,500	64,770,000
8	Maculla—Helania -	8th "	Imperador (II.) & Imperatrix (II.)	504·5	—	5955	5955	7·839	159,500	159,500	159,250	80,320,000
9	Maculla—Aden -	12th "		232·5	—	—	—	—	163,500	169,500	156,500	36,476,000
10	Helania—Aden -	16th "	Imperatrix (II.) and cable laid (1859).	737	5947	5610	5778·5	7·835	85,000	80,500	82,750	61,030,000
			C. AFTER THE PERIOD OF THE GUARANTEE.									
11	Kurrachee—Muscat	17th "	Imperador (I. & II.)	515	3971·8	4223·5	4097·65	7·956	207,150	207,750	207,450	106,830,000
12	Muscat—Helania -	20th "	Imperatrix (I. & II.)	406·6	3555·66	3918·66	3837·16	7·926	137,817	131,333·3	134,575·15	66,848,600
13	Helania—Aden -	11th Mar.	Imperatrix (II. and cable laid 1859.)	737	6284·3	5940·66	6112·5	8·29	73,717·66	76,720	75,218·83	55,476,500

App. No. 13.
Red Sea telegraph.
Aden-Kurrachee section.

Appendix No. 13—*continued.*

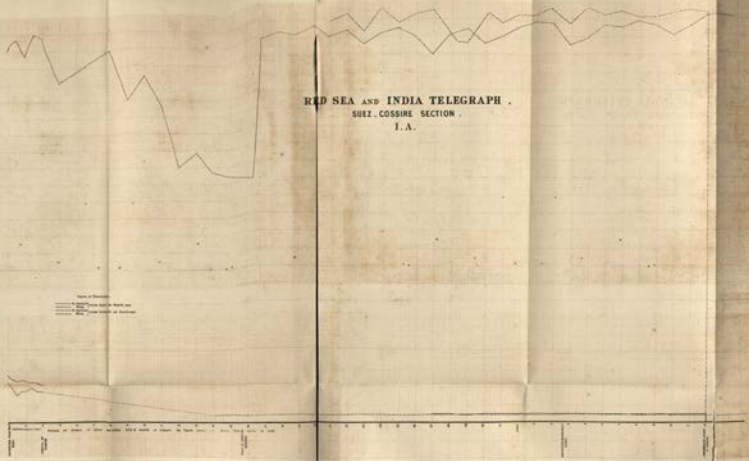
Table No. 2.—Record of Tests during Submersion of the Aden–Kurrachee Cable.

Section.	Date, 1860. Day.	Hour.	Length. In circuit.	Paid out.	Copper Resistance. East End.	West End.	Gutta-percha Resistance. East End.	West End.	No. of Fault.	Remarks.
1	2	3	4	5	6	7	8	9	10	11
I.	Jan.					Kurrachee–Muscat.				
	13	12	443	0	3,638·4	3,638	109,275	106,800		Ship starts at 2h. 5m. p.m.
		6		18·3	3,635·5	3,635	107,210	105,000		
	14	12 night		53·2	3,634·6	3,641	113,710	105,900		
		6		88·4	3,827·6	3,721	115,700	108,375		
		12		131	3,894	3,825	123,825	118,700		
		2		144·5	3,991	3,825	130,000	125,630		*Fault* 1. The irregularities of these test were owing to a destruction of the earth plates that accidentally had taken place.
		2. 40			14,000	5,000	140,000	132,000	1	
		6		170	3,578	3,524	146,925	130,000		
	15	12 night		209	3,593	3,602	162,430	152,203		
		6		246	3,603·5	3,574	168,930	155,900		
		12		290·5	3,569·5	3,588·5	181,250	174,490		
		4		304·6	3,607·3	3,574	195,000	182,250		Ship stops.
			760·5	304·8	6,092	6,008	74,700	70,000		
	16	9		309·5	6,022	6,030	73,240	65,550		
		12		329	6,005·5	5,998	72,526	68,365		
		6		374·7	6,077·4	6,026·5	72,030	71,180		
	17	12 night		418	5,971	5,986	80,470	76,760		
		6		461·4	6,046	5,946·5	86,623	83,720		
		1		513·4	6,053	5,863	94,680	90,210		Ship stops, and cable cut.
		3	515	515	4,019	4,065	224,500	229,500		
	19			515	4,056	4,054·25	272,843	236,000		
II.						Muscat–Helania.				
	24	7	694·5	1·5		4,003	79,500	65,000		Ship starts at 6h. 20m. a.m.
		10		17	4,040	4,045	81,500	76,700		
		10. 15	694·5	17·2	400	4,007	c. 1,400	c. 4,221	2	*Fault* 2.
		1	17·2	17·2	144	124·5	c. 805	850–1,000		
		11 night	493	17·2	4,030	4,020	80,000	73,500		Ship starts again.
	25	2		22·9	4,036	4,025	80,700	78,000		
		2. 29	493	25	3,500	4,015	c. 14,950	c. 16,900	3	*Fault* 3.
		4		25	205·5	198·9	—	—		
	26	4	484·5	15	3,935	3,945	81,000	80,000		
		6		22·8	3,943	3,940	82,000	81,000		
	27	12 night		63	3,940	3,930	84,500	86,500		
		6		104·4	3,938	3,913	95,000	94,200		
		12		144	3,939	3,904	99,350	100,000		
		5		181·6	3,940	3,889	111,100	102,000		
	28	12 night		220	3,927	3,886	117,000	114,000		
		6		261·7	3,945	3,857	121,000	122,400		
		12		305·9	3,940	3,870	139,500	139,000		
		6		347	3,950	3,840	162,500	147,000		
	29	12 night		390·7	3,910	3,860	161,000	160,000		
		6		434·1	3,932	3,834	172,000	170,000		
		12		477·5	3,953	3,845	176,400	175,000		
		8	509·5	—	4,060	4,060	164,700	156,000		
		8. 53		484·7	4,078	{3,900 / 4,015}	120,000	{95,000 / 60,000}	4	*Fault* 4. After the splice has been made.
	30	8. 42	484·5				172,000	175,000		
		12 night	502·5	484·5	4,081	4,056	154,000	154,000		Ship starts again.
		3		484·7	4,077	4,057	162,000	160,000		Ship starts and cable is cut.
	Feb. 4	1	496·75	496·6	3,978	3,958	162,700	150,000		Tests taken at the station.
III.						Helania–Maculla.				
	4		485		—	—	69,000	68,000		
		9			3,992	3,995	62,800	62,000		
	5	12 night			3,992	3,994	62,700	62,000		
		6			3,993	3,995	62,770	62,000		
		12		31·4	3,977	3,992	64,000	62,000		
		6		70·4	3,985	3,975	67,470	66,500		
	6	12 night		112·5	3,964	3,960	72,200	71,500		
		6		150·	3,972	3,940	75,750	74,500		
		12		195·7	3,917	3,937	83,970	84,250		
		6		235·7	3,924	3,917	96,500	97,000		
	7	12 night		276·7	3,930	3,895	109,200	113,000		
		6		316·4	3,944	3,865	121,000	120,000		
		11		354	3,928	3,860	125,300	123,000		
		11·25	485	354	—	3,853	63,800	22/100,000	5	*Fault* 5. Takes place at the same time when Captain Atkins' boat is capsized.
		1·4	362	362	3,839	2,782	c. 4,540	c. 18⁰/₁₀		
		4·45	361·25	361·25	—	—	—	218,000		
		9	484·25	361·23	3,908	3,882	125,000	125,000		
	8	12 night		384·5	3,893	3,885	133,100	132,500		
		6		424·1	3,927	3,849	143,700	144,500		
		12		460·4	3,933	3,829	161,500	157,000		

RED SEA AND INDIA TELEGRAPH.

SUEZ - COSSIRE SECTION.

I. A.

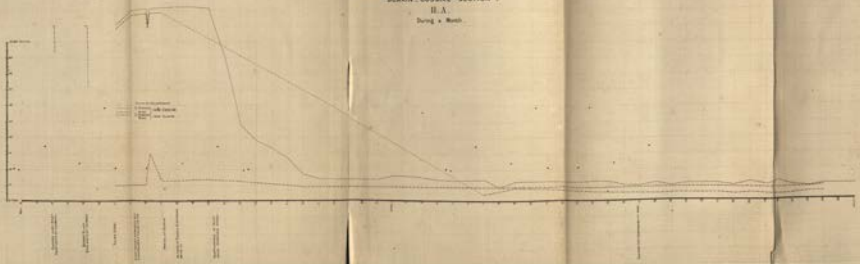


RED SEA AND INDIA TELEGRAPH.

SUAKIN - COSSIRE SECTION.

H. A.

During a Month.

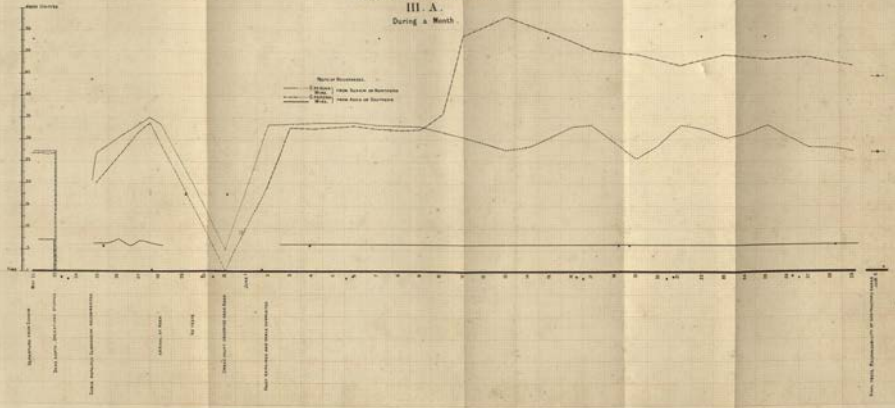


RED SEA AND INDIA TELEGRAPH.

ADEN - SUAKIN SECTION.

III. A.

During a Month



RED SEA AND INDIA TELEGRAPH.

SUAKIN. COSSIRE SECTION.

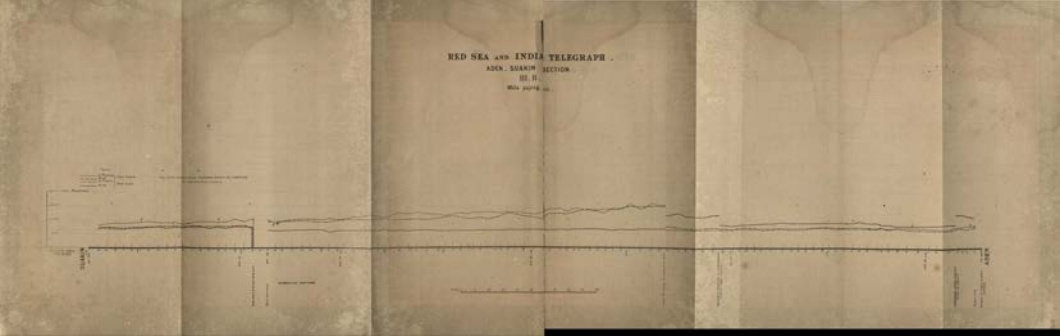
H. B.

While paying out.

RED SEA AND INDIA TELEGRAPH.

ADEN. BOMBAY SECTION.

III. II.

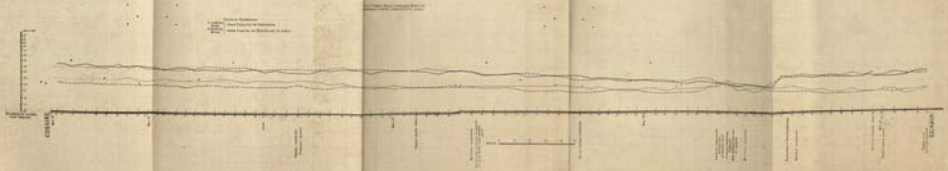


RED SEA AND INDIA TELEGRAPH.

SUAKIN. COSSIRE SECTION.

H.C.

Currents while paying out.

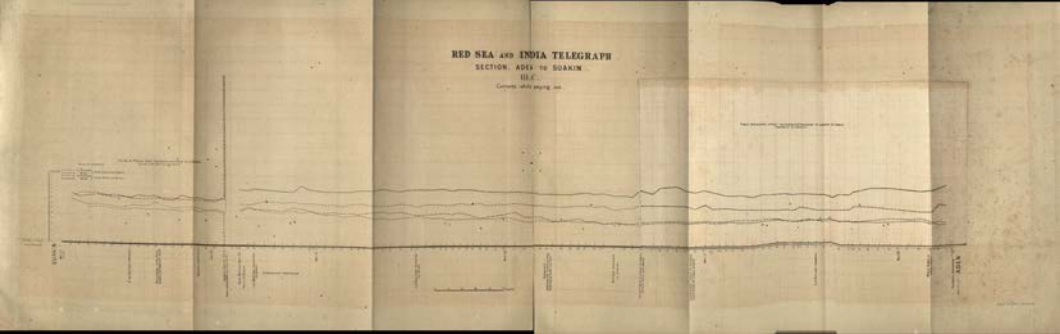


RED SEA AND INDIA TELEGRAPH

SECTION. ADEN TO SUAKIN

H.E.C.

Currents while paying out.



APPENDIX No. 13.—*continued.*

App. No. 13.

Red Sea telegraph.

Aden-Kurrachee section.

Table No. 2.—Record of Tests during Submersion of the Aden–Kurrachee Cable—*continued.*

Section.	Date, 1860. Day.	Hour.	Length. In circuit.	Paid out.	Copper Resistance. East End	West End.	Gutta-percha Resistance. East End.	West End.	No. of Fault.	Remarks.
1	2	3	4	5	6	7	8	9	10	11
III.	Feb.				HELANIA-MACULLA—*continued.*					
cont.	8	3·30		484·0	8,801	3,932	170,100	165,200		Ship stops. The position of the fault towards Aden is ascertained by tests up to 11th February. Ship advances to a distance of 232·5 knots from Aden, and the splice is made there.
	11	6	732	484	5,325	5,555	78,300	76,800		
	12	12 night			5,402	5,480	80,400	80,000		
		6	732	484	—	5,468	—	82,000		
		10			5,725	5,320	91,400	85,500		
		12	504·5	504·5	—	3,955	159,500	159,000		
					Determination of situation of fault in line already laid					
IV.					MACULLA–ADEN.					
	9		253	253	2,095·5	1,725·8	—	—	6	*Fault* 6. 20 knots containing the fault are cut out and replaced by new cable.
			20·5	20·5	168·1	168·3	—	—		
	10	4·14 n.	232·5	232·5	—	—	153,600	15⁸/₁,000		
V.					ADEN–HELANIA.					
					= 504·5 knots new cable + 232·5 knots old cable.					
	16		737	737	5,947	5,610	85,000	80,500		

SIEMENS, HALSKE, and Co.'s METHODS of determining the DISTANCES of FAULTS.

Methods of determining the distance of faults.

I. *When both Ends of the Cable are connected with the Testing Board.*

The ends of the cable are to be connected to the galvanometer angles of Wheatstone's parallelogram, in order to form two sides of it, and the battery put in circuit, using the faulty place as earth plate.

R. Let R represent the resistance of the length of the whole cable.

x. y. The respective resistances of the shorter and longer parts, from the ends to the leakage, and

w. w_1. that of the two other sides of the parallelogram. Then, in order to prevent any of the current passing through the galvanometer, it is necessary to add resistance $= r$ to that of the smaller part $= x$ of the faulty cable.

By knowing the resistance of the entire cable ($= R$), and observing the resistance ($= r$) necessary to be added, we obtain—

$$x = \frac{wR - w_1 r}{w + w_1}$$

$$y = \frac{wR + w r}{w + w_1}$$

and by dividing $x + y$ by the resistance of the cable per knot ($= \phi$), we arrive at the distance of the fault from the end of the cable—

$$\frac{x}{\phi} \text{ and } \frac{y}{\phi}$$

Messrs. Siemens, Halske, and Co. prefer this method, when practicable, as it is independent of polarization. It is particularly useful in $\frac{\text{manifold}}{\text{compound}}$ cables, so long as one wire remains well insulated.

II. *With a Single Line submerged.*

The tests of continuity and insulation from both ends, together with the known resistance of the cable per knot, supply materials for calculating the place of the fault in four different ways. Each may, therefore, be used as a check on the others. The data thus afforded may be increased, if desired, by interposing between the cable and battery various known resistances.

Messrs. S., H., & Co. ordinarily avail themselves of the following three sets of tests:—

I. Where a_i = insulation from one end,
b_i = do. from the opposite end, and
c = known resistance of the cable when sound.

II. a = continuity from one end,
b = do. from the opposite end, and
c = ut supra; or

III. a_i = insulation, and } from the same end,
a = continuity } and
c = ut supra.

From these are derived the resistances x and y from the ends A and B, as follows:—

From I. $$x = \frac{a_i - b_i}{2} + \frac{c}{2}$$

$$y = \frac{b_i - a_i}{2} + \frac{c}{2}$$

From II. $$x = \frac{c\sqrt{u}}{1 + \sqrt{u}}$$

$$y = \frac{c}{1 + \sqrt{u}}$$

where $u = \frac{a}{b} \cdot \frac{c - b}{c - a}$

If the line were not in a perfect state before the fault appeared, c, in this equation, should be replaced by c_1 and c_2, viz.:—

$$u = \frac{a}{b} \cdot \frac{c_1 - b}{c_2 - a}$$

where c_1 and c_2 represent the values of a and b before the questionable fault appeared.

Form III. is obtained—

$$x = a - \sqrt{(ai - a) \cdot (c - a)}$$

These formulæ are based on the supposition that—

1st. The line contains only one fault.

2nd. The earth contact is the same in both tests, and is not polarized, otherwise, in no case, will these conditions be realized.

The tests are to be $\frac{\text{compared}}{\text{combined}}$ and corrected according to circumstances.

With regard to battery power, Messrs. S., H., & Co. advise the employment of $\frac{\text{intensity}}{\text{strength}}$ only differing in its amount at each end, according to the distance of the fault, in order to obtain a more equal degree of polarization in $\frac{\text{both}}{\text{each}}$ tests.

After having roughly ascertained x, y, and z the resistance of the leakage), the relative numbers of cells n and n_1 to be taken for the final tests, are—

I. $$\frac{n}{n_i} = \frac{y + z}{x + z}$$

II. $$\frac{n}{n_i} = \frac{x}{y}, \text{ or}$$

III. $$\frac{n}{n_i} = \frac{y(x + z)}{x(y + z)}$$

relatively to the sets of tests given above.

App. No. 14.

Appendix No. 14.

On the Insulating Properties of Gutta-percha.—By Fleeming Jenkin.

Insulating properties of gutta percha. by Fleeming Jenkin.

The experiments described in this paper were undertaken with the view of determining the resistance opposed by the gutta-percha coating of submarine cables, at various temperatures, to the passage of an electric current, gutta-percha being considered as a conductor offering a resistance to the electric fluid similar to that offered by any so-called good conductor, such as copper or iron.

The first measurement given of the resistance of a substance usually called a non-conductor, which the author can find, is that given by Professor Thomson, of gutta-percha, in a lecture before the British Association at Dublin in 1857. Professor Thomson's results will be further alluded to in the course of the present paper.

The present experiments were made on various specimens of submarine cable at the works of Messrs. R. S. Newall and Co., at Birkenhead. They for the most part consist in measurements of the current which passed between the interior and the exterior of a gutta-percha covering when these two surfaces were placed, by means of conductors of small resistance, in connexion with the opposite poles of a powerful battery.

When the outer surface of the gutta-percha is in connexion with the earth and one pole of a battery (Fig. 1), whilst the metallic core is in connexion with the other pole of the battery, the current passing through the cylindrical coating is commonly called the loss or escape of electricity, and for the sake of brevity this term will be occasionally used with the above meaning—the insulated conductor being looked upon as a sort of reservoir, in which electricity supplied by one pole of the battery is confined by the gutta-percha coating.

The loss from a submarine cable or wire coated with gutta-percha increases in proportion to the length of the wire, inasmuch as the area of the gutta-percha, which here acts as a conductor, increases, and thus, by choosing for the experiment a sufficient length of cable, the area of the gutta-percha conductor becomes so large that the current passing through it may be easily compared with that coming from the same battery, which passes along a resistance coil of moderate dimensions, or even through the copper core of the cable itself. The ratio of the resistance of the two currents may be determined by measurements on a galvanometer, or on two galvanometers, the proportion between which is known, or by the arrangement known as Wheatstone's parallelogram, or differential arrangement. The second of these two methods was preferred in the present experiments, for reasons which will appear as the experiments are described in detail.

The resistance of the cylindrical coating relatively to the copper conductor or resistance coil being known, the resistance of a cubic foot of gutta-percha can be deduced by means of a formula due to Professor Thomson. The details and results of this calculation applied to tests on long submarine cables, and the particulars of those tests, are given in the second part of this paper.

There is, however, great difficulty in ascertaining the exact temperature of a considerable length of cable, and still greater difficulty in varying the temperature at will. Whilst, therefore, the absolute resistance of gutta-percha has been calculated from the loss on long submarine cables, the relative resistance of the gutta-percha at various temperatures has been determined by means of the loss on shorter lengths, which were capable of being heated or cooled by complete and continued immersion in water.

These experiments are described in the first part of this paper. By these no absolute measurement of the resistance is given, but numbers are obtained proportional to the loss as above defined, and therefore inversely proportional to the resistance of gutta-percha at the several temperatures. These numbers are tabulated and thrown into curves, which show graphically the change in the insulating properties of the material.

By the experiments and calculations given in the second part of the paper, the absolute resistance of the material was obtained for some few temperatures. The resistance at other temperatures results directly from the tables or curves given in Part I.

Part I.

Experiments on the Insulation of Short Lengths of Gutta-percha covered Wire, at various temperatures.

Two coils of wire, which will in the course of this paper be called No. 1 and No. 2, were obtained from the Gutta-percha Works, Wharf Road, London.

The length of wire in each coil was 2,028 yards or one knot; the copper conductor in each weighed 73 lbs. It consisted of a single wire of about 0·06 in. in diameter, the measurement being made by Cocker's patent wire gauge. No. 1 was covered according to Chatterton's patent; the wire first received a coating of Chatterton's compound; a coating of gutta-percha was then applied; this was covered by a layer of compound, followed by another coating of gutta-percha. The gutta-percha and compound weighed 160 lbs., and the total diameter of the covered wire was pretty accurately 0·3 in.

The wire in No. 2 was covered with two coats of pure gutta-percha to the diameter of 0·3 in.; the total weight of gutta-percha was 152 lbs.

The diameter of the gutta-percha is a somewhat rough dimension, as the gutta-percha is not put on with such regularity as to ensure a perfectly uniform diameter, or even a truly cylindrical section.

The experiments to be first described were made simultaneously on these two coils, and at a subsequent period tests were made on another coil, which will be called No. 3. This coil formed part of the core of the Red Sea cable, and was prepared under Chatterton's patent. Like No. 1, it consisted of two coatings of gutta-percha and two layers of varnish or compound, in the order above described. Coil No. 3 was, like 1 and 2, 2,028 yards, or one knot in length. The metal conductor was a strand of seven copper wires, each about 0·038 in. in diameter, and weighing altogether 180 lbs. The diameter of the circle circumscribing the strand was 0·105 in. The total diameter of the gutta-percha was 0·34 in., and the weight 212 lbs.

A section of the covered wire forming each of these three coils is given in Fig. 2.

Experiments on the insulation of short lengths of gutta percha covered wire at various temperatures.

These coils were tested under pressure and vacuum in the following manner:—In order to obviate as much as possible the danger of any flaw in the covering, such as might arise from a concealed air bubble or small puncture, the coils were placed in a strong cast-iron tank nearly full of water, and the ends of the insulated wires brought outside the tank through small stuffing-boxes provided for this purpose. The lid of the tank having been replaced, water was pumped out of the tank until a vacuum of about 15 in. of mercury was produced. Water was then pumped into the tank by an hydraulic ram until a pressure of 600 lbs. per square inch was reached. At this pressure one pole of a battery was connected with one end of the coil to be tested, the other end of the coil being kept free or insulated, whilst the second pole of the battery was put in communication with earth. A galvanometer of great delicacy, which will be presently described, was introduced in the circuit between the battery and the coil, so that any current passing from the inner to the outer surface of the gutta-percha, either by a flaw or by reason of the conducting power of that substance, would be shown by this galvanometer.

A sketch of the connexion described is shown in Fig. 1. A similar arrangement is frequently spoken of in the course of the experiments, and will be called the connexion for the ordinary insulation test.

The indications of the galvanometer were in each case satisfactory, the deflection not being greater than that expected from daily experience of similar coils.

After being removed from the pressure tank, the coils No. 1 and 2 were placed in a large wooden tub of about 3 ft. diameter, and 3 ft. 6 in. deep. This tub was carefully felted and provided with a lid, also covered with felt; the bottom of the tub alone was not felted. The place on which the tub stood was kept moist by a slight leakage from the tub, which was thus put in good connexion with the earth.

Coil No. 1 rested on the bottom of the tub, and coil No. 2 rested on coil No. 1. Water was poured into the tub until the surface of the water was several inches above the highest portion of this coil. By the word coil a sort of loose hank is meant, formed by winding the gutta-percha loosely on a centre, which is removed as soon as the coil or hank is complete. Three or four light gutta-percha thongs prevent the hank from falling into confusion, and facilitate its removal at will. The connexions for the ordinary insulation test were then made, as shown in Fig. 3, and the first careful experiment made on the 31st of August.

A battery of 72 pairs of plates arranged on Daniell's system was employed. The copper and zinc plates both measured 4½ in. × 3½ in., but these plates were only partly covered by the solutions. The plates were separated by a flat porous diaphragm ¼ in. thick, which unfortunately offered considerable resistance to the passage of the electric current. The porous diaphragms were secured by an

insulating mastic to the sides and bottom of a glass trough, divided transversely by webs, which, with the porous diaphragms, made compartments which held the two fluids and plates. This arrangement of battery is shown in Fig. 4, and is patented by Mr. Reid of London.

Gutta-percha covered wires were brought from the poles of this battery to the two terminals b and d, of a circular commutator A (Fig. 3), made by Messrs. Siemens and Halske, the details of which are shown in Fig. 5; one of the two remaining terminals a of the commutator was connected with earth by means of a copper plate E, buried in moist earth, and the other c was connected to one terminal e, of a small brass junction piece F, drawn half-size in Fig. 6, the two halves of which are screwed on to a circle of vulcanite, and are each provided with two terminals. When the conical brass plug P is in the hole i, a current can pass directly with very small resistance from e to f; when the plug P is withdrawn, the current is obliged to traverse the wire connecting h and g, and consequently the galvanometer G.

When the resistance of the wires joining C F is considerable, as must always be the case where delicate galvanometers are employed, no sensible portion of the current passes through the galvanometer when the plug P is in its place.

A piece of gutta-percha covered wire attached to the fourth terminal f of the circular junction piece F could be connected at will by means of an ordinary binding screw B, with *one* end, n, o, p, or q, of either coil in the tub; the binding screw was insulated by being allowed to hang free in the air; the batteries, galvanometers, junction pieces, and connecting wires were all carefully insulated by means of gutta-percha or vulcanite.

The sine galvanometer employed (Plate I.) was made by Messrs. Siemens and Halske of Berlin. In this instrument an astatic couple of magnets, made with extreme care, are suspended by means of a single fibre of unspun silk, a, b, six inches and seven-eighths long, from a brass fork b, fixed accurately over the centre of a divided circle c, which rests on a wooden frame d d, containing the coil. The brass fork is carried by a cross bar resting on the two standards I and H, and can without being turned round be lowered or raised by means of a nut at A, and a concealed screw. The length of each needle is $1\frac{7}{16}$ in., the weight of the astatic couple including the brass connexion is 4 grains, the internal diameter of the divided circle is $1\frac{7}{16}$ in. The coil consists of 20,188 turns of copper wire of gauge, and is composed of two equal and similar parts, the four ends of which are brought to the insulated terminals j, k, l, m.

By means of these the coil can be used as one continuous wire, or as two similar wires connected, so that each shall convey half the current in the same direction, or thirdly, as two similar wires, each conveying half the current in an opposite direction. When the first connexion was used, the coil will be described as connected in series; when the second connexion was used, as in double arc.

The third connexion forms a differential arrangement.

The coils, needles, thread and standards are protected by a cylindrical glass case fastened to a brass disc, on which the frame containing the coils and the standards I and H are secured.

The rim B of this disc projects beyond the framing which receives the glass case, and is divided into 360 parts; the external diameter of this divided circle is $8\frac{1}{4}$ in. An upright standard L, K, is screwed to the bottom of the disc, and supports a small prismatic lunette M, which is fixed in the plane determined by the line of the suspending fibre and the zero of the divided circle; by means of this lunette the slightest angular deviation of the needle from the zero point can be observed, the position of the needle at other points of the circle near the zero point can be observed with a very small parallax through the lunette by turning this latter on its axis in the clip Q, and elevating or depressing one end by means of the joint L.

A pin and socket in the centre of the tripod R, R support the divided disc, which can thus be made to rotate at will, together with the coil, lunette, &c., on an axis coincident with the line of the suspending thread. A small catch, not shown in the plate, throws the pin into gear with a tangent screw, of which the milled head appears at T. This screw permits a very delicate angular adjustment to be made, and prevents any accidental disturbance of the disc.

A vernier V fixed to the tripod allows the measurement of the angular motion of the disc to be made to the tenth of a degree. Finally, the whole instrument can be levelled by means of the screws S, S.

The resistance of the coils connected in series is equal to 1889×10^8 Thomson's absolute British units. The resistance was measured by Professor Wheatstone's differential arrangement. If the connexions shewn in Fig. 3 are now examined, it will appear that one pole of the battery will always remain connected with the earth, whilst the other pole will be in connexion with the junction piece F, and with the coil to be tested, whenever the connexion at n, o, p, or q, is completed by means of the binding screw B. By the handle K of the commutator A, the poles of the battery so connected can be reversed at will.

So long as the plug P connects the two halves of the junction piece, the galvanometer will not be affected, but so soon as the plug P is withdrawn, this instrument will indicate the passage of any current which may exist, in consequence of any connexion between the earth and the terminal m. It is evident that any such connexion would complete the electrical circuit between the two poles of the battery; for instance, should the further end or any portion of the coil be connected on purpose or by accident with the ground, a powerful current almost undiminished by resistance would pass through the galvanometer. The same may be said were the terminals f, g, or the binding screw B, in connexion with earth. When care is taken to insulate properly all these parts, any current observed can only be due to a connexion between the wire of the coil and the water in the tub, through or by means of the gutta-percha covering. The intensity of this current can be measured by means of the sine galvanometer, the sines of the deflections under various circumstances being inversely proportional to the resistances of the circuits so long as the battery power remains constant.

It was not, however, found possible in practice to insulate the various connexions so perfectly that no current should appear, even when the coil in the tub was entirely disconnected. The evaporation from the surface of the various exposed bright brass surfaces, and the slight film of hygrometric moisture covering the same parts, invariably formed a connexion of greater or less resistance between the terminal m and the earth. In calculating the resistance of the gutta-percha from the observed deflections a correction is thus rendered necessary, and for this purpose the loss due to connexions, as it may be termed, was carefully ascertained at the beginning and end of every experiment.

Moreover, although much care was taken to ensure the constancy of the batteries, their electro-motive force and resistance underwent considerable change from changes of temperature and other causes. A second correction to meet this variation is therefore necessary before observations made on different days or at different times can be compared.

The batteries employed at various times were compared in the following manner:—A circuit (Fig. 7) was arranged, including a tangent galvanometer, a set of resistance coils, and the battery; two or more deflections were taken with various resistances added by means of the coils—then let T = tangent of angle of deflection when a resistance R is added to the circuit, and t, the tangent of the angle of deflection when a resistance r is added to the circuit; let the resistance of the remaining portion of the circuit, *i. e.*, of the battery galvanometer and connecting wires, be called x; then—

$$\mathrm{T} : t = x + r : x + \mathrm{R}, \text{ and therefore } x = \frac{\mathrm{T\,R} - t\,r}{t - \mathrm{T}},$$

$$\text{when } r = o, \text{ then } x = \frac{\mathrm{T\,R}}{t - \mathrm{T}};$$

the resistance of the entire circuit in the two cases is $x + \mathrm{R}$ and $x + r$ respectively. Now the electro-motive force of the battery where the resistance is constant is proportional to the tangent of the deflection, and where the deflection is constant this force is proportional to the resistance.

The electro-motive force can therefore be measured or represented by the product of the resistance into the tangent of the deflection—the unity of measurement being in such case the battery which, with a circuit the resistance of which is unity, would produce a deflection of 45° on the galvanometer employed.

This method of comparing the electro-motive force of batteries leaves little to be desired when the observations are made with care and always on the same instrument; the measurements can be reduced into any generally received unit by the use of a simple co-efficient.

The electro-motive force of any battery with which a certain deflection was obtained being once known, the deflection which would have resulted from any other electro-motive force is given by a simple proportion. Thus if with an E M F, = 90, a deflection of b degrees is obtained on a sine galvanometer, then with an E M F = 100, we should have observed a deflection x, which may be found by the equation $\sin. x = \sin. b \frac{100}{90}$. The deflections obtained were in this manner reduced to those which would have resulted from a constant battery.

It was observed in the ordinary insulation test that the deflection rapidly decreased for some minutes after the first application of the battery, but that the original de-

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flection was again obtained by reversing the poles of the battery in connexion with the earth and the cable. The rapid and gradual decrease of deflection was always observed, although care was taken to prevent, by means of the plug P, the first rush of the current into the cable from passing through the galvanometer. No test of insulation is complete unless the time is recorded which elapses between the first connexion of the cable with the battery and the observation. The state of the cable previous to the admission of the current should also be noted, namely, whether the wire had been previously charged with either the opposite or the same electricity, and if so for how long. In consequence of this observation the author resolved to note the deflection at fixed periods of time after the first connexion had been made between the cable and the battery.

The experiments were conducted in the following manner:—The connexions being arranged as in Fig. 3, the coil to be tested was not at first connected to the binding screw B; this screw was left free or insulated, the plug P being in the junction piece F. The number was observed opposite which the vernier V of the sine galvanometer stood when the needle was exactly opposite zero point of the inner divided circle; this number will be found noted as the zero point in the left hand upper corner of Tables I. to XIX., which contain the results of the observations.

This number varied slightly from day to day, principally, the author believes, owing to the effect of the atmosphere on the torsion of the suspending thread, as great care was taken to remove every other disturbing force. After the zero point had been observed, the plug P was withdrawn from the junction piece, a slight deflection ensued, due to the loss from the connexions as before described, by means of the tangent screw; the divided disc, with coils, &c., was moved until the zero of the inner divided circle again exactly coincided with the point of the needle, and the number opposite the vernier was again observed. This number appears near the foot of the tables, and is called zinc or copper to connexion at beginning of experiment, according as the zinc or copper pole of the battery was at the time directly connected with the galvanometer through the commutator. As soon as this deflection had been observed, the plug P was replaced, the ends of the connecting wires at *i* and *m* reversed in those terminals; the commutator handle was also reversed and the plug P then withdrawn. The opposite pole of the battery was thus connected with the galvanometer, but the deflection of the needle was still to the same side as in the previous experiment. This deflection was again taken by means of the vernier, and appears next the previous number near the bottom of the second column.

The plug P was again replaced, and the coil to be tested connected with the binding screw B; the ends of the connecting wires were again reversed in the terminals *j* and *m*. At a given instant observed by the seconds hand of a watch, the commutator handle was reversed, and the plug P immediately afterwards withdrawn. The needle was now much more forcibly deflected; the divided disc and coils were, as before, moved round until the zero of the inner circle coincided with the point of the suspended needle. The deflection of the needle, however, was by no means permanent, but, after the first oscillations due to momentum had ceased, decreased rapidly. In order, therefore, to keep the needle point and the zero on the card opposite one another, it was necessary continually, by means of the tangent screws, to follow the needle with the coils. The deflections were read by observing the numbers opposite the vernier after intervals of one minute, two, three, four, and five minutes from the time of first reversing the current and withdrawing the plug P.

On the conclusion of this set of experiments, the plug P was replaced, the commutator handle reversed, and similar observations made with the opposite current.

The numbers observed will be found in the second column of the tables, and are called deflection after first minute, after second minute, &c., with zinc or copper to coil, as the case may have been. In these observations the author was aided by Mr. William Picken, who also rendered valuable assistance throughout the whole course of experiments.

The battery was then connected with resistance coils and a large tangent galvanometer, as shown in Fig. 7, and two deflections were taken, which are entered in the tables, and called S.C. + 30 and S.C. + 59·15, and occasionally S.C. + 82·27, S.C. being used as an abbreviation for the resistance of the short circuit, which term must be understood to include the galvanometer, batteries, and connecting wires.

30, 59·15, or 88·27 were the number of units respectively added to S.C. when the several corresponding deflections were taken. These units were those of the only resistance coils at my disposal, and were each equal to about $18{,}568 \times 10^4$, Thomson's British units.

The temperature of the water in the tub was taken at the beginning and end of each set of experiments, and will be found at the upper right hand corner of the tables. This temperature remained very constant during each experiment, owing to the careful felting of the tub. The temperature of the water was raised by the addition of warm water, or cooled by the addition of cold water, or, when necessary, of ice. Twelve or fourteen hours usually elapsed between the addition of such a fresh body of water or ice and the beginning of the experiments. In no case was this interval less than seven hours. By this means time was allowed for the gutta-percha to assume the same temperature as that of the surrounding water.

It was, however, found difficult to keep the water in the tub at a uniform temperature throughout, the water at the bottom being invariably cooler than that at the top of the tub. Several experiments were rendered useless, owing to this fact not having been at first observed. Care was subsequently taken occasionally to stir the water, and to note the temperature both at the top and the bottom of the tub, when any marked difference was observed. Still the author believes that the observations of the temperature form the least satisfactory portion of the experiments. He thinks, however, that the curves resulting from the mass of experiments correct in a great measure the errors due to this source.

The first eleven tables give the results of eleven sets of observations on coils Nos. 1 and 2.

The first and second columns have already been explained; the third column contains the difference between the number noted as zero and the numbers recorded in the second column. The third column, therefore, records the actual deflections of the needle in degrees and decimals. The fourth column contains these deflections in degrees and minutes. The fifth column contains the sines of these angles. The sixth column contains the sines of the angles corrected for the loss due to connexions, and is obtained by subtracting the mean of the sines of the angles of deflection due to the loss on connexions with zinc to connexions from the sines in the sixth column opposite zinc to coil, and the mean of the sines due to loss on connexions with copper to connexions, from the sines in the sixth column opposite copper to coil.

At the foot of the table on the right-hand side are the calculated resistances of S.C. and the electro-motive force of the battery, as before defined. The numbers in the seventh column are obtained by multiplying the numbers in the sixth by 100, and dividing by the electro-motive force.

This seventh column contains, therefore, the sines of the angles which would have been observed had there been no loss on the connexions, and had the battery power been constantly 100. These numbers are inversely proportional to the resistance of the circuit in each case, and since the resistance of the gutta-percha was in all cases extremely great compared with that of the rest of the circuit, these numbers may be taken as inversely proportional in each case to the resistance of the gutta-percha coating.

The several columns in the tables are given for the purpose of showing in each case the nature and extent of the corrections applied.

Tables 12 to 18 give the results of experiments made subsequently in the same manner on coil No. 3. The curves resulting from the experiments on these three coils are shown in Plates II., III., IV., V., and VI. The ordinates at the various temperatures correspond to the numbers in the seventh columns of the tables.

On examination of Fig. 1, Plate II., it will be seen that for coil No. 1, with a negative current, or zinc to coil between the limits of 50° and 80° Fahrenheit, two straight and parallel lines very nearly include between them the entire series of crosses which show the height of the ordinate, representing the loss at the various temperatures. Between these limits, therefore, we see that the decrease of resistance is sensibly constant for equal increments of temperature, and that the increase of resistance due to continued electrification is also nearly constant at all temperatures between 50° and 80°.

At 60° the resistance from this cause increases about 20 per cent. in five minutes.

On examining the relative position of the crosses in each group, it will be seen that the increase of resistance due to each minute cannot be very accurately determined from the experiments, but this increase obviously diminishes during each successive minute. On examination of Plates II., III., IV., V., and VI., it would appear that this increase of resistance during any minute is, in most cases, about one-half the increase during the preceding minute.

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It was not possible very accurately to observe the needle during the rapid diminution of deflection which occurred in the first minute after connexion with the battery; in many cases the resistance seemed of an unsteady nature, causing short jerks of the needle, and this effect sometimes continued even during the third, fourth, and fifth minute. The author is uncertain how much of this unsteadiness is due to the varying resistance of the gutta-percha, and how much to momentary inconstancy of the battery. It is certain that this appearance is much increased when the batteries employed have been long in use, or are from any cause in bad order.

Returning to the consideration of coil 1, Plate I., we observe that in Fig. 2, which represents the variation of resistance with a positive current or copper to coil, two straight parallel lines, similar to those of Fig. 1, bound the groups of crosses between the temperatures of 50° and 60°. These lines are, however, nearer the base than in Fig. 1, showing that the coating offers a greater resistance in this than in the previous case; the amount of the increase of resistance due to the continued connexion with the battery seems unchanged by a change in the pole connected with the wire. Above the temperature of 63° great irregularities may be observed in the position of the crosses, the resistance of the gutta-percha appearing to rise and fall in a very uncertain manner. No attempt has been made to include these observations in a curve. Possibly these irregularities may be due to defective observations, but the author would observe that, to remove the irregularities, it would be necessary to reject at least three of the groups, or, in other words, to assume that 15 observations were incorrect, whereas not one observation has been rejected in order to reduce irregularities in the appearance of any of the other curves. Moreover, the irregularity was so marked even during the observation, that every effort was made in each case to discover any possible cause of error. All the observations shown in Plates II., III. and IV., were made with the same connexions and at the same dates. The author, therefore, cannot attach less value to one series than to another; and he is of opinion that a very marked irregularity exists in the resistance opposed by the coating of No. 1 coil to the passage of a current when the interior of the coating was positively electrified. The presence of two different materials, the resinous compound and the pure gutta-percha, may account for this peculiarity.

The difference in the resistance of the coating when the copper was positively or negatively electrified, may be caused by the contact between the resinous compound and the copper; no such difference having been remarked when the gutta-percha was in contact with the copper.

On comparing the observations on coil 1 with those on coil 2, consisting of pure gutta-percha, as shown in Plates III. and IV., we at once perceive that the curves in these Plates present an entirely different character from the lines in Plate II.

In Plates III. and IV. the curves bounding the groups of observations approach nearer to the base line at low temperatures, and recede much further from that base line at high temperatures than the lines in Plate I. The increment of resistance is no longer constant for equal increments of temperatures, but is much greater at the high than at the low temperatures. The increase of resistance due to continued connexion with the battery is less in coil 2 than in coil 1, and increases slightly at the higher temperatures. This increase is not affected by a change in the pole of the battery; the identity of the curves in Plates II. and III. between the temperatures of 50° and 75° is remarkable.

The author believes it has been a generally received opinion that there exists a marked difference between the insulation even of a sound cable when the conducting wire is positively electrified, and the insulation of the same cable when that wire is negatively electrified. When the cable is coated with pure gutta-percha this does not seem to be the case. In such a cable, therefore, want of symmetry in the tests would indicate a fault.

The curve in Plate III., zinc to coil, rises somewhat higher than that in Plate IV., showing apparently that at high temperatures the resistance is affected by a change in the sign of the current.

The ordinates of the curves in Plates II., III., and IV., may be directly compared, that is to say, they are in all cases directly proportional to the loss through the two coverings at the various temperatures, and since the diameter and length of the wire in the two coils were equal, these ordinates give the value of the two materials as insulating covers under the several conditions. And here a result of great practical importance appears, viz., that although the wire coated according to Chatterton's patent (No. 1) is more perfectly insulated at high temperatures, coil No. 2 covered with pure gutta-percha is better insulated at low temperatures.

In the particular case before us the insulating qualities of the two coverings are equal with zinc to coil after the first minute at about 65°, and with copper to coil after the first minute at about 63°. After five minutes of continued electrification, the point of equality is about 1° lower. Wherever, therefore, the bottom of the sea is below 60°, a cable coated with pure gutta-percha will give decidedly better results as regards insulation than a cable covered with the compound. There are, however, several considerations to be urged in favour of the compound at all temperatures which may outweigh this defect. Among these may be named the adhesion produced between the copper and the covering, and the probability that any small aperture in the first coating of gutta-percha would be filled up in putting on the compound. It must also be borne in mind that the results obtained may possibly be due to some peculiarity in the particular coil under observation, and may not be confirmed by more extended researches. However this may be, it is very remarkable that so small a quantity of varnish as is employed, should so materially alter the insulating qualities of the coating.

The curves derived from coil 3 (Plates V. and VI.) appear in some respects intermediate between those derived from coils 1 and 2. The diminution of resistance at the higher temperatures is greater than in the former, and less than in the latter coil. This was to be expected from the fact that the compound in this coil forms a still smaller proportion of the entire coating than in coil 1. The ordinates of the curves shown in Plates V. and VI., compared with those of the curves in Plates II., III., and IV., do not directly afford the relative specific resistance of the covering of No. 3 coil, since the dimensions of the gutta-percha and wire are not the same in this coil as in the others. In the second part of this paper, in treating of the absolute resistance, a comparison will be made easy between the three curves, by the formation of tables of the specific resistance of each material at each temperature. The extra resistance due to the continued electrification of the copper wire is evidently of still greater magnitude in coil No. 3 than in the other coils. Indeed, 40 per cent. of the entire resistance is, at a temperature of 70°, due to this cause. This increase is probably due to the greater body or mass of gutta-percha surrounding the wire.

Comparing Plates V. and VI., we perceive a difference between the curves given by the wire when positively and negatively electrified; and here, as in coil No. 1, the varnish, not the gutta-percha, is in contact with the electrified wire. The author is, however, of opinion that had the observations been more numerous less difference would have appeared in the upper curve. As in coil No. 1, the curve obtained from negative internal electrification is somewhat the higher. The curves bounding the observations made five minutes after connexion with the battery are extremely similar. The extra resistance, as I have named it, increases rapidly in amount as the temperature increases, instead of remaining nearly constant, as in coils 1 and 2. The author regrets that a greater number of experiments were not made on this coil, especially between the temperatures of 70° and 60°, since the form of the curve between these points is almost conjectural. The observations recorded in Tables 12, 13, and 14, at the temperatures 49°, 52°, and 53° 50′, were unavoidably made in the absence of the author, and, owing to the very great loss on the connexions, the author is unable to place much confidence in the results, which, in the zinc curve, seem very anomalous.

In damp weather it was impossible to prevent this loss forming a very appreciable but somewhat uncertain quantity; continued connexion with the battery, or, in other words, electrification, appeared to have no influence on the amount of loss. It is very remarkable that in almost every case the loss from connexions is considerably greater when the wire and instruments were connected to the negative than when they were connected to the positive pole of the battery. The insulation of the galvanometer coils from the rest of the instrument was necessarily imperfect, and probably the chief loss arose from the moisture collected on the surface of this instrument; even when standing on gutta-percha it was necessary to use a sort of gutta-percha glove, when touching the tangent screw, to avoid error due to the escape from the surface of the body of the experimentor.

The following is a summary of the results obtained by the experiments described in this part of the paper.

The relative resistances at various temperatures of three different insulating coverings have been fixed.

The curves deduced from the experiments on pure gutta-percha are regular, and probably accurate: those showing the results obtained from a mixed covering are not so well defined. The general conclusions as to this covering may, however, probably be depended on.

The extra resistance due to continued electrification has

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been fully described, as the author believes, for the first time.

This phenomenon has great influence on all tests of insulation.

Finally, the relative value of the extra resistance and of the total resistance for each material and at each temperature has been determined.

Experiments on the insulation of short

Part II.

In the first part of this paper no attempt has been made to measure the resistance of gutta-percha by a comparison with the resistance of copper or with the resistance of any known unit, nor could the experiments there described lead to any absolute measurement. Assuming that the loss would, *ceteris paribus*, be proportioned to the length, the insulation of various lengths of any one cable at various temperatures might indeed be compared by reference to the results and curves there described, but further experiment and mathematical investigation are required before we can calculate the loss which may be expected through the covering of a cable different in size and proportions from any which has formed the subject of experiment.

Before we can do this it is necessary first to know the resistance opposed to the passage of a current of electricity by some definite quantity of the insulating coating, which may be taken as unity, and secondly to know the law connecting the resistance of that unit with the resistance of the actual cylindrical covering of a cable.

Professor Thomson has already supplied an equation expressing this law. Let S be the specific resistance of the material, say gutta-percha, used in covering wire, that is to say, the resistance of unity or of a bar one foot long and with a section of one square foot; let G be the actual resistance of the cylindrical covering of a length L measured in feet; let $\frac{a}{b}$ be the ratio of the external to the internal diameter of the cylindrical covering; then $S = \frac{2 \pi L G}{\log. \frac{a}{b}}$ (equation 1).

When the internal conductor is formed by a strand instead of by a single cylindrical wire, the internal diameter of the gutta-percha covering must be taken as a little less than that of the circle circumscribing the section of the conductor.

The author has endeavoured by the aid of this formula to determine the specific resistance of gutta-percha from tests made upon the Red Sea cable while in process of manufacture at the works of Messrs. R. S. Newall and Co., at Birkenhead.

A short description is necessary of the disposition of the cable and of the conditions to which each part was subjected during the manufacture, and when coiled in the warehouse previous to shipment.

Eight machines were employed in covering the gutta-percha with wire, the finished cable from these machines formed eight separate coils, four of which (called Nos. 1, 2, 3, 4) were contained in a building called the shed, and the remaining four numbers, 5, 6, 7, 8, were contained in a building known as $\frac{C}{5}$.

All these coils rested immediately on a brick foundation; they were surrounded by a cylinder of wrought iron intended to facilitate the formation and ensure the stability of the coil. None of these coils were under water at any time.

The shed was invariably at a temperature considerably higher than that of the warehouse, owing to the presence of the boiler in the former building, and to the immediate exposure of its roof to the action of the sun.

The absence of windows in the warehouse and the protection of the upper story kept the coils in this building at a temperature nearly as constant as that of a cellar. The temperature in the shed varied considerably during the 24 hours, and was far from equal even at the same moment in different parts of the building. In the shed therefore the temperature shown by a thermometer hanging against the side of any coil gave little indication of the temperature of the mass composing the coil. To meet this difficulty, long wrought-iron gas tubes were inserted half way up the coils, reaching from the circumference about six feet towards the centre of the coil. Thermometers were placed in these tubes, and the temperature shown by these was taken as the temperature of the coil. In the warehouse little or no difference was ever observed between the temperature inside and outside the tubes, but in the shed this difference sometimes reached 10° Fahrenheit. Unfortunately this precaution was not adopted until August 8th, late in the manufacture of the cable. All experiments on cables in the shed previously to this date are consequently of little value.

By far the greater part of the cable tested in each instance was covered with wire, and lay in the coils described. In each case, however, a small length, never exceeding two knots, was formed of gutta-percha covered wire, served with yarn only, and wound on a drum in another part of the works.

This portion formed the core about to be covered with wire, and was therefore unavoidably included in the list, although not at the temperature of the coil; and it is moreover probable that the tarred yarn and wooden drum may have formed an imperfect connexion between the outer surface of the gutta-percha and the earth. No allowance has been made for the different conditions to which this short length was subject, but the error arising from this source must be extremely small. A much graver objection might be founded on the fact that the manufactured cable was not under water during the test. Luckily, experiments have been made which prove that the loss from a sound wire-covered cable is hardly, if at all, increased by submersion, the temperature of the gutta-percha remaining constant. The experiments on which this opinion is grounded were made on June 20th and 25th.

On June 20th the cable was perfectly dry, and coiled in the usual iron wells. On June 25th the same cable was tested under water in the same wells. Water had been kept in the wells for more than 48 hours previously. The following table contains the remaining particulars of the experiment:—

Table XX.

	June 20.—Cable Dry.		June 25.—Cable Wet.	
	Angles.	Tangents.	Angles.	Tangents.
B R + 30	55	1·428	58°	1·600
B R + 59·15	43	·933	45·30	1·018
Loss on 226 knots	31½	·613	28	0·532
Loss on 696 knots	61°	1·804	59°	1·664
E. M. F.	78·45		81·6	
Temperatures	68½		62½	
Ordinate of temperature	1380		1080	

The ordinate representing the loss after the fifth minute with copper to coil at 62½, taken from Plate VI., is 1080; the ordinate corresponding to 68½ is 1380. The ordinates are taken from the lower curve, because at this date the testing current was seldom or never reversed, and in this case, as will afterwards be shown, the loss is nearly that due to five minutes continued electrification. By a direct proportion, the loss to be expected on June 25th may be calculated from the loss on June 20, provided the addition of the water has made no change in the insulation of the cable. The equation stands thus for the loss on 696 knots,

$$\text{tang. } x = \frac{1{\cdot}804 \times 1080 \times 81{\cdot}6}{1380 \times 78{\cdot}45}$$ (the resistance of the two circuits being supposed to be that of the gutta-percha alone) from the above tang. $x = 1{\cdot}468 =$ tang. 55·45. The equation for the loss on 226 knots—

$$\text{tang. } x = \frac{{\cdot}613 \times 1080 \times 81{\cdot}6}{1380 \times 78{\cdot}45} = 26° 30'.$$

The deflections actually observed were 59° and 28°, but we must here remark that the temperature, 68½, was the temperature of the shed in which the coil stood, and therefore probably 2 or 3 degrees higher than that of the coil. Thus, on the 17th September, the temperature of the shed was 69½°; that of the cables taken in the tubes previously described, 65° and 63°; on the 19th the shed was at 67½°, and the coils were at 64° and 62°, and a similar difference was invariably observed. A more satisfactory comparison between the tests of June 20th and 25th will, the author thinks, be given by calculating the temperature ordinate for the 20th June from the electro-motive force on the two days and the loss and temperature ordinate for the 25th June, on which day the temperature of the water covering the coils was directly measured. The ordinate required, calculated from the loss on 696 knots,

$$\frac{1{\cdot}804 \times 81{\cdot}6 \times 1080}{78{\cdot}45 \times 1{,}664} = 1319 = \text{ordinate } 66°.$$

Calculated from the loss on 226 knots,

$$\frac{{\cdot}613 \times 81{\cdot}6 \times 1080}{78{\cdot}45 \times {\cdot}532} = 1298 = \text{ordinate } 67\tfrac{1}{4}°;$$

that is to say, after due reduction for the difference of electro-motive force on the two days, the numbers expressing the loss are proportional to the ordinates from Plate VI., corresponding to 62½° and 66° or 67°. Since it is almost certain

that 66° or 67° was more nearly the temperature of the coil than the 68½° recorded, the author is of opinion that the resistance of gutta-percha may be safely calculated from the loss through a wire-covered cable contained in a dry metal tank. When the cable is not covered with wire it is no longer safe to calculate the resistance in this manner, for the author found that the loss from a hemp-covered cable gradually became larger as such cable was covered, and had increased about 70 per cent. by the time the whole cable was covered. This phenomenon was observed in three separate coils of about 70 knots each.

The author, however, does not, from the above experiments, conclude that the tests of a faulty wire-covered cable would be similar when dry and when under water. On the contrary, he is very strongly of opinion that no fault, unless of extraordinary magnitude, can be or will be detected unless the cable is under water, and in a coiled cable a fault might escape detection unless the coil were covered by water for a considerable time.

To return to the description of the experiments. The coils in the shed were distinguished as Nos. 1, 2, 3, and 4, of which No. 1 was invariably at a slightly higher temperature than the other coils. The coils in the warehouse were numbered 5, 6, 7, and 8, and were found to be usually at equal temperatures. Some of the most important experiments were made after the cables had been coiled into the two ships. They were coiled on board into iron wells, and the temperatures of these coils were taken by tubes inserted between the layers. No difference, therefore, except that due to temperature and electro-motive force, may be expected in the tests on board and on shore. The following description applies equally to all the coils :—

Each end of each coil was connected with a terminal *a*, *b*, (Fig. 9) in the testing office, by means of short pieces of wire-covered cables. These terminals formed part of the junction bar A, B ; by means of conical metal plugs fitting the holes 1, 2, 3, 4, &c., in the junction bar, the two ends of any one of the cables could at will be put in connexion with the end terminals A and B of the bar, or by the same plugs the cables could be joined in series, and the two extreme ends of the series be put in connexion with the terminals A and B. The terminal A could, by the circular junction piece O, be connected with earth E, or with the terminal of a commutator C, with which the two poles of the battery of 72 cells already described were directly connected. The terminal B was connected with one end of the resistance coils R, already mentioned, and the other end of the resistance coils was connected with two small circular junction pieces H, F, similar to that shown in Fig. 6. From the further terminal *d* of the second junction piece F, a connexion was carried to the remaining terminal of the commutator C. Either of the galvanometers T or T 2 could be included in the circuit by withdrawing the plugs F or H.

Three sets of tests were made daily, viz., the continuity tests, the insulation tests, and the battery tests. The continuity test was made as follows :—

The plugs in the junction bar A B were so arranged that the arc of the circuit from A to B was completed only by the cable to be tested ; thus if cable A B was to be tested plugs were placed in the holes 2, 3, 4, 5, 6, 7, 8, 9, and 10, and all other plugs withdrawn. The plug of junction-piece O being at I, and the index of the resistance coils at zero, the plug at F was withdrawn : the cable to be tested, and the tangent galvanometer then formed with the other connexions a complete metallic arc between the poles of the battery. The deflection on T was observed, and the angle noted as the continuity test of the cable included in the arc. The poles of the battery could be reversed at will by means of the commutator, but usually one deflection only was taken for each cable, with copper to that end of the cable next the galvanometer. The plug at F was then replaced, the plug at O moved to position II., and the plug at H withdrawn.

One pole of the battery was thus put to earth, and the strength of any current passing from the other pole to earth, through the covering of the cable, was shown by the tangent galvanometer T 2. The angle observed was entered as the insulation test of the cable. Usually this test was made with the positive pole of the battery in connexion with the cable.

In the final tests, from which the specific resistance of the covering has been chiefly determined, tests were made with both poles of the battery, and after 1, 2, 3, 4, 5 minutes continued electrification.

The third test was made by connecting A and B directly with plugs in the holes 1, 2, 3, 4, 5, 6, 7, and 8, replacing the plug at H and the plug of O to position I. The index of the resistance coil was then placed successively at 30, 60, and 90 units, and the several corresponding deflections observed on T 2 (the plug at F having been removed). This last test enabled the resistance and electro-motive force of the battery to be measured, as was described in the first part of this paper.

In order to make accurate deductions from a comparison of the relative resistance of the circuit formed during the continuity test with that made use of for the insulation test, as measured by the two galvanometers, the following values were required :—

1st. The resistance of the battery. This was on each occasion calculated from the battery tests in the manner described.

2nd. The resistance of galvanometer T 1.

3rd. The resistance of galvanometer T 2.

4th. The resistance of the copper wire of the cable.

5th. The change of resistance caused in copper wire by a change of temperature.

6th. The comparative delicacy of the two galvanometers.

The author was compelled to use for the measurement of resistance some very imperfect resistance coils, the unit of which was intended to represent the resistance of a German mile of iron land wire one-sixth in. diameter. The imperfection of the relative value of each coil was rectified by measurement with Wheatstone's differential arrangement ; thus 60 was found in reality to be of the value 59·15, and 90 of the value of 88·27. The accuracy of the measurements was not therefore affected by the imperfection of the coils. In most instances, however, in this paper the value of each resistance will be given in Professor Thomson's absolute British units, to which they have been reduced by a simple coefficient subsequently determined.

In the case of the electro-motive force only, it has been thought unnecessary to make the reduction, since the unit employed to express the E. M. F. is itself completely arbitrary.

The measurement of resistance of the copper wire used, either in the cable or in the galvanometer coils, was in all cases made by the arrangement known as Wheatstone's differential arrangement (Fig. 10). B is a small battery, the resistance to be measured being placed between A and D, and the measuring unit between D and E. A C E was in practice a straight copper wire, one metre long, stretched close to a scale, by which the position of the moveable point C on the wire could be determined with nicety, the wire A C E being supposed uniform $\frac{\text{Res. A D}}{\text{Res. D E}} = \frac{\text{A C}}{\text{E C}}$ when the index of the galvanometers remains at zero. By means of a special contrivance, the ends of the measuring wire A C E, in connexion with the points A E, could be reversed, and a second observation taken, in which the point C was necessarily placed on the opposite side of the centre of the wire A C E to that at which it had stood in the first observation. Any slight irregularity in the resistance of the wire was then compensated for by taking a mean between the two observations of the distance of C from the centre of the wire to be the distance of that point from the centre, dividing the measuring wire in the ratio of the two resistances A D and D E.

The instrument employed was manufactured by Messrs. Siemens and Halske, and was of very perfect workmanship. The galvanometer used was shown in Plate I. The coils were connected in double arc. By the above means the following values were determined :—

Resistance of the coil of T 1 = 622 × 10^7 T. U.
Resistance of the coil at T 2 = 148 × 10^9 T. U.
Resistance of one knot of the Red Sea copper,
= 250 × 10^6 T. U.
Resistance of German mile land wire,
= 185,658 × 10^4 T. U.

In order to determine the change of resistance produced in the copper wire by a change of temperature, special experiments had been made by the author with the differential arrangement in March 1859, on the occasion of some preliminary experiments on the change of resistance of gutta-percha.

Two knots of Alexandria and Candia insulated wire were immersed in water at various temperatures, and the resistance of the copper wire measured at each temperature by the differential arrangement. By leaving the coil for many hours in the water, care was taken that the copper should be at the same temperature as the water. The experiments and their result are contained in Table No. XXI. and in Plate VIII.

The increment of resistance due to equal increments of temperature is apparently constant between the temperatures 50° and 90° Fahrenheit. The equation $R = (1 + 0{\cdot}00192\,t), r$ (equation 2) gives the value of the resistance R of the copper wire at any temperature $t + a$ in function of the resistance r at any temperature a. The constant 0·00192 reduced for the centigrade thermometer becomes 0·00359. Becquerel

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gives 0·004.* No allowance was made for the alteration in the temperature of the resistance coils, but these being made of nickel wire, their resistance probably changed little in the course of the experiments.

The two galvanometers employed were of such different delicacy that it was not possible to compare them directly by including them in the same circuit. An intermediate instrument, a sine galvanometer called S 2, was employed for this purpose. Three observations were taken, comparing the sine first with T 1, and secondly with T 2. The results appear in Tables XXII. and XXIII.

The divisor adopted in consequence of these experiments, to reduce the tangents of the observations on T 2 to the tangents of the angles that would have been observed upon T 1, was 42·4.

T was an ordinary tangent galvanometer. The magnet was 2 in. long, and fastened to a brass pointer, 3¾ in. long. The diameter of the directing coils was 13½ in. The diameter of the wire used in the coils was 0·026 in.

T 2 was a tangent galvanometer made from Dr. Joule's instructions. A little magnet ⅜ in. long was fastened at right angles to a very light glass pointer, 2¾ in., and suspended by a single silk fibre, in. long. The weight of the magnet and pointer together was not one grain. The needle of this instrument came to rest very quickly, owing to its small weight, a point of great importance in the insulation test.

The internal diameter of the directing coils was 4⅛ in. The diameter of the wire used in the coils was 0·017 in.

The elements necessary for the comparison of the insulation and continuity tests are thus complete, and in order to obtain the resistance of the gutta-percha covering, we have only to state and resolve the equation expressing the relation between the currents which flow during the continuity and insulation tests.

The circuit of the continuity test is composed of the battery B R, the coils of the tangent galvanometer T 1, and the copper strand of the cable, which will be called M. The resistance of the cable was no doubt diminished in each case by a slight loss through the gutta-percha, greatest at each end of the cable, and gradually diminishing towards the centre, the gutta-percha forming a relieving arc of great resistance. This loss or relieving arc has, however, been neglected. The circuit of the continuity test being considered, C 1 = B R + T 1 + M.

The circuit in the insulation test is composed of B R, the coils of T 2, the resistance of the gutta-percha covering, x, and half the resistance of the copper strand $= \frac{M}{2}$. This last item results from the consideration that the whole current passes through the first foot of the wire, but the amount passing through each successive foot of the wire diminishes in proportion to the distance from the end connected with the battery, until through the last foot of the wire, no portion of the current may be said to pass. The circuit of the insulation test, therefore, $C\,2 = B\,R + T\,2 + \frac{M}{2} + x$.

All the quantities in C 1 and C 2 are known, with the exception of x.

The relation between C 1 and C 2 is given by the deflections observed in those circuits. Thus: let D be the deflection due to C 1, and d the deflection due to C 2, then (equation 3),

$$C\,1 : C\,2 :: \text{tang. } d : 42{\cdot}4,\ \text{tang. } D, \text{ and}$$

$$x = \frac{42{\cdot}4 \text{ tang. } D \times (B\,R + T\,1 + M)}{\text{tang. } d} - \left(B\,R + T\,2 + \frac{M}{2}\right).$$

The resistance G of the gutta-percha covering is thus obtained in the same units as those employed to express the resistance, M, B R, T 1, and T 2.

Professor Thomson's formula gives the connexion between the resistance of the hollow cylindrical coating and the resistance offered by a cubic foot of gutta-percha electrified on one face to the passage of electricity to the opposite face of the cube; in other words, to the specific resistance of gutta-percha referred to volume.

This formula will be proved if, from cables of very different proportions, but composed of the same material, we obtain the same value for the specific resistance of the material. The author has had very limited opportunities of verifying the formula, which will probably be found defective in extreme cases, owing to the influence of extra resistance of the gutta-percha due to continued electrification not having been taken into account. Wherever the author has had an opportunity of comparing cables of different dimensions, the numbers deduced by the formula have closely agreed with those given by observations when the observations were taken shortly after the first connexion of the wire with the battery.

The formula alluded to (No. 1). $S = \frac{2\pi L G}{\log. \frac{a}{b}}$ has been already stated at the beginning of the second part of this paper. This equation assumes that the resistance of an insulating cover is directly as the $\log. \frac{a}{b}$, directly as the resistance of the material, and inversely as the length of the wire covered.

The loss will of course be inversely as the resistance. That the loss is inversely proportional to the resistance of the material employed, and directly as the length of wire covered, is a statement which will hardly be denied, and has been found to hold true in lengths of from 1 knot to 2,000.

As regards the influence of the thickness of the gutta-percha cover, the following examples support the formula employed.

For the Red Sea cable $\log. \frac{a}{b} = 0{\cdot}5315$, taking the diameter of the strand somewhat less than that of the circumscribing circle; the loss at 50° upon No. 1 experimental coil, taken from the upper curve in Plate IV., is 610. The loss upon the Red Sea coil of the same length taken from the upper curve in Plate VI. is 860.

Now 5315 : 6990 = 610 : 802.

According to Professor Thomson's theory, therefore, the loss observed should have been 802 instead of 860.

At 60° from the same curves the respective observed losses were 1,160 and 1,460.

Now 5315 : 699 = 1160 : 1525.

The calculated and observed losses are respectively 1460 and 1525. The above numbers are from the copper curve. The calculated loss on the Red Sea coil is at 50° seven per cent. in defect, and at 60°, about four per cent. in excess of the observed loss. The observed loss at 60° from the zinc curve is about 1540 instead of 1525, a remarkable coincidence of theory and observation. As might have been expected, the loss at the lower temperature is greater where Chatterton's compound was used, and less at the higher temperature than the loss through pure gutta-percha; unfortunately the uncertain character of the experiments on which the curve for the Red Sea coil at low temperatures is based greatly diminishes the value of this comparison. The extra resistance tends to diminish the loss on the Red Sea coil to a greater extent than in No. 1 coil, and but for this the author believes that even at 60° the specific resistance of the covering of the Red Sea coil would have been less than that of No. 1 coil.

The loss after five minutes continued application of copper pole of the battery is very nearly equal from No. 1 coil and the Red Sea coil. The law enunciated is no longer applicable in this case, being overthrown by the unexpected influence of extra resistance. It would be most desirable that a series of experiments should be made on cables of very different proportions, for the purpose of ascertaining how far the equation given can be relied on, and applied for the purpose of comparing insulating covers of various cables. It is also very desirable that extended experiments should be made with the object of determining the law according to which extra resistance increases with the increased mass and surface electrified.

Meanwhile the author has felt justified in using equation (No. 1) by the agreement observed between theory and observation in the case cited, and by his confidence in the correctness of Professor's Thomson's theoretical views.

In applying the values of S obtained by this formula, however, the influence of extra resistance must never be lost sight of, and owing to this element the author is strongly of opinion that the resistance of a cube $\frac{1}{10}$th of an inch of gutta-percha would be very different from that of a cube the side of which measured one inch, and this again from that of a cube the side of which measured a foot. In this particular gutta-percha does not therefore appear to resemble the metals or good conductors, and any deductions founded on the assumption that the resistance increases as the length of the arc, and diminishes in proportion to its area, must be applied with caution.

With these reservations, the author gives the values contained in Table XXIV. and XXV. of the specific resistance of gutta-percha, calculated by means of equations 1 and 2, from a series of tests made on four coils of the Red Sea cable when in the holds of the ships "Imperador" and "Impersbriz" previous to their departure for India.

The deflections observed and other particulars of the experiment are given in Table XXIV., the contents of which

are sufficiently explained by the headings. The resistance of the battery added to that of T 1 was calculated in the manner before described; but it is somewhat unfortunate that on this day there exists a wider discrepancy between the various values of B. R. got from various added resistances than was usually observed. The difference, amounting to 3 or 4 G. M., although considerable in relation to the total resistance of the battery, is inconsiderable in relation to the total resistance of the circuit, and the error due to this defect is small.

The resistance of the copper wire of each cable was measured directly by the differential instrument. The calculated resistance and the observed resistance agreed very closely; thus the calculated resistance (by equation?) for 512 knots was 68·99 G. M.; the observed resistance, 69·3 G. M., and on the other lengths the agreement between observation and calculation was still more perfect.

The following is an example of the calculation employed to obtain the resistance G and specific resistance S from observations on 512 knots of cable in the main hold of the "Imperador."

Four different determinations give B R + T = 13·26
" = 18·09
" = 17·82
" = 13·87

Mean value of B R + T = 15·76

B R = 15·76 − T 1 = 12·41 G M; substituting the particular values in equation 3, we have,

$$\frac{42{\cdot}4\ D \times (B\ R + T1 + M)}{\text{tang. } d} =$$

$$\frac{42{\cdot}4 \times 0{\cdot}864 \times (12{\cdot}41 + 3{\cdot}35 + 68{\cdot}99)}{1{\cdot}235} = 2513{\cdot}92 \text{ G M.}$$

and $B\ R + T2 + \frac{M}{2} = 12{\cdot}41 + 79{\cdot}8 + 34{\cdot}49 = 126{\cdot}7$ G M.

G = 2513·92 − 126·7 = 2387·22 G M = absolute resistance of the gutta-percha covering. This number is given in the third division of Table XXIV.

Then by equation 1, $x = \frac{2\ \pi\ L\ G}{\log.\ \frac{a}{b}}$ where L = the length of the cable in feet, = the length of the cable in knots multiplied by 6082.

Moreover, to express x in Thomson's units, G must be multiplied by $185{,}658 \times 10^6$.

$$x = \frac{2 \times 3{\cdot}1416 \times 512 \times 185{,}658 \times 10^6 \times 2{,}387{\cdot}22}{0{\cdot}5315} = 1{,}631 \times 10^{17}.$$

The fourth division of Table XXIV. contains the values of x, or the specific resistances of the covering of each cable at the end of each successive minute.

Table XXV. contains the mean specific resistance of the four tables.

The tests given in Table XXIV. and XXV. are the most complete which the author has yet made; and the specific resistances of the coverings of the three coils, at various temperatures, have been deduced with the aid of the temperature curves given in the first part of the paper, from the mean value of the resistance after one minute's electrification, contained in Table XXV.

The resistance during the first minute is very nearly alike, with copper and with zinc to cable; but the resistance increases more with zinc to cable than with copper to cable, so that after five minutes the resistances differ considerably. The author has little doubt that, had the wire been covered with pure gutta-percha, little or no difference between the two tests would have been observed.

A certain analogy exists between the resistance of metals to electricity, and that of gutta-percha. Metals are heated by the current, and when heated resist the current more than before.

In metals, as in gutta-percha, therefore, an extra resistance is caused by the passage of a current, although that extra resistance must be due to a different immediate cause since gutta-percha, when warm, offers less resistance than when cold. Indeed the author is not aware of any experiments proving that the passage of electricity warms gutta-percha.

The heat developed at a fault by the passage of a current does not prove that gutta-percha is heated by the current, and it is possible that the electrical energy may undergo some partly or wholly different transformation. It appears most probable to the author that the extra resistance is due to an effect of polarization taking place in the mass of the gutta-percha under the influence of the current, and causing a current in the opposite direction. Differences observed in the continuity tests with different poles of the battery, and the presence of currents in the cable after electrification, point to this solution.

The author regrets that these anomalous appearances were observed only very shortly before the cables left England, and the experiments are too uncertain to afford ground for definite conclusions; but he hopes at some future period to study these phenomena, and should the results be of interest, to lay them before the society.

The difference between the tests with different poles of the battery would on the above view be similar to the difference which would be found between currents sent in opposite directions through a galvanic couple, the current due to that couple being in one case added to and in the other case subtracted from the main current. It does not appear that any such galvanic couple is formed by pure gutta-percha and copper, since the tests on coil No. 2 were quite similar with the two poles of the battery.

On the other hand, the want of symmetry between the tests of both No. 1 and No. 3 with opposite poles, points, on the above view, to some chemical action between either the copper and the compound or the gutta-percha and the compound.

The relation of the tests with negative and positive poles, and the amount and duration of extra resistance, may, when fully investigated, become valuable data for the detection of the position and nature of faults in a cable.

It may be remarked that in the tests of the cable in the holds, Table XXIV., the resistance after five minutes' electrification, is invariably greater with zinc than with copper to coil, whilst the reverse was the case with tests on the single knot of No. 1, in water. The tests of the single knot of Red Sea cable are hardly numerous enough to afford grounds for a definite conclusion, but there does not appear to exist any well-defined difference in the copper and zinc tests in this case. The author cannot at present give any certain explanation of this discrepancy. Should the extra resistance really prove due to polarization, it is very possible that the fact of immersion or non-immersion may influence the extra resistance, since the polarization may be either of the material of the gutta-percha or of molecules of water contained in it.

The currents produced by the cable itself, of which the copper and metal sheathing form one large plate, probably influenced the tests. The length of the cable may also have some effect.

In Plate VIII. the resistance of the gutta-percha at various temperatures is exhibited as deduced by the above method in absolute measurement from daily tests of cables during the process of manufacture. Tables XXV. and XXVI. give the particulars of these tests. The specific resistance of the various cables tested on the same day was considered inversely proportional to the various deflections, and directly proportional to the various lengths; a relation which is not strictly true, in consequence of the constants forming part of the circuit, but the error from this source is very small. The specific resistance was thus calculated directly for one cable each day, and the specific resistance of the other cables got by the above proportions.

These tests were, however, all somewhat imperfect. The tests of which the results are given in Table XXVI., and shown by small crosses in Plate VIII., were made before the phenomenon of extra resistance or the effect of frequently reversing the current had been observed. Although therefore the deflections were observed nearly as soon as the galvanometer needle ceased to oscillate the resistance calculated from these deflections is more nearly that due to the third or fourth minute of connexion than that due to a short connexion with the battery. The temperature noted in these experiments was observed outside the coil.

The observations given in Table XXVII., and represented by small circles on Plate VIII., were made after the effect of reversing the current had been observed, and the rule followed in these observations was to note the deflection which occurred as soon as they had ceased to oscillate.

This plan, however, has not been found to give very regular results; different observers by requiring a slightly different degree of steadiness in the needle note different deflections.

The time during which the reverse current has been connected with the cable also modifies the result. For the future, therefore, it is intended that the reverse current shall always be continued for a definite time, say three minutes, previous to that current being admitted from which the deflection is to be noted, and to note that deflection at a given fixed time, say one minute, after the admission of this second current. All the tests contained in Tables XXV. and XXVI. were made with copper to coil.

On Plate VIII. the ordinates of the six black circles show the mean resistance with copper to coil, deduced from the four complete tests of the 24th September. The lowest represents the resistance immediately after connexion; the second, third, fourth, fifth, and sixth show the resistance after one, two, three, four, and five minutes' electri-

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fication respectively. The dotted line passing through the second circle is the upper curve from Plate VI. transformed into a resistance curve by making the ordinates of the curve in Plate VIII. reciprocal to the ordinates of the curve in Plate VI., the absolute height of the ordinates being determined by the ordinate at 62°, derived from the tests of September 24.

The irregularity of the results of the daily tests is certainly considerable, but is not more than might be expected from the causes named. The difference between the resistance at high and low temperatures in the daily tests is not so great as that observed in the short coils of one knot.

There are two causes of this discrepancy: one undoubtedly is that the large coils were not really at the temperature of the surrounding air, or even at the thermometers in the tubes in these extreme cases. But another cause is, that when on the same day two coils of equal or nearly equal length were tested at different temperatures (as frequently happened, owing to the position of the coils), a strong tendency was seen, even in conscientious observers, to read a rather high deflection for the cooler cable, and a rather low deflection for the warmer cable; so that the deflections noted should not appear to be very different for the same length of cable. The striking influence of extra resistance renders this self-deception very easy.

The resistance of the gutta-percha covering might be directly measured by a differential arrangement similar to that used for measuring the resistance of the copper strand; and probably this direct method would give more accurate results than the indirect method followed by the author, which was adopted in consequence of his having no measuring coils of sufficient resistance for this purpose, and because he had at that time no differential arrangement fit for measuring very large and rapidly-varying resistances.

It is possible that with the very feeble currents necessary in this arrangement, the resistances obtained may be dissimilar, and the effect of extra resistance modified. The author does not, however, expect such to be the case.

The specific resistance of the material covering No. 2 coil is $1{,}934 \times 10^{10}$ at 62°, calculated from the specific resistance of the Red Sea covering by the following formula (equ. No. 4):—

$$S = \frac{s.\, o.\, \log.\, \frac{a}{b}}{O \log. \frac{A}{B}}$$

Where s = mean specific resistance of ed Sea cable for the first minute from Table XXIV., o = mean ordinate of loss at 62° from Plates V. and VI. for first minute; a and b = external and internal diameter of the Red Sea cable; S = specific resistance of the covering of coil 2; O = ordinate for the first minute at 62° from Plates III. or IV.; A. and B. the external and internal diameters of the gutta-percha of coil No. 2; o was in this case taken as $1635 = \frac{1650 + 1630}{2}$. $s = 2018 = \frac{2{,}078 + 1{,}957}{2}$. o and s are made a mean between the result of positive and negative tests, because the author believes the slight difference after one minute's electrification to have been almost accidental. $O = 1297$. $\log \frac{a}{b} = 0{\cdot}5{,}315$, $\frac{a}{b}$ being taken as a little more than the ratio of the outer diameter to the diameter of the circle, circumscribing the strand; that is to say, as 3·4 instead of 3·238, $\log. \frac{A}{B} = 0{\cdot}699$.

Table XXVIII. contains the specific resistance at each degree of temperature, from 50° to 83° of the pure gutta-percha which covered coil No. 2; after one minute's connexion with the battery, and after five minute's connexion with the battery, with copper to coil and with zinc to coil.

These resistances are calculated by equation No. 4., and are inversely proportional to ordinates measured from the curves in Plates III. and IV.

The results contained in this Table are the most perfect of any obtained from the experiments, and the author hopes that the numbers given will be found practically useful. Professor Thomson in a lecture to the British Association (Dublin, 1857), gave the value of the resistance of gutta-percha as from 20×10^{18} to 200×10^{18} times that of copper, between the temperatures of 40° and 80° Fahrenheit. The specific resistance of the copper alluded to was about 2·5, and the specific resistance of the gutta-percha as estimated by Professor Thomson ranged from 50×10^{18} to 200×10^{18}, limits included within the values determined by the author of this paper.

Tables XXIX. and XXX. contain the specific resistances at the temperatures marked of the coverings of No. 1 coil and of the Red Sea coil, calculated from the ordinates of the curves in Plates III., V., and VI., by means of equation No. 4.

The total resistance which may be expected in any cable of any length covered with similar materials may be calculated by taking the numbers opposite the required temperature, in the formula before given, resolved with respect to G

$$G = \frac{S \log. \frac{a}{b}}{2\pi L}$$

$\frac{a}{b}$ being the ratio of the internal and external diameters of the covering, L the length of the cable in feet.

The object of the experiments is thus attained. The general results of the experiments are these:—

The relative loss at various temperatures through pure gutta-percha has been pretty accurately determined for all ordinary temperatures. To a less extent the same knowledge has been gained concerning the other coatings containing Chatterton's compound. The latter appears superior at high and inferior at low temperatures. Attention has been drawn to the fact that considerably increased resistance follows the continued electrification of gutta-percha and its compounds. Some of the laws of this extra resistance have been determined, and some suggestions made as to the cause of the phenomenon.

The bounds have been pointed out within which formulæ may be used, which consider gutta-percha as a conductor of a like nature to metals.

The resistance of gutta-percha coverings has been obtained in units, such as are employed to measure the resistance of metals, and by the use of Professor Thomson's formulæ the specific resistance of an unit of the material has been fixed with some accuracy.

The resistance of other conductors, such as glass and the resins, may now probably by comparison with gutta-percha be obtained in the same units.

Incidentally the increase of resistance in copper with increased temperature has been given from new experiments, and it has been shown that the insulation of a sound wire covered cable is little, if at all, affected by submersion.

Finally, tables and formulæ are given by which the resistance of or the loss through any new cable coated with gutta-percha may be at least approximately estimated.

This paper has extended to a length not contemplated by the author. It is, however, difficult to distinguish between the essentials and non-essentials of any experiment, and the author has frequently felt his confidence in the published result of an experiment diminished by apparently slight omissions in the account of the observation. He has therefore endeavoured to lay before the Society, together with the results, every fact having relation to those results; by this means he hoped that any erroneous deductions on his part might be corrected, and more complete inferences drawn by abler men.

FLEEMING JENKIN.

February 3, 1860.

TABLE 1.—August 31st.

Zero, 3,467. Temperature at beginning, 49¼. Do. at end, 49¾.

—	No. Observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Deflexion.	Sine corrected for E M F.
Zinc to No. 1 Coil	3,526	5·9	5·54	1093	1093	1169
	3,522	5·5	5·30	0938	0938	1089
	3,520	5·3	5·18	0924	0924	1051
	3,518	5·1	5·6	0889	0889	1011
	3,517	5·0	5·0	0872	0872	0991
Copper to No. 1 Coil	3,522	5·5	5·30	0938	0938	1089
	3,518	5·1	5·6	0889	0889	1011
	3,515	4·8	4·48	0837	0837	0952
	3,514	4·7	4·42	0819	0819	0931
	3,514	4·7	4·42	0819	0819	0931
Zinc to No. 2 Coil	3,498	3·1	3·6	0541	0541	0615
	3,494	2·7	2·42	0471	0471	0535
	3,494	2·7	2·42	0471	0471	0535
	3,493	2·6	2·36	0454	0454	0516
	3,493	2·6	2·36	0454	0454	0516
Copper to No. 2 Coil	3,500	3·3	3·18	0576	0576	0655
	3,496	2·9	2·54	0506	0506	0575
	3,495	2·8	2·48	0488	0488	0555
	3,492	2·5	2·30	0436	0436	0496
	3,491	2·4	2·24	0419	0419	0476
Zinc to connexions at beginning.	3,467	·0	—	—	—	—
Do. end	3,467	·0	—	—	—	—
Copper do. beginning	3,467	·0	—	—	—	—
Do. end	3,467	·0	—	—	—	—

S C + 30 = 60°. S C = 20·75.
S C + 59·15 = 47° 45′ E M F = 87·93. Reciprocal = 1137.

TABLE 2.—September 1st.

Zero 3,467. Temperature at beginning, 52½. Do. at end, 52¾.

—	No. Observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Con-nexion.	Sine corrected for S M F.
Zinc to No. 1 Coil -	3,557	7·0	7·0	1225	1185	1281
	3,533	6·6	6·36	1145	1115	1205
	3,523	6·2	6·12	1060	1046	1131
	3,527	6·0	6·0	1045	1011	1093
	3,524	5·7	5·42	0953	0923	0997
Copper to No. 1 Coil	3,527	6·0	6·0	1045	1010	1092
	3,523	5·6	5·36	0976	0941	1017
	3,521	5·4	5·24	0941	0906	0979
	3,519	5·2	5·12	0906	0871	0942
	3,518	5·1	5·6	0889	0854	0924
Zinc to No. 2 Coil -	3,505	3·8	3·48	0663	0629	0680
	3,501	3·4	3·24	0593	0559	0604
	3,500	3·3	3·18	0576	0542	0586
	3,498	3·1	3·6	0541	0507	0548
	3,497	3·0	3·0	0523	0489	0529
Copper to No. 2 Coil	3,505	3·8	3·48	0663	0628	0679
	3,501	3·4	3·24	0593	0558	0603
	3,498	3·1	3·6	0541	0506	0547
	3,497	3·0	3·0	0523	0488	0527
	3,496	2·9	2·54	0506	0471	0509
Zinc to connexions at beginning.	3,470	·3	·18	0032	} Mean 0034	—
Do. end - -	3,468	·1	·6	0017		—
Copper do. beginning	3,465	·2	·12	0035	} 0035	—
Do. end - -	3,467	·0	·0	0		—

S C + 30 = 61°. S C = 21·31.

S C + 59·15 = 49°. E M F = 92·5 Reciprocal = 1081.

TABLE 4.—September 3.

Zero 3,463. Temperature at beginning, 59°. Do. end, 59°.

—	No. Observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Con-nexion.	Sine corrected for S M F.
Zinc to No. 1 Coil -	3,550	8·7	8·42	1513	1201	1437
	3,547	8·4	8·24	1461	1339	1383
	3,543	8·0	8·0	1392	1270	1312
	3,541	7·8	7·48	1357	1235	1276
	3,538	7·5	7·30	1305	1183	1222
Copper to No. 1 Coil	3,534	7·1	7· 6	1236	1211	1251
	3,528	6·5	6·30	1132	1107	1143
	3,529	6·6	6·36	1149	1124	1161
	3,528	6·3	6·18	1097	1072	1107
	3,525	6·2	6·12	1080	1055	1090
Zinc to No. 2 Coil -	3,527	6·4	6·24	1115	0993	1026
	3,524	6·1	6· 6	1063	0941	0972
	3,523	6·0	6· 0	1045	0923	0953
	3,523	6·0	6· 0	1045	0923	0953
	3,522	5·9	5·54	1028	0906	0936
Copper to No. 2 Coil	3,527	6·4	6·24	1115	1090	1126
	3,522	5·9	5·54	1028	1003	1036
	3,519	5·6	5·36	0976	0951	0983
	3,516	5·3	5·18	0924	0899	0929
	3,515	5·2	5·12	0906	0881	0910
Copper, to connexion at beginning - -	3,468	·1	·6	0017	} Mean 0025	—
Ditto end - -	3,465	·2	·12	0035		—
Zinc ditto, beginning	3,474	·7	·42	0122	} 0122	—
Ditto end - -	3,470	·7	·42	0122		—

S C + 30 = 61½°. S C 22·54.

S C + 59·15 = 49°·50. E M F, 96·77. Reciprocal = 1018.

TABLE 3.—September 2nd.

Zero 3,467. Temperature at beginning, 55¼°. Do. at end, 55.

—	No. Observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Con-nexion.	Sine corrected for S M F.
Zinc to No. 1 Coil -	3,547	8·0	8·0	1392	1322	1397
	3,542	7·5	7·30	1305	1236	1277
	3,542	7·5	7·30	1305	1236	1277
	3,537	7·0	7·0	1219	1150	1188
	3,532	6·5	6·30	1132	1063	1098
Copper to No. 1 Coil	3,529	7·2	7.12	1253	1184	1223
	3,524	6·7	6·42	1167	1098	1134
	3,528	6·1	6·6	1063	994	1027
	3,526	5·9	5·54	1028	959	0991
	3,526	5·9	5·54	1028	959	0991
Zinc to No. 2 Coil -	3,521	5·4	5·24	0941	0872	0901
	3,517	5·0	5·0	0872	0803	1829
	3,514	4·7	4·42	0819	0750	0775
	3,513	4·6	4·36	0802	0733	0757
	3,512	4·5	4·30	0785	0716	0740
Copper to No. 2 Coil	3,521	5·4	5·24	0941	0872	0901
	3,514	4·7	4·42	0819	0750	0775
	3,512	4·5	4·30	0785	0716	0740
	3,511	4·4	4·24	0767	0698	0721
	3,510	4·3	4·18	0750	0681	0703
Zinc to connexions at beginning.	3,472	·5	·30	0087	} Mean 0069	—
Do. end - -	3,470	·3	·18	0032		—
Copper do. beginning	3,472	·5	·30	0087	} 0069	—
Do. end - -	3,470	·5	·18	0032		—

S C + 30 = 61½°. S C = 22·55.

S C + 59·15 = 49° 50′. E M F, 96·77. Reciprocal = 1033.

TABLE 5.—August 30.

Zero 3,463. Temperature at beginning, 61·6. Do. end, 61·6.

—	No. Observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Con-nexion.	Sine corrected for S M F.
Zinc to No. 1 Coil -	3,542	7·9	7·54	1374	1322	1428
	3,540	7·7	7·42	1340	1288	1391
	3,536	7·3	7·18	1271	1219	1316
	3,534	7·1	7· 6	1236	1184	1279
	3,532	6·9	6·54	1201	1149	1241
Copper to No. 1 Coil	3,539	7·6	7·36	1323	1268	1301
	3,534	7·1	7· 6	1236	1201	1291
	3,531	6·8	6·48	1184	1149	1241
	3,529	6·6	6·36	1149	1114	1203
	3,529	6·6	6·36	1149	1114	1203
Zinc to No. 2 Coil -	3,535	7·2	7·12	1253	1201	1297
	3,533	6·5	6·30	1132	1080	1166
	3,535	6·2	6·12	1080	1028	1110
	3,123	6·0	6· 0	1045	993	1072
	3,525	6·2	6·12	1080	1028	1110
Copper to No. 2 Coil	3,534	7·1	7· 6	1236	1201	1297
	3,531	6·8	6·48	1184	1149	1241
	3,526	6·3	6·18	1097	1062	1147
	3,525	6·2	6·12	1080	1045	1128
	3,524	6·1	6· 6	1063	1028	1110
Zinc to connexions at beginning - -	3,466	·3	·18	0092	} Mean 0089	—
Ditto end - -	3,466	·3	·18	0092		—
Copper do. beginning	3,463	·0	·0	0	} 0035	—
Ditto end - -	3,465	·2	·12	0035		—

S C + 30 = 61°. S C = 21·31.

S C + 59·15 = 49°. E M F, 92·57. Reciprocal = 1080.

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TABLE 6.—September 5.

Zero 3467. Temperature 82½°.

	No. Observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Conduction.	Sine corrected for 2 M. F.
Zinc to No. 1 Coil	3,547	8·0	8·0	1392	1392	1447
	3,543	7·6	7·36	1323	1323	1376
	3,540	7·3	7·18	1271	1271	1321
	3,540	7·3	7·18	1271	1271	1321
	3,538	7·1	7·6	1236	1236	1285
Copper to No. 1 Coil	3,541	7·4	7·24	1288	1288	1339
	3,545	7·8	8·36	1184	1184	1231
	3,533	6·6	6·36	1149	1149	1194
	3,531	6·4	6·24	1115	1115	1159
	3,530	6·3	6·18	1097	1097	1141
Zinc to No. 2 Coil	3,541	7·4	7·24	1288	1288	1339
	3,534	6·7	6·42	1167	1167	1213
	3,533	6·6	6·36	1149	1149	1194
	3,531	6·4	6·30	1132	1132	1177
	3,531	6·4	6·24	1115	1115	1160
Copper to No. 2 Coil	3,538	7·1	7·6	1236	1236	1285
	3,533	6·6	6·36	1149	1149	1194
	3,530	6·3	6·18	1097	1097	1140
	3,529	6·2	6·12	1080	1080	1123
	3,528	6·1	6·6	1063	1063	1106
Zinc to connexion at beginning.	3,467	·0				
Do. do. end	3,467	·0				
Copper do. beginning	3,467	·0				
Do. do. end	3,467	·0				

S C + 30° = 62° 15′. S C = 20·6.

S C + 59·15 = 56° 20′. E M F = 96·17. Reciprocal = 1040.

TABLE 8.—September 7.

Zero 3468. Temperature of Bottom coil, 71°. Top coil, 71½°.

	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Conduction.	Sine corrected for 2 M. F.
Zinc to No. 1 Coil	3,557	9·4	9·24	1633	1476	1672
	3,556	9·3	9·18	1616	1459	1653
	3,553	9·0	9·0	1564	1407	1594
	3,550	8·7	8·42	1513	1356	1536
	3,549	8·6	8·36	1495	1338	1516
Copper to No. 1 Coil	3,538	7·5	7·30	1305	1252	1418
	3,529	7·6	6·36	1149	1096	1241
	3,523	6·9	6·0	1045	0992	1123
	3,522	5·9	5·54	1028	0975	1105
	3,522	5·9	5·54	1028	0975	1105
Zinc to No. 2 Coil	3,603	14·0	14·0	2419	2252	2551
	3,596	13·2	13·18	2300	2143	2428
	3,593	13·0	13·0	2249	2092	2370
	3,591	12·8	12·48	2215	2058	2331
	3,590	12·7	12·42	2196	2041	2312
Copper to No. 2 Coil	3,592	12·9	12·54	2232	2179	2468
	3,583	12·2	12·12	2109	2066	2333
	3,578	11·5	11·30	1994	1941	2199
	3,577	11·4	11·24	1977	1924	2180
	3,577	11·4	11·24	1977	1924	2180
Zinc to connexion at beginning.	3,470	0·7	0·42	122	Mean. 157	
Do. do. at end	3,474	1·1	1·6	192		
Copper do. beginning	3,465	0·2	0·12	35	53	
Do. do. at end	3,467	0·4	0·24	70		

S C + 30 = 61°. S C = 18·9.

S C + 59·15 = 48° 30′. E M F = 88·22. Reciprocal = 1133.

TABLE 7.—September 6.

Zero 3461. Temperature, Top coil, No. 2, 66½°. Bottom coil, No. 1, 65½°.

	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Conduction.	Sine corrected for 2 M. F.
Zinc to No. 1 Coil	3,540	7·9	7·54	1374	1374	1575
	3,536	7·5	7·30	1305	1305	1496
	3,535	7·4	7·24	1288	1288	1479
	3,535	7·4	7·24	1288	1288	1477
	3,535	7·4	7·24	1288	1288	1477
Copper to No. 1 Coil	3,527	6·6	6·36	1149	1149	1317
	3,518	5·8	5·48	1011	1011	1159
	3,514	5·3	5·18	0924	0924	1059
	3,513	5·2	5·12	0906	0906	1039
	3,510	4·9	4·54	0854	0854	980
Zinc to No. 2 Coil	3,544	8·3	8·18	1444	1444	1656
	3,534	7·3	7·18	1271	1271	1457
	3,531	7·0	7·0	1219	1219	1398
	3,530	6·9	6·54	1201	1201	1377
	3,530	6·9	6·54	1201	1201	1378
Copper at No. 2 Coil	3,541	8·0	8·0	1392	1392	1596
	3,535	7·4	7·24	1288	1288	1477
	3,532	7·1	7·6	1236	1236	1417
	3,531	7·0	7·0	1219	1219	1398
	3,530	6·9	6·54	1201	1201	1378
Zinc to connexion at beginning.	3,461	·0	·0	0		
Do. do. at end	3,461	·0	·0	0		
Copper do. beginning	3,461	·0	·0	0		
Do. do. at end	3,461	·0	·0	0		

S C + 30 = 61° 10′. S C = 18·01.

S C + 59·15 = 48·30. E M F = 87·21. Reciprocal = 1447.

TABLE 9.—September 7, 2nd Exp.

Zero 3565. Temperature, Top coil No. 76°. Bottom No. 74°.

	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Conduction.	Sine corrected for 2 M. F.
Zinc to No. 1 Coil	3,556	9·1	9·6	1582	1582	1751
	3,556	9·1	9·6	1582	1582	1751
	3,554	8·9	8·54	1547	1547	1712
	3,552	8·7	8·42	1513	1513	1675
	3,552	8·7	8·42	1513	1513	1675
Copper to No. 1 Coil	3,544	7·9	7·54	1374	1374	1521
	3,542	7·7	7·42	1340	1340	1483
	3,532	7·7	7·42	1340	1340	1483
	3,532	7·7	7·42	1340	1340	1483
	3,534	7·7	7·42	1340	1340	1483
Zinc to No. 2 Coil	3,637	17·2	17·12	2957	2957	3273
	3,631	16·6	16·36	2857	2857	3162
	3,629	16·4	16·24	2823	2823	3125
	3,628	16·3	16·18	2807	2807	3107
	3,628	16·3	16·18	2807	2807	3107
Copper to No. 2 Coil	3,637	17·2	17·12	2157	2957	3273
	3,628	16·3	16·18	2807	2807	3107
	3,625	16·0	16·0	3756	2756	3050
	3,623	15·8	15·48	2726	2723	3014
	3,623	15·8	15·41	2723	2723	3014
Copper to connexion at beginning.	3,465					
Do. do. at end	3,465					
Zinc do. at beginning	3,465					
Do. do. at end	3,465					

S C + 30 = 61° 20′. S C = 19·4.

S C + 59·15 = 49°. E M F = 96·30. Reciprocal = 1157.

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TABLE 10.—September 8.

Zero 460. Temperature, Top coil, 78.
Do. Bottom, 76.

	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Con-nexion.	Sine corrected for E M F.
Zinc to No. 1 Coil	3,558	10·6	10·36	1820	1752	1939
	3,562	10·0	10·0	1736	1649	1825
	3,560	9·8	9·48	1702	1615	1787
	3,556	9·4	9·24	1633	1946	1711
	3,552	9·0	9·0	1564	1467	1624
Copper to No. 1 Coil	3,563	10·0	10·0	1736	1719	1902
	3,552	9·0	9·0	1564	1547	1712
	3,541	7·9	7·54	1374	1357	1502
	3,541	7·9	7·54	1374	1307	1502
	3,539	7·7	7·42	1340	1323	1463
Zinc to No. 2 Coil	76	21·4	21·24	3649	3362	3943
	69	20·3	20·18	3469	3382	3743
	60	19·8	19·48	3390	3303	3656
	61	19·9	19·54	3404	3317	3672
	61	19·9	19·54	3404	3317	3672
Copper to No 2 Coil	63	20·3	20·18	3469	3422	3821
	59	19·7	19·42	3371	3354	3712
	58	19·6	19·36	3354	3337	3694
	56	19·4	19·24	3322	3305	3659
	56	19·4	19·24	3322	3305	3659
Copper to connexion at beginning.	3,461	0·1	0·6	0017	Mean. 0017	
Do. do. at end	3,464	0·1	0·6	0017		
Zinc do. at beginning.	3,463	0·3	0·18	0052	0174	
Do. do. at end	3,470	0·7	0·42	0122		

S C + 30 = 61°. B R = 20·08.
S C + 59·15 = 483°. E M F 90·34. Reciprocal = 1107.

TABLE 12.—October 15.

Zero 2291. Temperature 49°

	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Con-nexion.	Sine corrected for E M F.
Zinc to Coil No. 3	2,355	6·4	6·24	1116	635	733
	2,351	6·0	6·0	1045	565	652
	2,344	5·3	5·10	924	444	512
	2,341	5·0	5·0	872	392	452
	2,340	4·9	4·54	854	374	432
Copper do.	2,341	5·0	5·0	872	680	785
	2,336	4·5	4·30	785	593	684
	2,330	3·9	3·54	680	485	563
	2,329	·5	3·30	610	418	482
	2,325	3·4	3·24	593	401	463
Zinc to connexion at beginning.	2,319	2·8	2·48	488	480	
Do. end	2,318	2·7	2·42	471		
Copper do. beginning.	2,302	1·1	1·6	192	192	
Do. end	2,302	1·1	1·6	192		

S C + 60° = 47°. S C = 21°65.
S C + 90 = 38°·15. E M F = 86°62. Reciprocal = 1°154.

TABLE 11.—September 8.

Zero 3463. Temperature, Top coil, 83°
Do. Bottom do. 80°

	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Con-nexion.	Sine corrected for E M F.
	3,584	12·1	12·6	2096	1808	1952
	3,574	11·1	11·6	1926	1638	1769
	3,567	10·4	10·24	1800	1518	1639
	3,565	10·2	10·12	1771	1533	1602
	3,565	10·2	10·12	1771	1483	1602
	3,564	10·1	10·6	1754	1554	1678
	3,560	9·7	9·42	1685	1485	1603
	3,560	9·7	9·42	1685	1485	1603
	3,545	8·2	8·12	1426	1226	1324
	3,542	7·9	7·54	1374	1174	1268
	20·4	34·1	34·6	5606	5318	5743
	19·7	33·4	33·24	5338	5070	5475
	17·8	31·5	31·30	5225	4937	5331
	17·0	30·7	30·42	5160	4816	5232
	16·9	30·6	30·36	5050	5802	5186
	18·0	31·7	31·42	5255	5055	5459
	15·5	29·2	29·12	4879	4679	5053
	15·5	29·2	29·12	4879	4679	5053
	14·8	28·5	28·30	4712	4572	4937
	14·4	28·1	28·6	4710	4510	4870
	3,476	1·3	1·18	227	288	
	3,483	2·0	2·0	349		
	3,476	1·2	1·12	209	209	
	3,474	1·1	1·6	192		

S C + 30 = 61°. S C = 21°2.
S C + 59·15 = 49°. E M F = 92·55. Reciprocal = 1080.

TABLE 13.—October 17.

Zero 2290. Temperature 52°

	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Con-nexion.	Sine corrected for E M F.
Zinc to Coil No. 3	2,364	7·4	7·24	1288	904	1155
	2,353	6·3	6·18	1097	713	911
	2,348	5·8	5·48	1011	627	801
	2,345	5·5	5·30	0958	574	734
	2,343	5·3	5·18	0924	540	690
Copper to Coil No. 3	2,347	5·7	5·42	993	679	868
	2,339	4·8	4·48	837	523	668
	2,335	4·5	4·30	785	471	602
	2,332	4·2	4·12	733	418	534
	2,330	4·0	4·0	698	384	491
Zinc to connexion at beginning	2,312	2·2	2·12	384	Mean. 384	
Do. end	2,312	2·2	2·12	380		
Copper do. beginning	2,314	2·4	2·24	419	314	
Do. do. end	2,302	1·2	1·12	209		

S C + 59·15 = 47°·45. S C = 11°92.
S C + 88·27 = 38°. E M F = 78·248. Reciprocal = 1·278.

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Table 14.—October 19.

Zero 2236. Temperature 53°·50

—	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Conn.	Sine corrected for E M F.
Zinc to Coil No. 3	2,408	12·2	12·12	2113	1329	1514
	2,392	19·6	19·36	1839	1055	1202
	2,376	9·2	9·12	1599	815	928
	2,375	8·9	8·54	1547	763	869
	2,372	8·6	8·36	1496	712	811
Copper to Coil No. 3	2,374	8·8	8·36	1530	1042	1187
	2,376	9·0	9· 0	1564	1076	1226
	2,394	6·8	6·48	1184	696	793
	2,348	6·9	6·12	1090	592	674
	2,347	6·1	6·6	1063	575	655
Zinc to connexion at beginning.	2,327	4·1	4·6	715	Mean 784	
Do. end	2,335	4·9	4·54	854		
Copper do. beginning	2,311	2·3	2·30	436	488	
Do. end	2,317	3·1	3·6	541		

S C + 60 = 46°·30. S C = 24·15.
S C + 90 = 38°·0. E M F = 87·80. Reciprocal = 1·39

Table 17.—September 7.

Zero 3463. Temperature 74°.

—	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Conn.	Sine corrected for E M F.
Zinc to No. 3 Coil	3,624	16·1	16·6	2772	2511	2845
	3,596	13·3	13·18	2300	2036	2309
	3,582	11·9	11·54	2062	1800	2039
	3,573	11·0	11·0	1908	1646	1865
	3,569	10·6	10·36	1840	1578	1788
	3,567	10·4	10·24	1805	1543	1748
Copper to No. 3 Coil	3,626	16·3	16·18	2807	2695	3042
	3,585	12·2	12·12	2115	1991	2256
	3,570	10·7	10·42	1857	1735	1966
	3,562	9·9	9·54	1719	1597	1809
	3,554	9·1	9·6	1582	1460	1654
Zinc to connexion	3,478	1·5	1·30	262		
Copper do.	3,470	0·7	0·42	122		

S C + 30 = 61°. S C = 18·9.
S C + 59·15 = 48°·3. E M F = 88·22. Reciprocal = 1·133.

Table 15.—September 6.

Zero 3465. Temperature at top, 51½°.
" at end, 58½°.

—	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Conn.	Sine corrected for E M F.
Zinc to No. 3 Coil	3,543	7·8	7·48	1367	1305	1468
	3,532	6·7	6·42	1167	1115	1254
	3,525	6·0	6·0	1045	993	1117
	3,522	5·7	5·42	993	941	1059
	3,519	5·4	5·24	941	889	1000
Copper to No. 3 Coil	3,534	6·9	6·54	1201	1194	1343
	3,524	5·9	5·54	1028	1021	1149
	3,520	5·5	5·30	958	951	1070
	3,515	5·0	5·0	872	865	9731
	3,513	4·8	4·48	837	830	934
Zinc to connexion	3,468	0·3	·18	52		
Copper do.	3,466	0·1	·6	7		

S C + 30 = 61° 20'. S C = 18'.62.
S C + 59'·15 = 48° 53'. E M F = 88'·93. Reciprocal = 1'·125.

Table 18.—September 8, 1859.

Zero 3463. Temperature 79°.

—	No. observed.	Angle.	Angle in Minutes.	Sine.	Sine corrected for Conn.	Sine corrected for E M F.
Zinc to Coil No. 3	3,646	18·3	18·18	3140	3018	3341
	3,619	15·6	15·36	2690	2568	2843
	3,602	13·9	13·54	2402	2280	2524
	3,597	13·4	13·24	2317	2195	2430
	3,590	12·7	12·42	2198	2076	2298
Copper to Coil No. 3	3,670	20·7	20·42	3535	3518	3894
	3,633	17·0	17·0	2934	2905	3216
	3,603	14·0	14·0	2420	2403	2660
	3,589	13·6	13·36	2351	2334	2584
	3,594	13·1	13·6	2266	2249	2489
Zinc to connexion	3,470	·7	·42	0122		
Copper do.	3,464	·1	·6	0017		

S C + 30 = 61°. B R = 20·08.
S C = 59·15 = 48½°. E M F = 90·34. Reciprocal = 1·107.

Table 16.

Zero 3463. Temperature 71°.

—	No. observed.	Angle.	Angle to Minutes.	Sine.	Sine corrected for Conn'xion.	Sine corrected for E M F.
Zinc to No. 3 Coil	3,610	14·7	14·42	2538	2172	2340
	3,587	12·4	12·24	2147	1781	1923
	3,573	10·9	10·54	1891	1525	1647
	3,561	9·8	9·48	1702	1336	1443
	3,558	9·5	9·30	1650	1284	1387
Copper to Coil No. 3	3,620	15·7	15·42	2706	2514	2715
	3,587	12·4	12·24	2147	1955	2111
	3,570	10·7	10·42	1857	1665	1798
	3,561	9·8	9·48	1702	1510	1631
	3,555	9·2	9·12	1599	1407	1520
Zinc to connexion	3,484	2·1	2·6	366		
Copper do.	3,472	1·1	1·6	192		

S C + 30 = 61°. S C = 21·3.
S C + 59·85 = 49°. E M F = 92·55. Reciprocal = 1·080.

Table 21.

Experiments made to determine the Resistance of Copper Wire at various Temperatures.

Two knots of Copper Wire compared with 17823×10^3, Thomson's units.

Mean Temperature of Water, Fahrenheit.	Temperature of Room, Fahrenheit.	Observed Division of Measuring Wire in Point C to Right of Centre.	Point C to Left of Centre.	Mean Position of C.	Mean Position of C.	Resistance in Thomson's Units.
	89		7429	74170	28880	620×10^6
	83½	69¼	7440	74275	25725	616×10^6
	73¾	68¾	7469	74495	25505	611×10^6
	66¾	68¾	7492	74805	25200	606×10^6
	61¾	68¾	7506	74965	24935	592×10^6
	53¾	69¼	7559	75270	24730	583×10^6

TABLE 22.

EXPERIMENTS made to determine the comparative Delicacy of Tangent Galvanometer called T_1, and Sine Galvanometer called S_1.

Tangent Galvanometer T_1.		Sine Galvanometer S_1.		Ratio.
Angle Observed.	Tangent.	Angle Observed.	Sine.	
° ′		° ′		
5 30	0963890	48 29	7333690	7·513
5 45	1006947	48 45	7313296	7·469
6 0	1051042	51 30	7329032	7·466
Mean ratio of indications of T_1 to indications of T_2 - -				7·475

TABLE 23.

EXPERIMENTS made to determine the comparative Delicacy of Sine Galvanometer called S_2, and Tangent Galvanometer called T_2.

Sine Galvanometer S_2.		Tangent Galvanometer T_2.		Ratio.
Angle observed.	Sine.	Angle observed.	Tangent.	
°		° ′		
7	1278693	34 30	6872810	5·64
8	1391731	36 35	7264339	5·72
9	1564345	41 30	8847253	5·65
Means ratio of indications of T_2 to indications of S_2 - -				5·67

$5·67 \times 7·475 = 42·38$ = Ratio of indications of T_2 to indications of T_1.

TABLE 24.

	Main Hold, Imperador.		Fore Hold, Imperador.		Main Hold, Imperatriz.		Fore Hold, Imperatriz.	
Length in Knots -	512		464		443		436	
Resistance in G. M. -	68·99		62·56		60·1		58·75	
Continuity on T_1,	40°.50′		43·15		42°.15′		44°.50′	
B. R. in G. M. -	15·76 G. M.		13·76 G. M.		14·00 G. M.		15·76 G. M.	
Insulation Test on T_1.	zinc.	copper.	zinc.	copper.	zinc.	copper.	zinc.	copper.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Needle steady -	51. 0	51. 0	46. 0	49. 0	46. 30	47. 0	47. 30	45. 0
After 30″ - -	—	48. 30	42. 30	46. 30	43. 30	44. 30	43. 30	44. 0
„ 1′ - -	44. 30	45. 0	39. 30	44. 0	39. 30	41. 30	41. 30	41. 0
„ 2′ - -	41. 0	43. 0	36. 30	42. 30	35. 30	38. 30	38. 0	38. 30
„ 3′ - -	38. 30	41. 0	35. 30	40. 0	33. 30	37. 0	36. 30	37. 0
„ 4′ - -	37. 30	39. 30	35. 0	38. 0	32. 30	36. 30	35. 0	36. 30
„ 5′ - -	36. 30	39. 0	35. 0	38. 0	31. 30	36. 0	34. 0	—
Resistance of Gutta-Percha Coating in G.M.	zinc.	copper.	zinc.	copper.	zinc.	copper.	zinc.	copper.
Needle steady -	2387·22	2387·22	2892·76	2593·76	2586·18	2540·74	2756·76	3018·69
After 30″ - -	—	2620·82	3288·10	2844·06	2885·57	2781·68	3187·45	3129·22
„ 1′ - -	3030·67	2978·00	3669·03	3111·33	3341·67	3103·04	3426·75	3492·08
„ 2′ - -	3446·02	3200·95	4099·21	3298·82	3880·66	3467·97	3899·25	3828·44
„ 3′ - -	3778·45	3446·02	4259·17	3600·99	4188·9	3663·09	4122·03	4042·23
„ 4′ - -	3921·15	3641·14	4340·56	3877·58	4358·04	3734·67	4364·52	4122·03
„ 5′ - -	4068·84	3706·26	4340·56	3877·58	4533·18	3803·61	4530·67	—
Specific Resistance in Thomson's Absolute British Units.	zinc.	copper.	zinc.	copper.	zinc.	copper.	zinc.	copper.
Needle steady -	1631×10^{17}	1631×10^{17}	1791×10^{17}	1606×10^{17}	1529×10^{17}	1502×10^{17}	1604×10^{17}	1757×10^{17}
After 30″ - -	—	1791 „	2036 „	1799 „	1709 „	1645 „	1855 „	1821 „
„ 1′ - -	2071 „	2035 „	2271 „	1926 „	1976 „	1835 „	1994 „	2032 „
„ 2′ - -	2355 „	2187 „	2538 „	2035 „	2299 „	2051 „	2269 „	2228 „
„ 3′ - -	2582 „	2355 „	2737 „	2228 „	2497 „	2166 „	2399 „	2343 „
„ 4′ - -	2680 „	2488 „	2687 „	2401 „	2582 „	2208 „	2540 „	2395 „
„ 5′ - -	2781 „	2533 „	2687 „	2401 „	2686 „	2249 „	2687 „	—

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TABLE 25.

MEAN SPECIFIC RESISTANCE of Gutta Percha from the Tests of Sept. 24, contained in Table 24.

Interval of Time elapsed between first Connexion of Cable with Battery and Observation.	Resistance with Zinc to Cable in Thomson's absolute Units.	Resistance with Copper to Cable in Thomson's absolute Units.
Needle first steady, *i.e.*, so soon as the resistance could be measured	1639×10^{17}	1624×10^{17}
After electrification for 30 seconds	1867 ,,	1761 ,,
,, ,, ,, 1 minute	2078 ,,	1957 ,,
,, ,, ,, 2 ,,	2365 ,,	2125 ,,
,, ,, ,, 3 ,,	2554 ,,	2275 ,,
,, ,, ,, 4 ,,	2622 ,,	2374 ,,
,, ,, ,, 5 ,,	2698 ,,	2399 ,,

TABLE 26.

DAILY TESTS before the Practice of Reversing had been adopted. —Temperatures marked taken Outside the Coil.

Date of Observation.	Coil or Coils tested.	Temperature in Degrees Fahrenheit.	Length of Cable in Knots.	Deflection—Insulity on T.	Deflection called Insulation on T.	BR = Resistance of Battery in G M.	Specific Resistance of Covering in T U.
		°		° ′	° ′		$\times 10^{17}$
Feb. 24	5, 6, 7, 8	48	269·7	34 0	24 30	32·00	2822
,,	1, 2, 3, 4	61	271·9	,,	28 30	,,	2389 ,,
Mar. 10	5, 6, 7, 8	50	382·2	50 15	32 30	34·97	3106 ,,
,,	1	62½	101·9	—	14 30	,,	2210 ,,
April 29	1, 5, 6, 7, 8	53	678·2	40 30	52 0	23·65	2819 ,,
May 11	1, 5, 6, 7	60	738·8	33 15	52 0	20·27	2362 ,,
,,	1	63½	186·2	—	20 4	,,	2122 ,,
June 2	1, 5, 6, 7	66	930·2	27 30	63 15	20·65	1818 ,,
July 25	5	68	325·9	54 0	45 0	19·4	1579 ,,
,,	1	75½	327·3	—	53	,,	1196 ,,

TABLE 27.

DAILY TESTS, after the Practice of Reversing had been adopted.—Temperatures taken in Tubes.

Date of Observation.	Coil or Coils tested.	Temperature in Degrees Fahrenheit.	Length of Cable in Knots.	Deflection, called Insulity on T.	Deflection called Insulation in T.	B R = Resistance of Battery in G M.	Specific Resistance of Covering in T U.
		°		° ′	° ′		$\times 10^{17}$
August 13	6	70½	327·5	60 6	57	23·26	1372
,,	5	70½	361·8	—	59	,,	1403 ,,
,,	7	70½	351·7	—	60	,,	1310 ,,
,,	1	79	367·5	—	66	,,	1066 ,,
August 17	1	76	372·4	53	62	22·41	1065 ,,
,,	5	67	367·4	—	56	,,	1308 ,,
,,	6	67	333·8	—	53	,,	1327 ,,
September 7	1	73	426·4	47 15	57	18·00	1233 ,,
,,	2	69½	129·0	—	21		1496 ,,
,,	3	67½	183·2	—	25	,,	1430 ,,
,,	4	67½	184·0	—	22	,,	1477 ,,
,,	5	61¾	267	—	34	,,	1562 ,,
,,	6	61	377·2	—	45	,,	1621 ,,
September 26	Fore hold	62	491	41	45	14·5	1870 ,,
,,	Main hold	62	443	—	42	,,	1874 ,,
,,	Fore hold	62	445	—	44	,,	1755 ,,
,,	Main hold	62	457	—	43	,,	1866 ,,

TABLE 28.

SPECIFIC RESISTANCE of pure GUTTA PERCHA.

Temperature.	Zinc to Coil. Specific Resistance after Electrification for 1 Minute.	Zinc to Coil. Specific Resistance after Electrification for 5 Minutes.	Temperature.	Copper to Coil. Specific Resistance after Electrification for 1 Minute.	Copper to Coil. Specific Resistance after Electrification for 5 Minutes.
°	$\times 10^{17}$	$\times 10^{17}$	°	$\times 10^{17}$	$\times 10^{17}$
50	4113	5663	50	4113	5663
51	3813 ,,	5119 ,,	51	3813 ,,	5109 ,,
52	3548 ,,	4654 ,,	52	3548 ,,	4654 ,,
53	3314 ,,	4289 ,,	53	3314 ,,	4259 ,,
54	3105 ,,	3926 ,,	54	3105 ,,	3926 ,,
55	2917 ,,	3636 ,,	55	2917 ,,	3636 ,,
56	2742 ,,	3376 ,,	56	2742 ,,	3376 ,,
57	2578 ,,	3136 ,,	57	2578 ,,	3136 ,,
58	2429 ,,	2920 ,,	58	2429 ,,	2920 ,,
59	2289 ,,	2724 ,,	59	2289 ,,	2724 ,,
60	2163 ,,	2549 ,,	60	2163 ,,	2549 ,,
61	2045 ,,	2389 ,,	61	2045 ,,	2389 ,,
62	1934 ,,	2242 ,,	62	1934 ,,	2242 ,,
63	1830 ,,	2106 ,,	63	1830 ,,	2106 ,,
64	1730 ,,	1978 ,,	64	1730 ,,	1978 ,,
65	1634 ,,	1858 ,,	65	1634 ,,	1858 ,,
66	1539 ,,	1742 ,,	66	1539 ,,	1742 ,,
67	1443 ,,	1626 ,,	67	1443 ,,	1626 ,,
68	1348 ,,	1510 ,,	68	1348 ,,	1510 ,,
69	1254 ,,	1399 ,,	69	1254 ,,	1399 ,,
70	1162 ,,	1291 ,,	70	1193 ,,	1291 ,,
71	1079 ,,	1193 ,,	71	1078 ,,	1192 ,,
72	1001 ,,	1103 ,,	72	999 ,,	1100 ,,
73	930 ,,	1021 ,,	73	924 ,,	1013 ,,
74	864 ,,	945 ,,	74	858 ,,	937 ,,
75	805 ,,	877 ,,	75	796 ,,	866 ,,
76	749 ,,	814 ,,	76	740 ,,	803 ,,
77	698 ,,	757 ,,	77	687 ,,	745 ,,
78	650 ,,	704 ,,	78	638 ,,	691 ,,
79	606 ,,	657 ,,	79	592 ,,	640 ,,
80	566 ,,	613 ,,	80	548 ,,	591 ,,
81	530 ,,	573 ,,	81	510 ,,	550 ,,
82	497 ,,	537 ,,	82	477 ,,	514 ,,
83	467 ,,	505 ,,	83	450 ,,	484 ,,

TABLE 29.

SPECIFIC RESISTANCE in Thomson's Units of the Red Sea Covering at various Temperatures.

Temperature.	Zinc to Cable. After Electrification for 1 Minute.	Zinc to Cable. After Electrification for 5 Minutes.	Copper to Cable. After Electrification for 1 Minute.	Copper to Cable. After Electrification for 5 Minutes.
°	$\times 10^{17}$	$\times 10^{17}$	$\times 10^{17}$	$\times 10^{17}$
60	2162	3330	2239	3465
65	1819 ,,	2847 ,,	1739 ,,	2770 ,,
70	1469 ,,	2378 ,,	1318 ,,	2239 ,,
75	1160 ,,	1753 ,,	1000 ,,	1739 ,,

TABLE 30.

SPECIFIC RESISTANCE in Thomson's Units of the Covering of No. 1 Experimental Coil at various Temperatures.

Temperature.	Zinc to Cable. After Electrification for 1 Minute.	Zinc to Cable. After Electrification for 5 Minutes.	Copper to Cable. After Electrification for 1 Minute.	Copper to Cable. After Electrification for 5 Minutes.
°	$\times 10^{17}$	$\times 10^{17}$	$\times 10^{17}$	$\times 10^{17}$
50	2023	2560	2240	2883
55	1836 ,,	2303 ,,	2036 ,,	2561 ,,
60	1718 ,,	2091 ,,	1886 ,,	2302 ,,
65	1598 ,,	1915 ,,	— ,,	— ,,
70	1491 ,,	1767 ,,	— ,,	— ,,
75	1401 ,,	1640 ,,	— ,,	— ,,
80	1320 ,,	1530 ,,		
0				

Fig. 1.

C and Z, poles of the battery.
E, earth-plate.
A B, wire coated with insulating material.
G, galvanometer introduced between Z and A to measure the loss through the coating of A B.

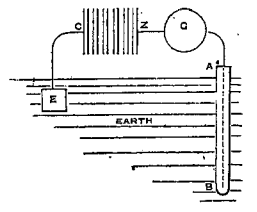


Fig. 2.

Section of No. 1 coil. Section of No. 2 coil. Section of No. 3 coil.

Fig. 3.

C and Z, poles of the battery.
A A, circular commutator with four terminal screws, *a*, *b*, *c*, *d*.
F, junction piece with four terminal screws *e*, *f*, *g*, *h*.
G, galvanometer, with four terminal screws *i*, *k*, *l*, *m*.
B, binding screw, which can be attached to *n*, *o*, *p*, or *q*, the ends of the two coils in tub T.
E, earth-plate.

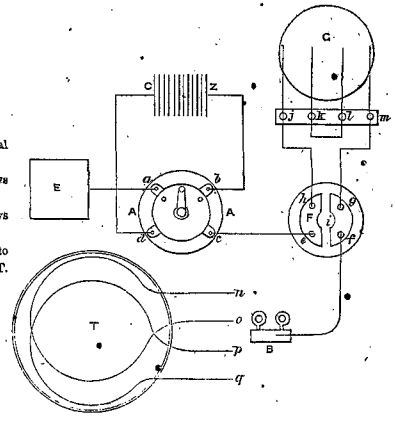
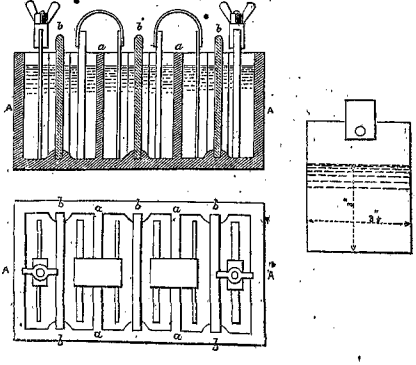


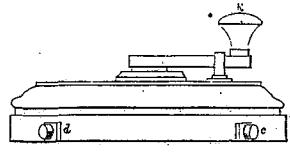
Fig. 4.

A A, glass trough divided into three compartments by the webs *a*, *a*.
b, *b*, *b*, porous diaphragms, secured with mastic in the grooves provided to receive them.



App. No. 14. Insulating properties of gutta percha, by Fleeming Jenkin.

Fig. 5.



a, *b*, *c*, *d*, four terminals connected with four tongues A, B, C, D.

E, F, G, H, four spiral springs, which press the tongues against a central spindle I.

f, *h*, two insulated metal arcs, which connect either *a* with *d* and *b* with *c*, or *a* with *b* and *c* with *d*, according to the position of the handle *k*.

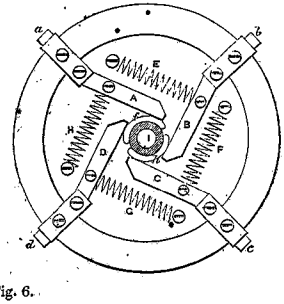


Fig. 6.

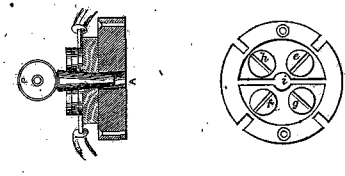
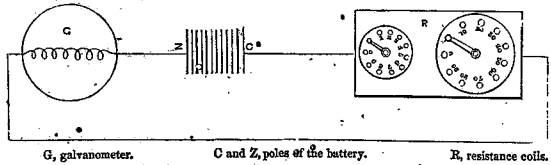
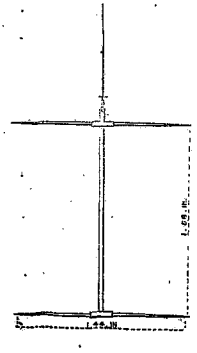


Fig. 7.



G, galvanometer. C and Z, poles of the battery. R, resistance coils.

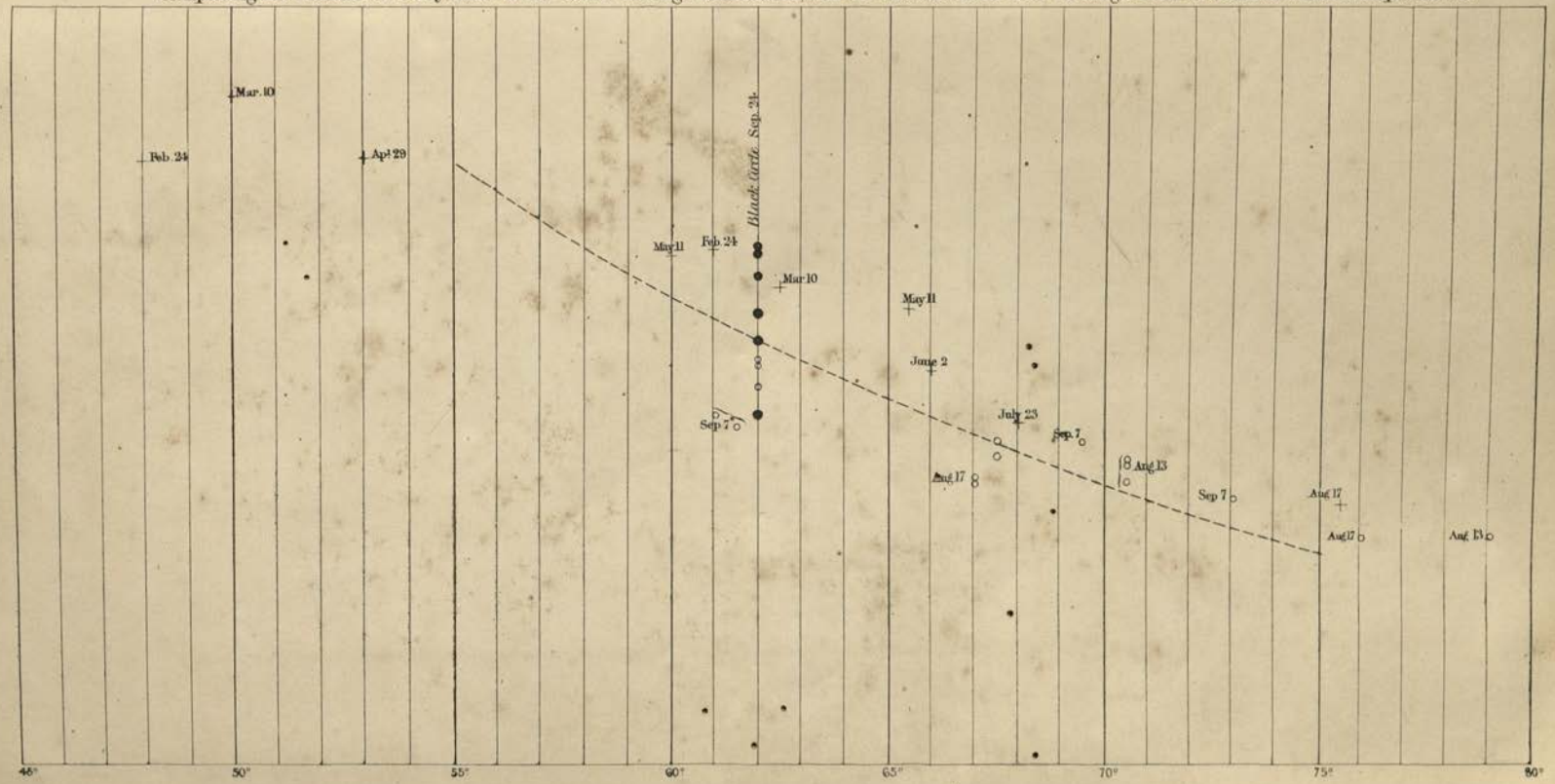
Fig. 8.



Pl. VIII

DIAGRAM

Comparing the Results of Daily Tests of the Cable during Manufacture, with the Results of the Tests on single Knot of Cable of various Temperatures.

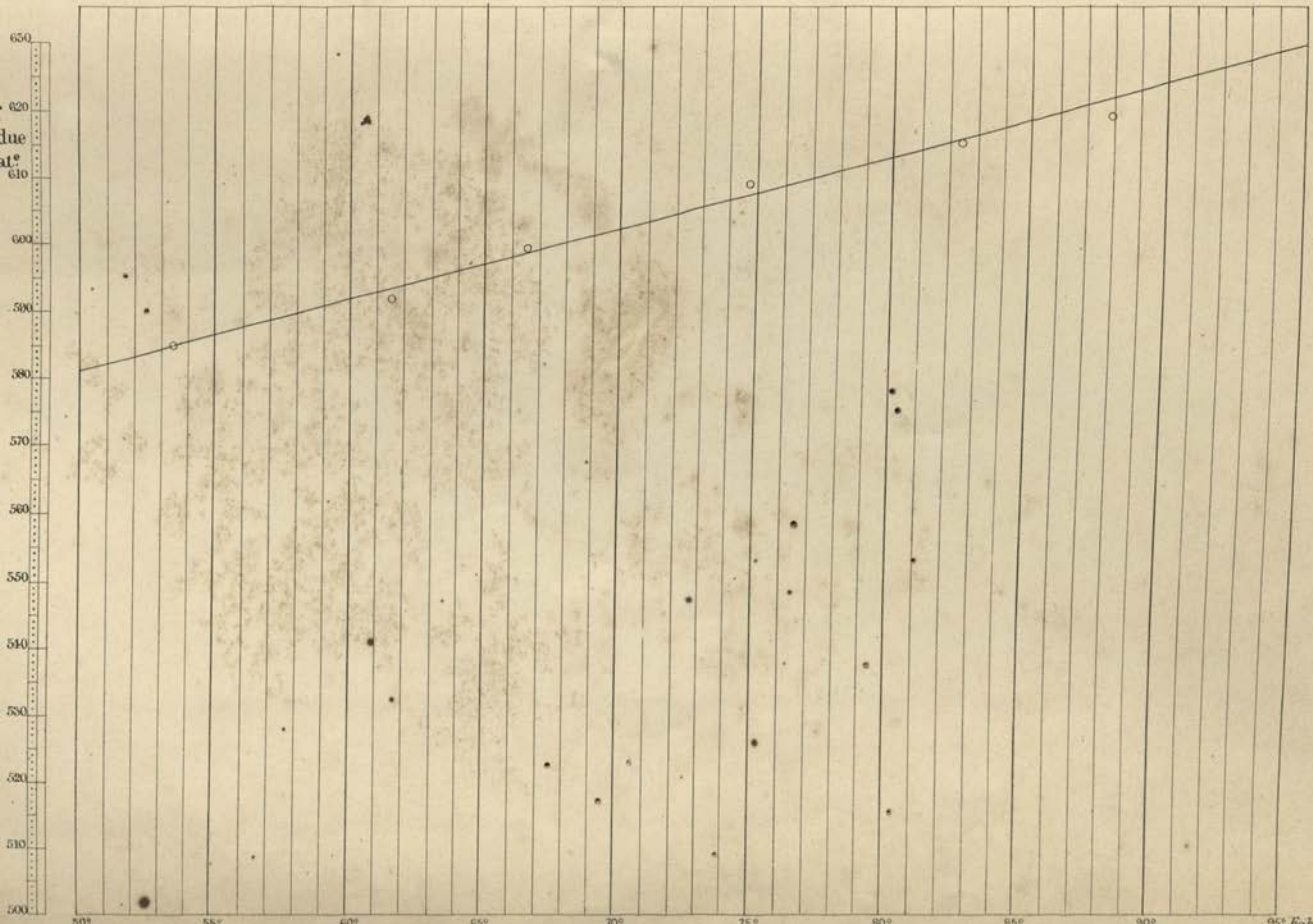


Day & Son. Lith^rs to the Queen

Vertical Scale 6 × 10^m = 1^m

DIAGRAM

Shewing the Increase of Resistance of Copper Wire, due to an Increase of Temperat.e

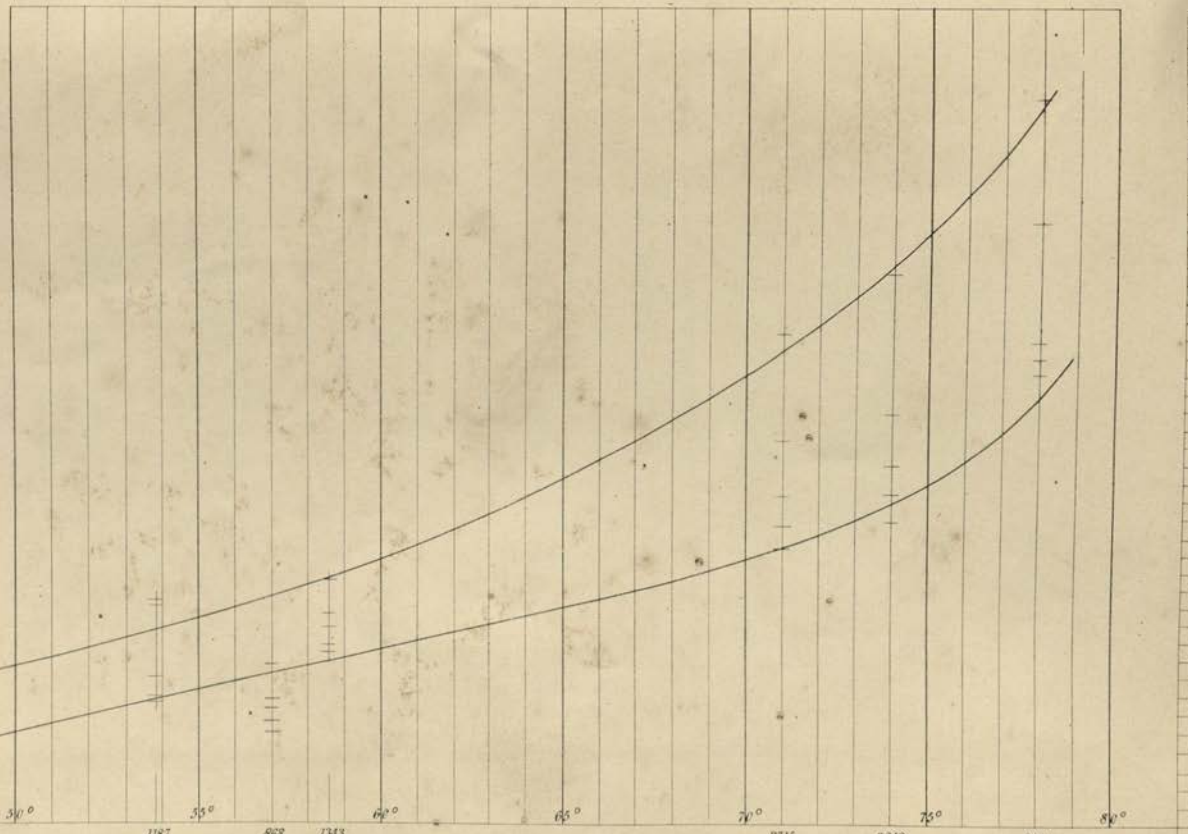


True Base Line 500 Div.ns below Base Line shewn

Plate VI.

COPPER TO COIL Nº 3.

RED SEA CORE.



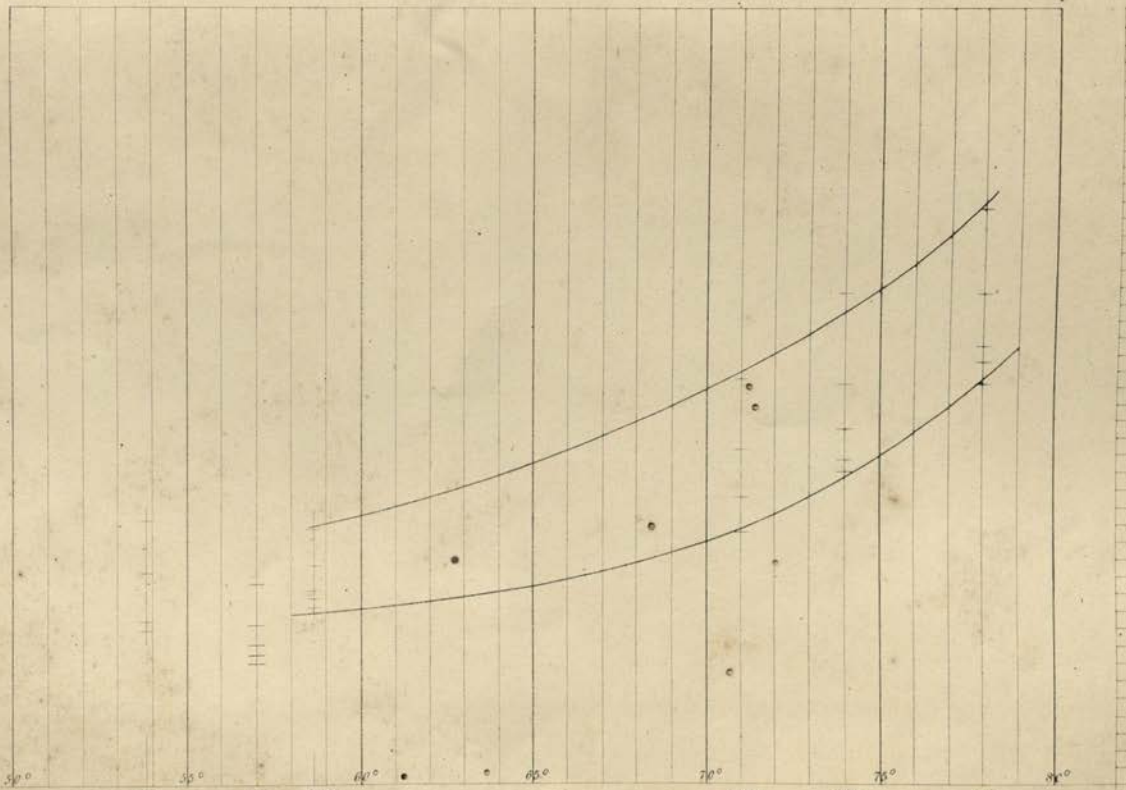
Vertical Scale. 800 parts = 1ft.

785	1187	868	1343	2715	3042	3894
684	1226	668	1149	2111	2256	3218
563	783	602	1070	1798	1966	2660
482	674	534	973	1631	1809	2584
463	655	491	934	1520	1654	2490

Plate V.

ZINC TO COIL Nº 3.

RED SEA CORE.

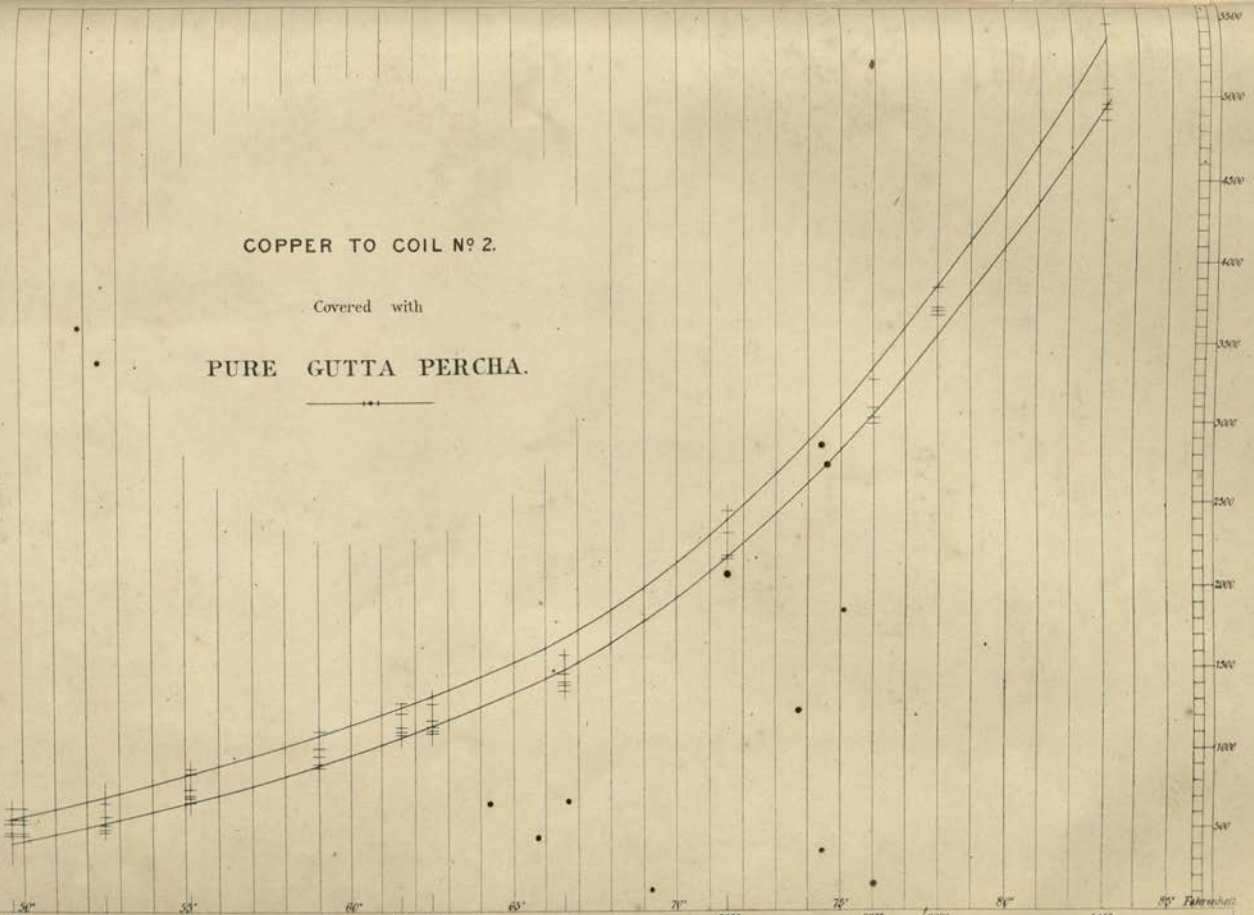


Vertical Scale. 800 parts 1in.

733	1314	1155	1468	2346	2845	3341
652	1202	911	1254	1923	2309	2843
512	928	801	1117	1647	2039	2524
452	869	734	1059	1443	1865	2430
432	811	696	1000	1387	1788	2298

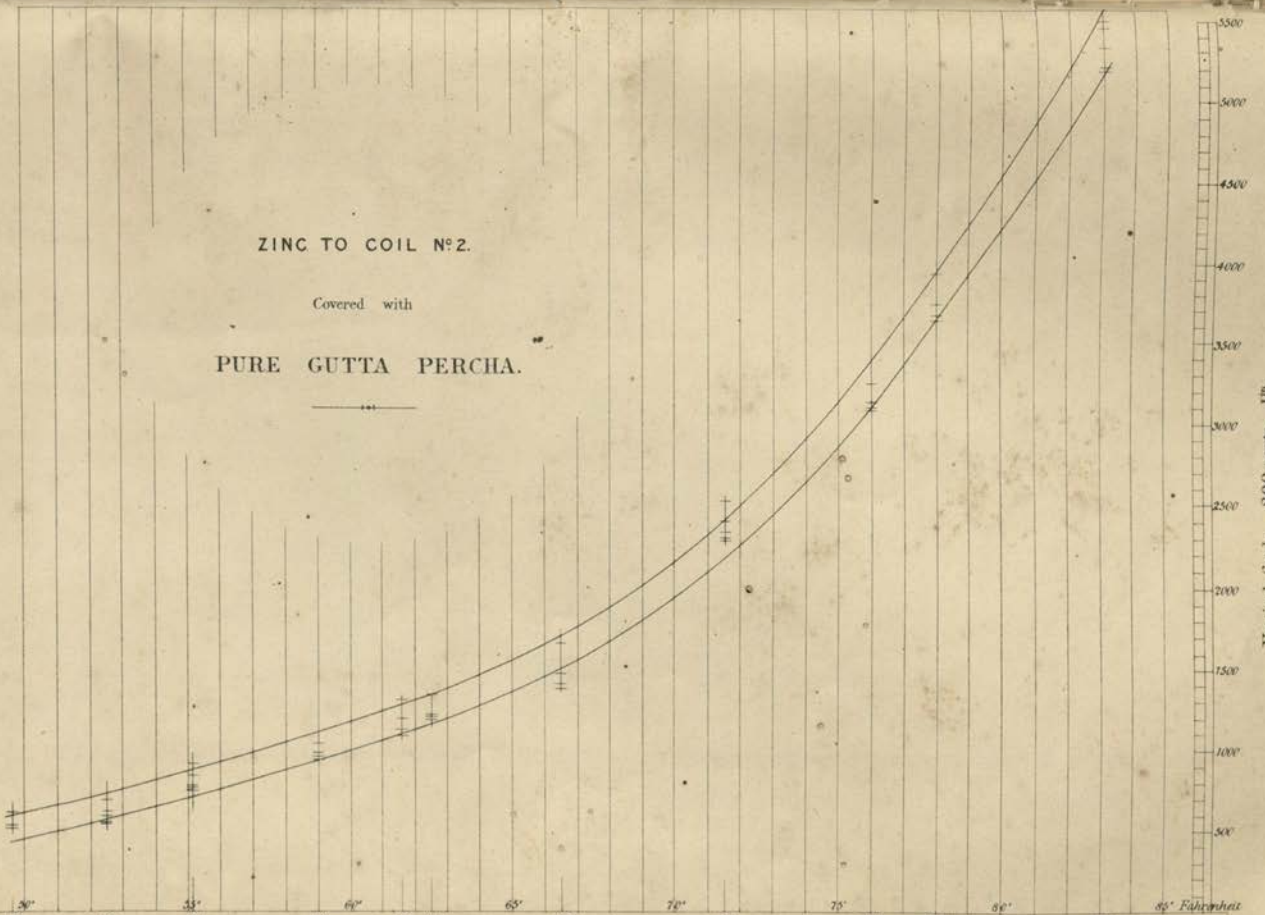
Day & Son, Lith. to the Queen.

Plate IV.



Loss after 1st minute	655	679	901	1126	1297	1285	1596	2866	3225	3821	5458
" " 2nd "	575	603	775	1036	1241	1194	1477	2393	3107	3712	5053
" " 3d "	555	547	740	982	1147	1140	1417	2293	3030	3684	5053
" " 4th "	496	527	721	929	1128	1123	1399	2150	3034	3659	4931
" " 5th "	470	509	703	910	1108	1106	1358	2086	2974	3659	4931

Plate III.

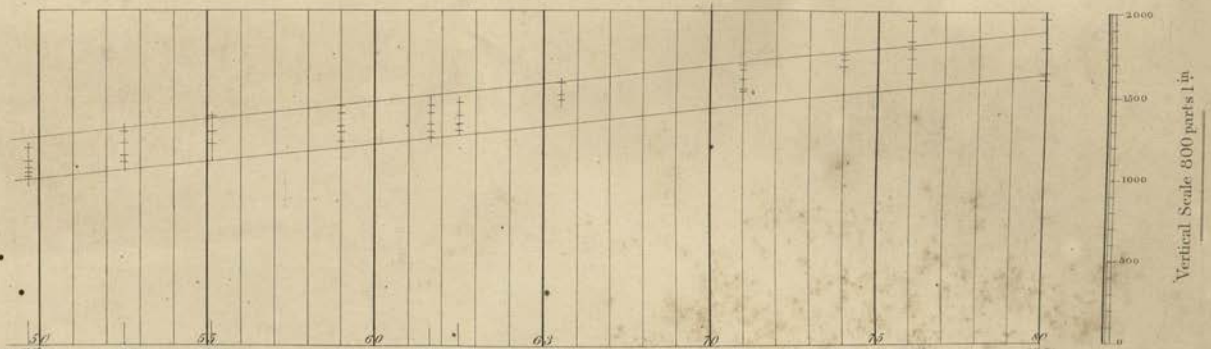


Loss after 1st minute	615	660	907	1078	1207	1398	1656	2551	3273	3945	5743
" " 2nd "	535	604	829	972	1166	1213	1451	2408	3162	3743	5475
" " 3d "	535	586	773	953	1110	1194	1398	2370	3125	3656	5331
" " 4th "	516	548	751	953	1072	1173	1372	2331	3107	3672	5203
" " 5th "	516	529	740	926	1110	1166	1378	2312	3107	3672	5186

Fig. 1.

ZINC TO COIL Nº 1

COVERED WITH GUTTA PERCHA AND CHATTERTON'S COMPOUND.

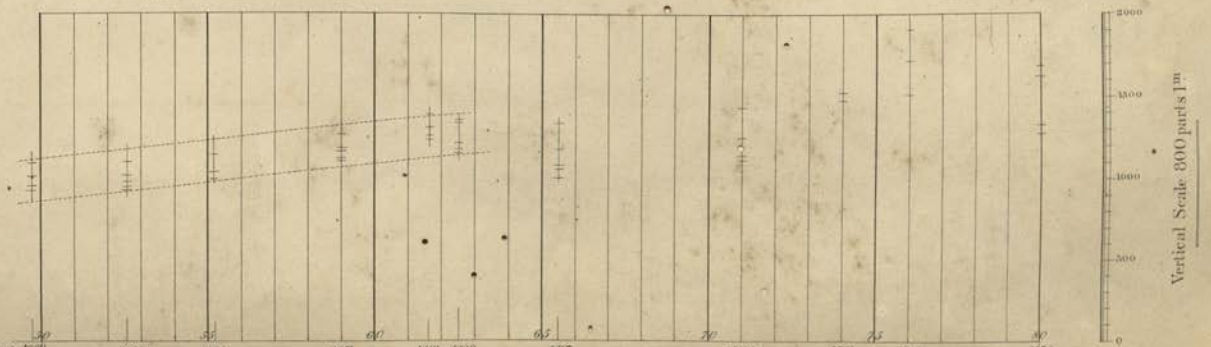


Loss after 1st minute	1169	1281	1367	1437	1428	1447	1575	1672	1751	1939	1952
" " 2nd "	1089	1205	1277	1383	1391	1375	1496	1653	1751	1825	1769
" " 3rd "	1051	1131	1277	1312	1316	1321	1477	1594	1712	1787	1639
" " 4th "	1011	1093	1188	1276	1279	1321	1477	1536	1675	1711	1602
" " 5th "	991	1037	1098	1222	1241	1285	1477	1516	1675	1624	1602

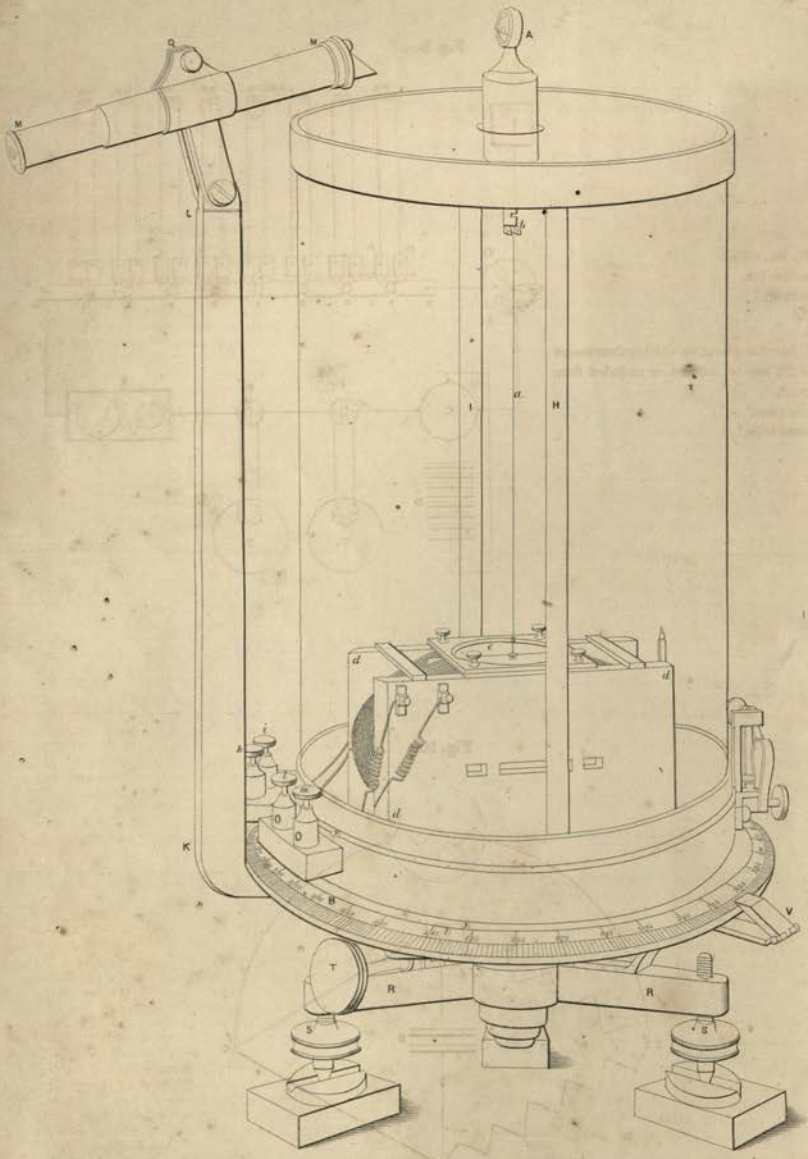
Fig. 2.

COPPER TO COIL Nº 1

COVERED WITH GUTTA PERCHA AND CHATTERTON'S COMPOUND.



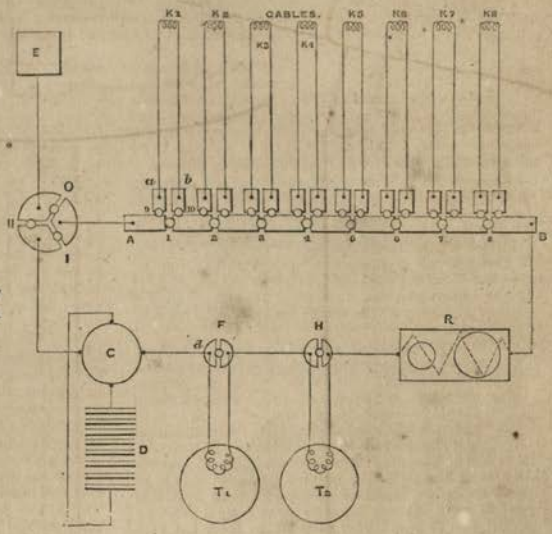
Loss after 1st minute	1089	1092	1223	1251	1391	1339	1317	1418	1521	1902	1678
" " 2nd "	1011	1017	1154	1143	1297	1251	1159	1241	1483	1712	1603
" " 3rd "	952	979	1027	1161	1241	1194	1059	1123	1483	1502	1603
" " 4th "	931	942	991	1107	1203	1159	1039	1105	1483	1502	1324
" " 5th "	931	923	991	1090	1203	1141	980	1105	1483	1465	1268



20088 Turns of wire in Coils.
Resistance of Coils in series 1889 x 10⁷ T.U.
Length of suspending thread 6 7/8 in.
Diam.r of external divided circle 8 1/4 in.
Diam.r of internal divided circle 7.16.

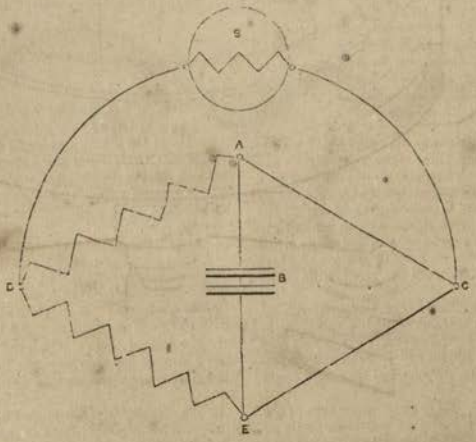
Day & Son, Lith.rs to the Queen.

Fig. 9.



K_1, K_2, K_3, &c., cables.
A, B, junction bar.
C, commutator.
D, battery.
E, earth.
F and H, junction pieces by which galvanometer T_1 and T_2 can be included or excluded from the circuit.
O, junction piece.
R, resistance coils.

Fig. 10.



App. No. 15.

Appendix No. 15.

Sir Charles Bright on the requirements of an electric cable of great length, and laid at a great depth.

Letter from Sir Charles Bright to Sir Stafford Northcote relative to the Requirements of an Electric Cable of Great Length and to be laid at a Great Depth.

72, Old Broad Street,
May 21, 1859.

Sir,

I have the honour to submit my opinion upon the points contained in the memorandum accompanying your letter of the 17th inst., relating to the requirements of a telegraphic cable, 1,100 nautical miles in length, to be submerged to depths varying from 100 to 2,500 fathoms.

Question No. 1. What is the form of electrical conductor best-suited for a telegraphic cable 1,100 nautical miles in length, submerged to depths varying from 100 to 2,500 fathoms? Solid wire or strand?

1. There are electrical advantages in the use of solid wire, but I consider the mechanical benefit gained by forming a conductor of several wires laid together in a strand to be of superior importance, and I therefore recommend that a strand of pure copper wire should be adopted for the conductor of your cable.

2. What thickness of conducting metal? 3. What speed a cable ought to work at with the conductor recommended? 4. What insulating medium should be employed, and up to what thickness should the conductor be insulated having especial reference to the retardation of the electric current in a circuit of the extent contemplated?

2, 3, and 4. The dimensions of the conductor, and the thickness of the insulating material by which it is to be covered are mainly dependent upon the speed at which it is desired to work through the cable when laid; I presume, however, that it is not your intention to incur the very great outlay necessary for the attainment of a rate of working equal to the speed which may be reached with short submarine lines, or wires suspended from posts, but that a moderate speed—say, of ten words per minute—would suffice for your purpose. For the accomplishment of such a rate of working I should advise that about three hundredweight and a half of copper should be used for the conductor to each nautical mile, which would make a seven-wire strand $\frac{3}{32}$ of an inch in diameter. The copper should be very carefully tested for conductivity during the manufacture of the cable, as very great variation has been found to exist in the capacity of different samples of copper wire of the same size and weight, and apparently equal in all respects.

The conductor should be covered to half an inch in diameter with the best gutta-percha, and the compound of gutta-percha, wood tar, and resin, now usually applied by the Gutta Percha Company in their layers immediately over, and between, the wires insulated for submarine cables, by which a more complete adhesion of the surfaces in contact is obtained.

Recommendation for the outer covering of a cable to be laid between Plymouth and Gibraltar.

The quantity of gutta-percha thus applied, (which should be laid in six separate coatings, three of the composition, and three of pure gutta-percha alternately) would amount to a little more by weight than the conductor, and would ensure an ample degree of insulation, if properly tested during the construction of the cable. With this object, in addition to the usual tests at the gutta-percha works, the cable when finished should be coiled in water, flake by flake, and there be tested with the greatest care until the completion of its delivery on board ship.

Such a conductor as the above, if submerged without injury could be worked well, without requiring an excessive amount of battery power, at the rate of ten words per minute, through the length which you have in contemplation; it would cost about 100*l.* per nautical mile, and could be made in ten weeks.

Special attention should be paid to the construction of the joints in the conductor, and a series of resistance coils of fine wire should be made before the departure of the cable exactly representing the resistance of various lengths of the cable, by which in the event of any defect or fracture hereafter, the position of the fault may be readily determined.

5. Whether it is desirable or practicable to add to, or assist the insulation of the conductor by any process in the application of the outer covering?

5. I do not know of any process that I could recommend by which the insulation of such a conductor as I have described could be improved to any useful extent; but the addition, to the outside of the insulator, of any non-absorbent material (although not as good an insulator as gutta-percha) would tend to reduce the degree of retardation proportionately to the thickness and insulating properties of the material so used. Nothing of the kind has been practically applied hitherto, and there are several points connected with its introduction which require further consideration and experiments.

6. What should be the form of the outer covering?

6. The form of outer covering which I should prefer would be governed by the depths to which the cable is to be laid and the nature of the bottom. Near the shore, and until the soft clay bottom which is generally found at the greater depths is reached, a strong covering of solid iron wire tapering by degrees in its advance towards the deep sea, would in my opinion be the best and most permanent mode of protecting the conductor.

In the greater depths a combination of iron and hemp might be adopted by which the specific gravity of the cable would be reduced, at the same time that sufficient strength, and protection for the conductor would be preserved.

If you have not already obtained so many soundings upon the route over which the cable will be laid as to indicate generally the depth and bottom, they should be procured before deciding upon the form and dimensions of the outer covering, and before the actual laying of the cable is commenced careful soundings should be taken, any sudden variation in depth being very carefully examined.

If you are in a position to give me the information in regard to various depths on the track, I shall be glad to lay before you my opinion as to the form of outer covering to be used, with specimens of the system which I may recommend.

The manufacture of the conductor and insulating material should not be delayed on account of the outer covering, if time is as important in the execution of this work as your letter leads me to suppose.

I have, &c.,
(Signed) Charles T. Bright.

Sir Stafford Northcote, Bart., M.P.,
&c., &c., &c.

72, Old Broad Street, E.C.,
June 9th, 1859.

Sir,

Since I addressed my report of the 21st ultimo to you, I have obtained such information regarding the general character of the soundings along the route of the proposed telegraphic cable from the Lizard to Gibraltar, as enables me now to lay before you my opinion upon the form of outer covering which I should recommend to be used in combination with the insulated conductor formerly described.

For the main portion of the line, where the depth is not more than about five hundred fathoms, I should advise that the outer covering should consist of fourteen No. 12 guage, best charcoal iron wires, covered separately with Manilla yarn well saturated with tar, oil, and beeswax, a serving of yarn should also be laid over the conductor.

In the Bay of Biscay, where the depth increases considerably, I should lay precisely the same form of cable, with wires of the same guage, but of steel, instead of charcoal wire.

In the Straits of Gibraltar, and near the English coast where there is any likelihood of injury from ship's anchors, a heavier cable composed of solid iron wires whould be laid, the core being thickly covered with tarred yarn to form a bedding for the outer wires; I should recommend the adoption of fourteen No. 2 guage iron wires as the outer covering for this part of the line, to be tapered gradually into the main cable after leaving anchorage ground.

I have, &c.,
(Signed) Charles T. Bright.

Sir Stafford Northcote, Bart., M.P.,
&c., &c., &c.

LETTER from Mr. J. A. LONGRIDGE to Captain GALTON on PAYING-OUT APPARATUS for SUBMARINE TELEGRAPH CABLES.

Captain Galton, R.E., Board of Trade.

DEAR SIR,

I SEND you herewith sketch of the paying-out apparatus I showed you this morning.

The object is twofold:

1st. To avoid the severe strains caused by the friction of heavy rotatory machinery when the vessel pitches.

2nd. To reduce *ad libitum* the injurious normal pressure of the cable upon the paying-out machine.

1st. This is accomplished in methods 1 and 2 by substituting a sliding motion of the cable over a surface, the friction of the cable being the retarding force instead of the friction of breaks, and in the third method by so arranging the cable over revolving drums that when the strain exceeds that of the break, the cable slips away, without increasing the velocity of rotation of the machinery. By any one of these methods the inertia of the machinery has no effect on the cable.

2nd. The normal pressure at any point is equal to the tension divided by the radius of curvature. If, now, we take a cable running out with a tension of 30 cwt. over a 5-feet drum, the normal pressure equals $\frac{30}{5}$ = 6 cwt. per lineal foot or 56 lbs. per lineal inch; an amount which, if the gutta percha be at all soft, would be very likely to injure the insulation; and suppose that, by a sudden pitch of the vessel, the inertia of the wheels caused an extra tension of 20 cwt., which is greatly within the limits of what might happen, this would bring an extra normal pressure on the cable of 38 lbs. per lineal inch, making altogether 56+38 =94 lbs. per lineal inch. Such a pressure, besides the injury to the core, would prevent the adoption of a system of sliding, because the friction would be so great as to tear off the entire coating.

Now the only means to reduce the normal pressure is to increase the radius of curvature, to which there is a practical limit if circular drums be used.

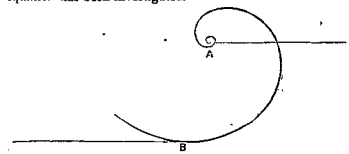
Again, with circular drums the normal pressure is greatest where the cable leaves the drum, and least where it enters on it; whereas it ought to be constant throughout. It is therefore evident, that by adopting a curve of variable radius we can obtain a constant pressure.

This is the principle of the methods 1 and 2.

App. No. 16. Paying-out apparatus.

METHOD 1.

The curve of constant pressure is a spiral of which the equation has been investigated.



Representing this spiral by the sketch in the margin, the cable would enter it at A and leave it at B, and

$$\frac{\text{the tension at A}}{\text{tension at B}} = \frac{\text{Radius of curve of A}}{\text{Radius of curve at B}}$$

Taking the radius of curvature of B = 16 feet and of A = 2 feet, the tension at A = ⅛th of that of B.

The length of curve required depends on the co-efficient of friction, and is of course greater as the latter is less.

The following table gives an example of the action of such a curve, of which the radii are 16 feet and 2 feet respectively.

Let T = tension at leaving the curve.
t = tension at entering the curve.
P = normal pressure on the lineal inch of cable on curve.
p = normal pressure on a 5-feet drum at leaving the drum.

T.	t.	P.	p.
Cwt.	Cwt.	Lbs.	Lbs.
32	4	19	120
24	3	14½	90
16	2	9½	60
8	1	4¾	30

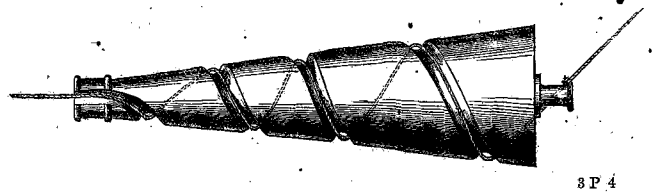
The next Table gives the length of the curves and their horizontal abscissæ (if developed as shown in Column 1) corresponding to various co-efficients of friction.

Co-efficient of Friction. f.	Length of Curve. L.	Horizontal Length. c d.
	Feet.	
½	84	56
⅔	70	47
⅝	64	42
½	42	28

The curve may for convenience be divided into sections, and placed either vertically or horizontally along the deck, and so arranged that the cable may be thrown off it in a moment. One or more of the sections may be so arranged as to indicate the tension of the cable.

METHOD 2.

The curve may be formed by a spiral groove cut in a cone as shown in Fig. 2. If this cone be rectangular the curve possesses the same properties as the curve of equal pressure: The cable enters at the small end of the cone, and makes one, two, or more turns round it, the number of turns depending on the amount of friction required. If the cone be free to revolve, the cable turns it round and runs itself free.



App. No. 14. Paying-out apparatus.

If, on the other hand, it be desired to increase the retarding force, the cone is turned in a reverse direction, and winds more cable on it.

The bearing of the cone is so arranged as to offer no impediment to the escape of the cable when required.

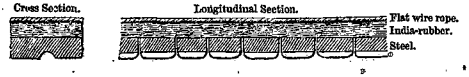
Thus, by this arrangement, the retarding force can be varied at will with the greatest ease.

The cone also takes up much less space than the curve. Its properties in reducing the normal pressure and tension, are similar to those exhibited in Table 1, given above, for the curve. It also affords great facilities for indicating the tension.

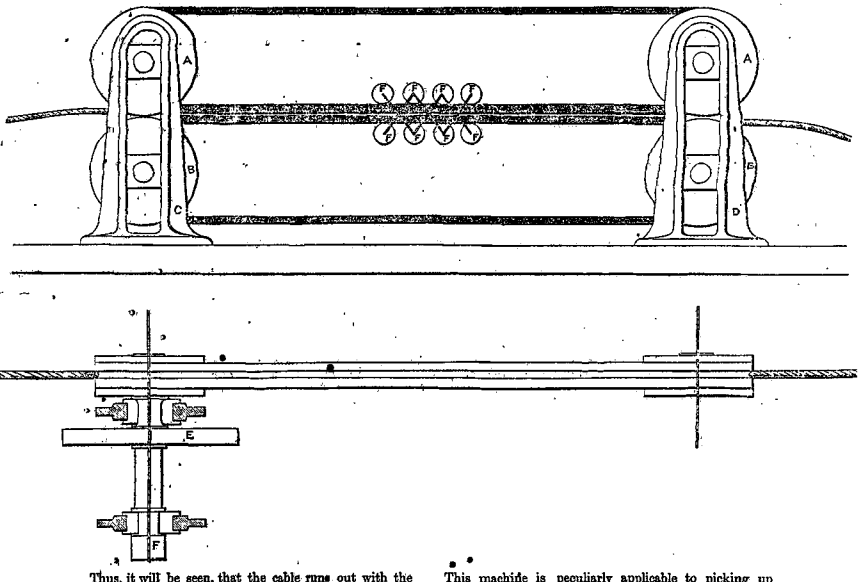
METHOD 3.

The cable is here passed between two endless belts passing round drums AA and BB. The standard C is fixed, whilst D is moveable so as to tighten up the belts when required.

The distance apart of the top and bottom drums can be varied at will. The endless belts are formed thus—



and are acted on by small rollers FFF, pressing on the back of each, so connected that the pressure of the two belts on the cable can be varied at will. The cable can be thrown off the machine and put on again in a moment. E is a break-wheel, and F the shaft-end which may be connected to a small steam-engine for hauling in.



Thus, it will be seen, that the cable runs out with the belts at a tension, regulated by the break-wheel, and the increased strain due to inertia is avoided by the cable slipping through the belts when the tension exceeds that due to the friction imposed by the rollers FFF, variable at will.

This machine is peculiarly applicable to picking up cables.

I remain, yours faithfully,

JAMES A. LONGRIDGE.

18, Abingdon Street, Westminster,
28th November 1859.

APPENDIX, No. 16.

App. No. 16.

ATLANTIC TELEGRAPH CABLE.

Atlantic telegraph.

Atlantic Telegraph Company, 22, Old Broad Street, Engineer's Department, August 19, 1858.

GENTLEMEN,

On arriving at Valentia, on the morning of the 5th instant, I forwarded to you by telegraph a brief report of the success which has attended the Company's endeavours to place Newfoundland in electrical communication with Ireland, and I have now the honour to lay before you fuller particulars of the operations carried out on board H.M.S. "*Agamemnon*," which I have been unable to do sooner, owing to the pressure consequent upon the return of the expedition.

Sir C. Bright's report on the successful paying-out of the cable.

After our departure from Queenstown, at two a.m. on the 18th ult., we proceeded towards the rendezvous, which we reached on the night of the 28th, having been delayed by contrary winds and a head swell. We found the "Niagara," "Valorous," and "Gorgon," which had left Queenstown on the 17th, waiting for us, and on the morning of the 29th, the sea being smooth, and the barometer standing at 30.15, the "Agamemnon" and "Niagara" were connected together by a hawser stern to stern; the end of the cable on board the latter ship was then brought by the boats of the "Valorous" to the "Agamemnon," where the splice was finished by one o'clock, local time, our position then being lat. 52° 8′ N., long. 32° 27′ W., distant 938$\frac{3}{10}$ statute, or 815 nautical miles, from the White Strand Bay at Valentia.

Having veered out a sufficient length to bring the splice into the centre of the curve formed by the cable hanging between the ships, the hawser was released, and we proceeded on our course slowly, paying out slack freely for the first three hours, after which the speed of the ship was increased to four, and at seven p.m. to five knots per hour.

All went on well until 7.45 p.m., when, immediately after passing from the outside to the centre of the coil in the main-hold, the beginning of the first turn of the flake next below that in process of delivery was seen (on being exposed by the uncoiling of the cable above it) to be squeezed between the side of the cone in the eye of the coil and the end of the piece of wood by which the leading-in part of the coil was defended.

This injury occurred through the extent to which the coil was disturbed during the gales encountered in our previous voyage. Although the whole of the upper part of the coil which had been displaced to such an extent as to promise any difficulty in paying out was removed, and coiled on the upper deck abaft the foremast, it would appear that all the new cable which had been lately placed on the top of the main coil had shifted somewhat in the heavy weather, for it was necessary to rectify another defect, arising from the same cause, at a similar part of the coil soon after.

The old cable, which had been coiled for a longer time, and was more thickly coated with the mixture of tar and pitch, was not in the least degree disturbed.

When the defective piece had been passed under some of the turns of the flake then paying out to the outside, in order to allow of more narrow examination than could be made in the centre of the coil where the cable was passing out or the hold, Professor Thomson reported that continuity had ceased.

On the cessation of signals I requested Captain Preedy to stop the ship, having placed Mr. Clifford to superintend the machine, so that as little cable might be payed out as was consistent with safety, Mr. Canning taking charge of the reinstatement of the injury, while Mr. Hoare attended to the dynamometer.

It is in great measure owing to the care of these gentlemen that no ill resulted from this critical mischance.

At 9.15 the fault was repaired, and shortly afterwards signals were again reported from the "Niagara." We had at this time payed out 46 nautical miles of cable from the "Agamemnon."

The depth of water at the time of this stoppage was 2,030 fathoms, according to the nearest sounding.

By noon on the 30th we had payed out 135.3 nautical miles, being then in lat. 52° 24′, long. 29° 59′, by observation, and 718 miles distant from Valentia, the "Niagara" having laid 130 miles of cable.

After this the wind freshened, and a heavy swell got up, increasing the motion of the ship very much, and at midnight it was blowing hard from S.S.E., the consumption of coal required to keep up the speed which I desired to maintain being so great that some apprehension was felt in regard to the sufficiency of our supply of fuel.

At noon on the 31st the "Agamemnon" had payed out 280 miles, and the "Niagara" 285.

The weather did not allow of any observation, but our run, by dead reckoning, made us about 605 miles from Valentia, and in the locality where the depth of 2,400 fathoms (the greatest in our route) was obtained by Captain Dayman, in H.M.S. "Cyclops," last year.

During this day the wind continued to blow heavily, the sea running very high; by midnight the barometer had fallen to 29.50, and everything indicated a change for worse rather than for better weather; we had then payed out 358 miles of cable, the "Niagara" 365.

At noon on Sunday, August 1st, we were 478$\frac{4}{10}$ miles from Valentia, our position by observation being lat. 52° 26′ 30″, long. 23° 16′ 30″, 434 miles having been paid out from the "Agamemnon," and 440 by the "Niagara."

During the morning the wind had changed to the S.W., and the weather gave signs of amendment, but a heavy swell remained, and in the afternoon the breeze freshened, squalls followed each other in rapid succession, and the ship pitched as much as before.

By noon on the 2nd we were in lat. 52° 35′, long. 19° 48′, 351·6 miles from Valentia, 605 miles of cable having been laid from the "Agamemnon," and 615 from the "Niagara."

In the afternoon the force of the wind decreased, and the motion of the ship was much easier: at three p.m. we had to alter our course for a few minutes to avoid a three-masted schooner, which passed us on the port bow so closely as to make it a subject for congratulation that she did not cross our path astern; the cable grew out very much to the starboard side during the change, but I caused an additional amount of slack to be paid out at the time, so that no undue strain came upon it.

During the evening the weather was squally, and by four in the morning of the 3rd the wind had got round to the N.W., and a long slow swell from the S.W. caused the ship to pitch and roll as much as before. At this time some excitement was created by a barque bearing down upon our starboard beam; we increased speed to clear her, but she hove to on being intercepted by the "Valorous."

At noon, on the 3rd, we had payed out 776 miles of cable, being then in lat. 52° 26′, long. 16° 7′ 40″, 211·2 miles from Valentia, the "Niagara" having laid 780 miles.

After this the depth of water, which had averaged 2,000 fathoms since the 1st instant, began to lessen, and at 5 p.m. the greatest variation in our tract (from 1,750 to 550 fathoms within about ten miles) occurred, an extra per-centage of slack being laid to provide for any irregularities which might there exist on the bottom; by midnight the depth had further decreased to 216 fathoms.

App. No. 16.
Atlantic telegraph.
Sir C. Bright's report on the successful paying-out of the cable.

At 4 a.m. on the morning of the 4th, the larger coil in the main hold was exhausted, and we commenced paying out from the upper deck coil.

By noon the water had deepened again to 400 fathoms. We were then in lat. 52° 11′, long. 12° 40′, only 89½ miles from Valentia, having laid 924 miles of cable, while the "Niagara" had laid 925.

During the day the wind and sea dropped, and at 8 p.m., having reduced our distance from Valentia to 50 miles, the "Valorous" steamed a head, to make out the land.

The water now shoaled gradually; at 8·30 p.m., having finished the second coil, a change was effected to the cable on the orlop deck.

At midnight we were in company with the "Valorous," in sight of the upper Skellig light, and at dawn, on the morning of the 5th, abreast of the Blasquets, steaming slowly towards Valentia.

At 6 a.m. we anchored in Doulas Bay, 2,022 nautical miles, having been paid out between the two ships, and proceeded to coil a sufficient length of cable to reach the shore into one of the paddle-box boats of the "Valorous."

The wind freshened in the course of the morning, by which the landing of the end was somewhat delayed, the swell becoming so great that Captain Preedy got up steam in the "Agamemnon," ready to put out to sea at any moment.

At 3 p.m. the end of the cable was safely brought to the beach, and passed into the company's station at Knightstown.

The strain upon the cable varied during the paying out under different circumstances of weather, depth of water, and speed of ship, as will be seen by the accompanying tabular log, which furnish details recorded several times in each hour, of the indicated strain, weight on brakes, angle of cable, rate of paying out, rate of ship, revolutions of screw, distance run according to Massey's log, distance made good by observations, and a journal of all the events worthy of note in each watch; an entry is also made of Greenwich time, so that the electrician's diary, and the log kept on board the "Niagara" may be more readily compared with it.

Some inconvenience was experienced by the great accumulation of pitch and tar, a second coating of which was laid on the cable when coiled away at Keyham for the winter, to prevent it from rusting; but this had also its advantage in keeping down the cable leading from the coil, which had, if too dry in any place, a tendency to fly out when running at a high speed.

The paying out machinery (consisting of the addition of Mr. Appold's brake, to one of the two machines fitted on board each ship last year, as recommended by your committee, with the dynamometer for indicating the strain) has worked exceedingly well, in a manner which reflects the highest credit upon the manufacturers, Messrs. Easton and Amos.

The hand wheel for lifting the weights when required, designed by Mr. Amos, was of considerable service during the unfavourable weather which prevailed for the chief part of the voyage.

The amount of slack payed out amounted to 22 per cent. upon the distance run; less might have been laid, but I considered it desirable, to ensure the cable laying everywhere on the bottom, that ample slack should be used to cover any irregularities within the bounds of probability.

I must not conclude this Report without again expressing my deep sense and appreciation of the laborious zeal and untiring patience exhibited by Captain Preedy and the officers and company of the "Agamemnon," nor can I too strongly express my obligation to Mr. Canning and Mr. Clifford, who so ably took part with me in the general superintendence of the work, and to Mr. Hoare and Mr. Moore, whose supervision of the dynamometer and machinery was of the utmost value to us; and it must not be forgotten that Captain Hudson, and the officers and crew of the "Niagara," with Mr. Everett and Mr. Woodhouse, who had charge of the operation of paying out from the "Niagara," with the assistance of Mr. Kell, have also performed their share of the labour equally with those who have returned to Ireland in the "Agamemnon."

I have the honour to remain,
Your most obedient Servant,
CHARLES T. BRIGHT,
Engineer.

To the Directors of
The Atlantic Telegraph Company.

App. No. 16.
Atlantic telegraph.
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I have the honour to remain,
Your most obedient Servant,
CHARLES T. BRIGHT,
Engineer.

To the Directors of
The Atlantic Telegraph Company.

APPENDIX No. 16—*continued.*

Log of the successful PAYING-OUT the ATLANTIC CABLE on Board the AGAMEMNON, from the 29th July to the 5th August 1858.

Appended to Report from Sir Charles Bright to the Directors of the Atlantic Telegraph.

DATE.	Greenwich Time.	Ship's Time.	Ship's Speed by Log.	Cable payed out.	No. of Revolutions of Machine per Minute.	No. of Revolutions of Screw per Minute.	Weight in lbs. upon Paddle.	Indicated Strain. Lowest.	Indicated Strain. Highest.	Angle of Cable. Vertical.	Angle of Cable. Horizontal.	Flakes payed out of Cab.	Splices and Lapplugs.	Distance by Massey's Log.	Distance made good at Noon.	REMARKS.
	H. M.	H. M.	Knots.	Mls. Fths.												H. M.
July 29 -	3 16		Splice lowered.	Commenced paying out, starting.			200	fathoms before				—	—	—	—	3.28. Engines started after paying out 70 fathoms; brakes not yet set up.
	3 30	—	1·6	1 —	30	12	1866	—	—	47	15	—	—	—	—	1.30 a.t. Increased ship two revolutions of screw
	4 0	—	1·7	3 506	30	14	1866	—	1200	25	8	—	—	—	—	1.33 a.t. Speed ship, 1·7.
	4 30	—	2·8	6 350	38	20	—	1200	1600	25	8	—	—	—	—	4.12 a.t. Increased number of revolutions of screw to 18.
	5 0	2.45	3·0	10 —	38	20	1266	1300	1700	22	12	—	—	—	—	4.18. A whale passed close to cable.
	5 30	3 15	3·0	13 —	39	20	1666	1300	1800	23	8	—	—	—	—	4.20. Increased number of revolutions of screw to 20.
	6 15	4 0	—	16 800	32	20	1666	1300	1700	23	5	—	—	—	—	4.30. Reduced weight on brakes to 1266; ship going 2·7, 5 p.m.; increased weight on brakes to 1666. CHARLES T. BRIGHT.
	6 30	—	—	17 800	32	22	,,	—	—	—	—	—	—	—	—	6.20 a.t. Increased speed of engines 2 revolutions, 10 moveable weights being on brake levers.
	6 45	4.30	3·0	19 300	34	22	,,	1300	1650	22	10	—	—	—	—	7.5 a.t. Increased speed of engines 2 revolutions.
	7 5	4 50	3·4	21 —	34	24	,,	1300	1700	—	—	—	—	—	—	7.15 a.t. Ditto ditto ditto.
	7 30	5 15	3·9	24 500	40	26	1866	1300	1700	17½	5	—	—	—	—	7.29 a.t. Ditto ditto ditto.
	8 0	5 35.	4·8	27 400	40	28	,,	1400	1800	15	3	—	—	—	—	8.10 a.t. Ditto ditto ditto.
	8 20	6 0	4·5	30 —	38	30	,,	1500	1850	15	3	—	—	—	—	During this watch cable paying out easily; strain steady. S. CANNING.
	8 45	6.30	4·5	33 500	38	30	,,	1400	1800	15	3	—	—	—	—	8.55 a.t. Increased weights to 2066 (14 moveable + fixed weights and levers = 666 lbs.); cable going out 7 knots, ship 4·4.
	9 0	6 45	4·5	34 400	39	30	2066	1550	1950	15	3	—	—	—	—	9.30 a.t. Speed of screw increased two revolutions.
	9 20	7 0	4·6	36 600	40	32	,,	1550	2000	14	3	—	—	—	—	10.5 a.t. Four weights taken off brakes and engine stopped to allow lapping to be made to cable in main hold. Want of continuity reported from electrical-room.
	9 50	8 0	4·6	39 700	40	32	,,	1500	1900	14	3	—	—	25½	—	8 p.m., s.t. Massey's log, 25¾ miles. Ship stopped, veering out cable slowly, and making good pricked and out places opened for testing in mainhold. Insulation reported good; continuity bad.
	10 15	8 25	—	42 —	25	—	,,	less than	1200	—	—	—	—	—	—	11.35. Signals reported from Niagara again:
	10 30	8 40	—	43 —	21	—	,,	,,	,,	,,	58	—	—	—	—	CHARLES T. BRIGHT.
	10 47	8 57	—	43 750	14	—	2666	1900	2700	,,	—	—	—	—	—	The splice passed over the drums and was paid overboard, with a considerable length of cable after it, under very small strain. An augmented strain was put on to retard the progress and diminish further loss before signals came from Niagara. W. THOMSON.
	11 5	9 15	1·2	44 125	8	—	2666	1900	2000	—	—	—	—	—	—	11.35 Signals reported from Niagara again.
	11 13	9 8	—	44 340	—	—	3066	1900	3600	—	—	—	—	—	—	11.90 p.m. Ship going on again. Lapping on cable passed over drums; strain stable.
	11 16	9 10	—	44 380	8	—	3466	,,	3700	—	—	—	—	—	—	CHARLES T. BRIGHT.
	11 20	9 15	—	44 400	35	—	2266	less than	1200	—	—	—	—	—	—	11.50. Engines increased to 22 revolutions.
	11 25	9 20	—	45 —	40	—	,,	,,	,,	—	—	—	—	—	—	JULY 30,
	11 33	9 30	—	46 —	36	—	,,	1300	1900	60	—	—	—	—	—	12.5 a.m. Engines increased to 24 revolutions.
	11 45	9 40	—	46 700	36	—	1666	1300	1700	—	—	—	—	—	—	12.26 a.m. Ditto 26
	11 55	—	—	47 930	41	—	,,	,,	,,	—	—	—	—	—	—	During this watch there has been much variation in the speed of the ship, which accounts for the great speed of cable payed out at times. Otherwise everything has gone on satisfactorily. Roll of ship five degrees each way. S. CANNING.
July 30 -	12 12	10 2	1·2	50 —	40	28	—	1400	1700	20	—	—	—	—	—	2.48 a.t. Increased speed of ship to 30 revolutions. Slowed ship four revolutions, a place in the hold requiring lapping.
	12 30	10 40	3·5	52 100	40	30	2066	1500	1900	15	5	—	—	—	—	4.50 a.t. 70 miles payed out from Niagara.
	12 40	10 50	4·0	53 306	48	32	,,	1600	1800	14	—	—	—	—	—	6.0 a.m. Speed of engines increased two revolutions in consequence of head breeze. During this watch all has gone well; one bad place in hold occasioned by working of ship during calm, made good at 1.30. CHARLES T. BRIGHT.
	12 50	11 0	5·5	53 800	44	,,	,,	1600	1900	—	—	—	—	—	—	6.45. Niagara signal of 80 miles paid out.
	12 55	11 5	—	53 400	44	36	,,	,,	,,	—	—	—	—	—	—	H. CLIFFORD.
	1 15	11 5	3·8	57 900	41	36	2266	1700	2000	14	—	—	—	—	—	6.50. Signal made to Niagara that we have payed out 90 miles. H. CLIFFORD.
	1 25	11 25	—	60 —	40	28	,,	1800	1900	—	—	—	—	—	—	7.20 a.t. Roll of ship four degrees each way.
	1 55	11 45	—	61 840	38	28	,,	1800	2000	16	—	—	—	—	—	7.40 a.t. Increased speed of ship two revolutions, making 34.
	2 10	12 0	3·8	62 300	36	28	,,	,,	,,	—	—	—	—	—	—	8.25 a.t. Niagara signalled 90 miles payed out.
	2 25	12 15	—	65 600	36	,,	,,	1900	2000	,,	—	—	—	—	—	8.50 a.t. Signalled to Niagara 100 miles payed out.
	2 40	12 30	3·5	67 700	34	30	,,	1700	1900	17½	—	—	—	—	—	8.50 a.t. Splice between old and new cable payed out. Eased strain on cable for five minutes.
	3 4	12 44	3·7	68 900	35	,,	,,	,,	,,	,,	—	—	—	—	—	8.55 a.t. Increased speed of engines to 26 revolutions.
	3 15	1 0	4·2	70 —	38	,,	,,	,,	,,	,,	—	—	—	—	—	10.4 a.t. Signalled Niagara 110 miles of cable payed out. During this watch everything has gone on satisfactorily. S. CANNING.
	3 20	1 30	—	72 400	33	,,	,,	,,	,,	,,	—	—	—	—	—	11.20 a.t. Increased speed of ship two revolutions, the head breeze freshening, and the cable growing to windward.
	3 45	1 36	—	73 —	30	26	,,	1700	1950	,,	—	—	—	—	—	11.30. Niagara. Signal of 110 miles.
	4 10	2 0	3·6	74 770	32	30	,,	1700	1900	17½	—	—	—	—	—	11.50. Valorous signals that she is going five knots.
	4 40	2 30	4·0	77 700	35	30	,,	1700	1850	15	3	—	—	—	—	12.25. Cable growing a good deal to starboard side, probably due to stern-gear.
	5 10	3 0	4·2	80 700	36	,,	,,	1700	1800	15	2	—	—	—	—	1 p.m. Temperature of water of brakes 100 deg.
	5 45	3.30	3·6	83 —	35	,,	,,	1700	1900	16	—	—	—	—	—	120 miles payed out from Niagara. CHARLES T. BRIGHT.
	6 17	4 0	3·4	87 —	30	32	,,	1700	2000	16	—	—	—	—	—	All going well during watch from 8 to 12. CHARLES T. BRIGHT.
	6 45	4 25	4·3	89 700	34	,,	,,	1600	1900	16	—	—	—	—	—	2.30 a.t. 130 miles cable payed out from Niagara.
	7 50	4 35	4·4	90 —	36	,,	,,	,,	,,	,,	—	—	—	—	—	4.7 p.m. a.t. 140 miles do. do.
	7 20	5 0	4·1	93 200	36	34	,,	1600	1900	16	1	—	—	—	—	4.36 a.t. 150 miles of cable payed out.
	7 50	5 30	4·5	96 —	36	,,	,,	1600	1900	16	½	—	—	—	—	5.37 a.t. 160 miles of cable payed out from the Niagara.
	8 20	6 20	4·5	99 200	36	,,	,,	1900	1900	16	,,	—	—	—	—	During this watch all has gone well. S. CANNING.
	8 40	6 30	4·4	101. 250	36	,,	,,	1500	1900	12½	4	—	Splice	—	—	4.40. Increased speed of ship two revolutions, but reduced again to 30 per min., as the consumption of coal would be disproportionately added to by running 40.
	9 20	7 0	4·6	105 500	38	36	,,	1500	1950	12	2	—	—	—	—	6.40. Niagara signalled 160 miles cable payed out.
	9 40	7 30	4·8	108 —	36	36	,,	1500	1900	12	4	—	—	—	—	5.45 a.t. Increased speed of ship to 40. Breeze from E. freshening. All going well during the watch from 4 to 8. Strong head breeze prevailing. Barometer falling. CHARLES T. BRIGHT.
	10 10	8 0	4·8	110 700	37	36	,,	1600	1900	12	4	—	—	74	—	8.5 a.t. Niagara signalled 170 miles cable payed out.
	10 50	8 39	,,	113 700	36	,,	,,	,,	,,	11	4	—	—	—	—	
	11 20	9 0	,,	116 850	38	,,	,,	,,	,,	,,	5	—	—	—	—	
	11 38	9 15	,,	118 450	39	38	,,	,,	,,	,,	,,	—	—	—	—	
	11 50	9 30	,,	120 —	40	,,	,,	1500	,,	,,	4½	—	—	—	—	
	12 5	9 45	,,	121 700	37	,,	,,	,,	2000	12½	,,	—	—	—	—	
	12 20	10 0	,,	123 300	,,	,,	,,	1600	,,	,,	,,	—	—	—	—	
	12 35	10 15	,,	124 800	39	,,	,,	,,	1900	15	—	—	Lapplings.	—	—	
	12 50	10 39	,,	126 506	38	,,	,,	,,	,,	,,	4½	—	—	—	—	
	1 5	10-45	,,	128 150	39	,,	,,	,,	1600	1900	,,	—	—	—	—	
	1 20	11 0	,,	129 600	38	,,	2266	1500	2000	,,	3	—	—	—	—	
	2 35	11 15	,,	131 000	40	,,	,,	1600	1900	,,	,,	—	—	—	—	
	1 50	11 30	,,	133 —	39	,,	,,	,,	,,	,,	5	—	—	—	—	
Noon.		12. 0	4·8	135 800	38	38	2266	1500	2000	12½	5	174	—	94·7	98	
	Fri. 2 35	12 35	4·2	138 —	36	36	,,	1600	1900	15	7	—	—	—	—	

App. No. 16.

APPENDIX No. 16—*continued.*

Log of the successful Paying-out the Atlantic Cable on Board the Agamemnon—*continued.*

Date.	Greenwich Time.	Ship's Time.	Ship's Speed by Log.	Cable payed out.	No. of Revolutions of Paying-out Machine per Minute.	No. of Revolutions of Screw per Minute.	Weight in lbs. upon Dynamo.	Indicated Strain. Lowest.	Indicated Strain. Highest.	Angle of Cable. Vertical.	Angle of Cable. Horizontal.	Flakes payed out of Coil.	Splices and Lappings.	Distance by Massey's Log.	Distance made good at Noon.	Remarks.
	H. M.	H. M.	knots.	Mls. Fths.												H. M.
July 30.	3 7	1 0	4·4	141 800	34	38	2266	1500	1900	13	7	—	—	—	—	7.35 a.r. 180 miles cable payed out.
	3 37	1 30	4·5	144 —	37	,,	,,	,,	,,	,,	10	—	—	—	—	9.45 a.r. Niagara payed out 189 miles.
	4 7	2 0		147 —	36	,,	,,	,,	,,	15	8	—	—	—	—	All has gone on well during this watch. Weather same as last. S. Canning.
	4 37	2 30	4·4	150 400	36	38	,,	,,	,,	15	8	—	—	—	—	10.45. p.m. G.T. Wind and sea getting up increased motion of ship.
	5 7	3 0	4·5	153 —	34	,,	,,	,,	,,	13	2	—	—	—	—	10.50. Squall with rain.
	5 37	3 30	,,	156 —	,,	,,	,,	,,	,,	,,	,,	—	—	—	—	11.10. Reduced weight on brakes to 2,666 lbs. (14 moveable and 4 fixed).
	6 7	4 0	4·2	158 —	33	,,	,,	,,	,,	,,	,,	—	—	—	—	11.30. 150 miles payed out from Niagara.
	6 37	4 30	4·4	163 —	34	,,	,,	,,	,,	,,	,,	—	—	—	—	11.40. Reduced weight on brakes to 1,866.
	7 20	5 0	4·4	166 —	33	,,	,,	,,	,,	13	3	—	—	—	—	12.0. Increased to 2,066, speed of machine having risen to 38. Ship going 4 knots.
	7 40	5 30	4·3	169 —	35	,,	,,	,,	,,	,,	,,	—	—	—	—	
	8 5	6 0	4·2	171 800	34	40	,,	1400	1900	,,	,,	—	—	—	—	JULY 31.
	8 35	6 30	4·6	174 800	37	,,	,,	1500	1900	12½	1	—	—	—	—	12.40. 200 miles payed out from Niagara.
	9 5	7 0	4·6	177 800	37	,,	,,	,,	,,	12½	3	—	Splice completed	—	—	Squall with rain. Ship pitching a good deal.
	9 45	7 30	4·7	179 600	37	,,	,,	,,	,,	12	2¾	24		—	—	1.30 a.m. G.T. Blowing strong.
	10 5	8 0	4·7	183 —	37	,,	,,	,,	,,	,,	,,	—	—	—	—	All well during the watch from 8 to 12. Weather squally with rain. Charles T. Bright.
	10 35	8 30	4·8	186 —	35	,,	,,	300	2000	15	3	—	—	40¾ from noon.	—	2.35 a.m. G.T. Ship pitching; drums nearly stopped eased weights on brakes from 2666 to 1666.
	11 5	9 0	4·5	188 630	33	,,	2066	1200	1800	15	4	—	—	—	—	2.30 a.m. G.T. 210 miles payed out from Niagara.
	11 35	9 30	4·6	191 300	34	,,	1200	1700	,,	,,	,,	—	—	—	—	4 a.m. G.T. Speed of engines increased to 36 revolutions; hands stationed helping brake wheels round.
	11 50	9 46	—	192 800	36	,,	1866	,,	,,	—	—	—	—	—	—	3.30 a.m. G.T. Increased the weight to 1866 lbs.
																3.45 a.m. G.T. 220 miles payed out from Niagara.
																4.40. Increased speed of engines to 45.
July 31.	12 5	10 0	4·4	194 250	35	,,	2066	,,	,,	15	,,	—	—	—	—	4.50. Engines going full speed.
Midnight.	12 20	10 15	,,	195 650	34	,,	,,	,,	,,	,,	,,	—	—	—	—	6.20 G.T. Blowing nearly a gale of wind; barometer, 19 deg. 30 min. and still falling; considerable sea on; fore and main trysails set to help engines. H. Clifford.
	12 35	10 30	4·6	197 300	38	,,	,,	,,	1800	,,	,,	—	—	—	—	
	12 50	10 45	—	198 750	36	,,	,,	,,	1950	,,	,,	—	—	—	—	5.20 G.T. Niagara signalled 230 miles cable payed out; during this watch there has been a strong head wind with rain & ship pitching heavily; speed of cable very variable in consequence; heavy sea on; all right at termination of this watch from 12 to 4 a.m. S. Canning.
	1 5	11 0	4·6	200 450	35	,,	,,	,,	2100	,,	,,	—	—	—	—	
	1 20	11 15	—	201 600	,,	,,	,,	,,	2000	—	—	—	—	—	—	6.45 a.m. Niagara payed out 240 miles.
	1 35	11 30	3·8	203 —	32	,,	1300	1900	,,	,,	,,	—	—	—	—	6.50. Took in trysails to keep ship better on her course, angle of cable having increased to the starboard side.
	1 50	11 45	—	204 550	33	,,	1200	,,	,,	—	—	—	—	—	—	
	2 5	12 0	3·5	205 900	34	,,	,,	2000	16	7	—	—	—	—	—	7.35. Ship pitching very much indeed; drums stopped when all the weights were lifted by the steering wheel; heavy sea.
	2 35	12 30	3·5	208 600	28	34	1666	1200	1600	17	7	—	—	—	—	
	3 5	1 0	—	210 250	34	38	,,	,,	,,	,,	,,	—	—	—	—	8.19. Ship pitching very much indeed; drums stopped when all the weights were lifted by the steering wheel; heavy sea.
	3 19	1 14	4·2	212 550	39	44	,,	,,	,,	15	13	—	—	—	—	
	3 33	1 30	4·5	214 —	33	,,	1866	,,	,,	,,	,,	—	—	—	—	8.22 G.T. Niagara signalled 250 miles of cable payed out.
	4 6	2 0	—	217 200	37	,,	,,	,,	,,	,,	,,	—	—	—	—	8.50. Fore trysail and storm staysail set; blowing a gale of wind; heavy sea on, men continually in attendance to turn brake-wheels.
	4 35	2 30	4·4	220 650	39	,,	1200	1600	,,	10	,,	—	—	—	—	
	5 5	3 0	4·4	224 —	40	46	,,	1000	1750	,,	,,	—	—	—	—	9.45. Niagara signalled 260 miles out. The vertical angle over the stern sheave shows great variation with the pitching of the ship, say 12½ deg. to 20 deg. H. Clifford.
	5 35	3 30	4·4	227 300	38	Sub-speed	,,	1000	1700	,,	,,	—	—	—	—	
	6 5	4 0	4·4	231 —	37	,,	,,	1000	1700	,,	,,	—	—	77½ from noon yesterday.	—	All well at end of this watch. Sam. S.T. Charles T. Bright.
	6 35	4 30	4·0	233 800	37	,,	,,	1000	,,	,,	11	—	—			
	7 5	5 0	4·5	237 800	38	,,	,,	,,	,,	,,	5	—	—			
	7 35	5 30	4·2	240 —	38	,,	,,	1800	14	11	—	—	—			11.20. Niagara payed out 270 miles.
	8 5	6 0	4·4	243 —	36	,,	,,	,,	,,	,,	,,	—	—			1 p.m. G.T. Niagara payed out 280 miles.
	8 35	6 30	4·0	247 —	39	,,	,,	1700	14	9	—	—	—			2.0 G.T. 290 miles payed out. During this watch the weather has continued very boisterous; ship full speed, only steaming 4 and 4½ knots; coals very bad; men, as previous, watch stationed to turn drums; all right at end of watch. S. Canning.
	9 5	7 0	4·6	250 300	38	,,	,,	1800	14	10	—	—	—			
	9 35	7 30	4·4	253 500	39	,,	,,	,,	,,	,,	—	—	—			
	10 5	8 0	4·5	256 600	39	,,	,,	,,	12	4	—	—	—	97·2 from noon yesterday.	—	2.45 G.T. 300 miles payed out from Niagara.
	10 35	8 30	4·6	259 200	36	,,	,,	1700	15	8	—	—	—			4.0 G.T. 300 miles payed out from Niagara.
	11 5	9 0	,,	262 —	38	,,	1200	1600	,,	,,	—	—	—			5.15 G.T. 300 miles payed out from Agamemnon.
	11 35	9 30	4·2	264 400	36	,,	,,	,,	,,	,,	—	—	—			6.20 G.T. 316 miles payed out from Niagara.
																6.0 G.T. During this watch sea has been very high and the ship pitching much. At close of watch passing soundings of 2,400 fathoms, made by Cyclops, in 1857. Charles T. Bright.
	12 5	10 0	4·0	268 200	,,	,,	,,	,,	,,	,,	,,	—	—			
	12 35	10 30	4·4	271 300	35	,,	,,	1700	,,	9	—	—	—			7.0 G.T. Niagara payed out 320 miles.
	1 5	11 0	4·2	274 200	37	,,	,,	,,	,,	8	—	—	—			6.50 G.T. 310 miles payed out. Weather during this watch, from 4 to 6 p.m., same as previous watch, except at times accompanied with squalls and heavy rain; cause of reduction of speed at end of watch being want of steam. Barometer still falling. Men at drums as usual, 29 deg. 63 min. S. Canning.
	1 35	11 30	4·0	277 600	35	,,	,,	,,	,,	10	—	—	—			
Noon.	2 0	12 0	—	280 —	,,	,,	,,	,,	,,	,,	—	—	—	107·2 from noon yesterday.	113 estimated run since noon yesterday.	
	2 20	12 20	4·6	283 300	,,	,,	,,	,,	,,	,,	—	—	—			Probably error during last four hours by fouling.
	2 22	1 15	—	288 800	37	,,	,,	,,	,,	,,	—	—	—			
	3 39	1 36	—	289 600	39	,,	,,	,,	,,	,,	—	—	—			8.30 G.T. 3.0 miles of cable payed out, by chart, 2,400 fathoms water.
	3 52	1 45	4·2	291 —	37	,,	,,	1200	1700	12½	10	—	—			8.40 G.T. Niagara payed out 330 miles of cable; all well at end of this watch; weather clearing up apparently, but sea still heavy, and occasional squalls; speed of ship increasing by lessened force of wind. Charles T. Bright.
	4 7	2 13	—	293 500	40	,,	,,	1200	1700	12½	10	—	—			No observation, weather too thick.
	4 22	2 30	4·5	295 150	39	,,	,,	1000	1700	,,	,,	—	—			113 to-day 98 yester day
	4 52	2 45	4·6	297 600	40	,,	,,	,,	,,	,,	,,	—	—			
	5 7	3 0	—	299 200	,,	,,	,,	,,	,,	,,	,,	—	—			211
	5 22	3 15	4·6	301 —	38	,,	1866	1000	1700	15	11	—	—			10.5 p.m. G.T. Niagara, 340, payed out.
	5 45	3 45	,,	302 300	34	,,	,,	1200	1500	,,	,,	—	—			11.12 G.T. Engine reduced 2 revolutions.
	6 0	4 0	4·4	304 700	36	,,	,,	,,	1600	,,	10	—	—			11.30 G.T. Speed of engines reduced to 38.
	6 15	4 15	—	306 300	39	,,	,,	,,	,,	,,	,,	—	—			Do. Increased to 40.
	6 30	4 30	4·4	308 —	39	,,	,,	,,	,,	,,	,,	—	—			11.45 G.T. Niagara payed out 350 miles.
	6 45	5 0	,,	309 200	38	,,	1300	1700	14½	7½	—	—	—			11.55 G.T. Ceased engines to 38.
	7 0	5 15	4·4	311 —	37	,,	,,	,,	,,	,,	8	—	—			12.8 a.m. G.T. Splice went out.
	7 30	*5 45	4·4	314 —	34	,,	,,	,,	,,	14	11½	—	—			12.15 G.T. Do. do.
	8 0	6 5	3·5	317 500	34	,,	,,	,,	,,	,,	10	—	—			12.30. Reduced speed of engines to 36 revols.
	8 30	6 35	4·5	320 —	37	,,	,,	,,	,,	15	7½	—	—			12.32 a.m. G.T. Third splice went out.
	8 54	7 0	4·5	323 —	40	,,	,,	,,	,,	15	10	—	—			1 G.T. Reduced speed of engines to 34 revols.
	9 24	7 30	5·0	—	42	,,	1200	1600	12½	7½	—	—	—			12.30. Between 12 and 12.30 G.T. the current from Niagara became much weaker. Immediately after that tests by currents from this ship indicated defective insulation somewhere or dead earth at a very remote part of the cable. At 12.30 the current came from the Niagara, and the regular signals followed, but much weaker than usual. The tests after 12.30 still indicated defect of the same character, but less in degree and seeming to diminish gradually. From 12.30 to 1 the cable tested quite as usual. From 1 to 1.10, strong regular currents were received. Since that time many irregularities have been observed. By 1 o'clock the cable tested quite as usual, and continues to do so. W. Thomson.
	9 53	8 0	5·5	329 400	42	,,	,,	,,	,,	,,	5	—	—	40 from noon this day.		
	10 20	8 24	4·6	333 —	41	42	,,	,,	1700	,,	10	—	—	211		
	11 3	9 0	5·0	337 600	43	,,	,,	,,	1500	,,	5	—	—	40		
	11 30.	9 30	4·4	340 600	41	40	,,	,,	,,	,,	,,	—	—	291 run. 329 p. out; 7810		

APPENDIX No. 16.—*continued.*

Log of the successful Paying-out the Atlantic Cable on Board the Agamemnon—*continued.*

DATE.	Greenwich Time.	Ship's Time.	Ship's Speed, by Log.	Cable payed out.	No. of Revolutions of Machine per Minute.	No. of Revolutions of Screw per Minute.	Weights in lbs. upon Brakes.	Indicated Strain. Lowest.	Indicated Strain. Highest.	Angle of Cable. Vertical.	Angle of Cable. Horizontal.	Flakes payed out of Coil.	Splices and Lappings.	Distance by Massey's Log.	Distance made good at Noon.	REMARKS.
	H. M.	H. M.	knots.	Mls. Fths.												H.M.
July 31.	12 0	10 0	5·2	343 —	43	38	1866	1200	1600	12½	5	—	—	—	—	1.5 a.m., G.T. Niagara payed out 300 miles.
	12 30	10 30	5·2	347 750	38	,,	,,	,,	,,	,,	,,	—	—	—	—	8 to 12 p.m., July 31. In this watch all has gone well, the sea becoming calmer. Wind blowing, and getting more favourable; enabled to reduce speed of engines in consequence. S. CANNING.
	1 0	11 0	5·0	351 600	42	36	,,	,,	,,	,,	,,	4	—	—	—	
	1 30	11 30	4·8	354 600	38	34	,,	,,	,,	,,	,,	—	—	—	—	
																AUGUST 1.
																2.25 a.m. G.T. Splice went out.
																2.40 a.m. G.T. Niagara payed out 370 miles.
																3.14 a.m. G.T. Splice went out.
																3.25 a.m. G.T. Increased speed of engines 2 revolutions.
August 1	2 0	12 mid.	4·2	358 —	35	,,	,,	,,	,,	,,	,,	—	—	—	—	
	2 15	12 15	,,	359 850	37	,,	,,	1200	1700	,,	,,	—	—	—	—	3.35 a.m. G.T. Rate of machine 42, added 2 weights, making weight and brakes 2,066; ship going easy; 14 moveable weights now on.
	2 30	12 30	4·5	361 400	,,	,,	,,	,,	1600	,,	5	—	—	—	—	3.50 a.m. G.T. Reduced the engines 2 revolutions.
	2 45	12 45	,,	363 000	33	,,	,,	,,	,,	,,	,,	—	—	—	—	CHARLES T. BRIGHT.
	3 0	1 0	4·5	365 700	40	,,	,,	,,	,,	,,	,,	—	—	—	—	
	3 15	1 15	4·5	367 100	36	,,	,,	1700	,,	,,	,,	—	—	—	—	3.50 G.T. 380 miles payed out.
	3 30	1 30	,,	368 800	42	36	,,	,,	,,	,,	,,	—	—	—	—	5.30 G.T. 390 miles payed out from Niagara.
	3 45	1 45	,,	369 600	41	,,	2066	,,	,,	,,	,,	—	—	—	—	During the watch from 12 to 4 a.m. all has gone well all right in electrical cabin.
	4 0	2 0	5·3	370 400	44	34	,,	,,	,,	,,	,,	—	—	—	—	CHARLES T. BRIGHT.
	4 15	2 15	4·8	373 500	44	,,	2066	,,	1600	,,	,,	—	—	—	—	6.40 G.T. 390 miles payed out.
	4 30	2 30	4·8	375 900	46	34	2066	1200	1600	12½	5	—	—	—	—	7.0 G.T. Niagara payed out 400 miles. Electrical condition of cable reported by Professor *Thomson as decidedly improved by submersion.*
	5 0	3 0	4·6	378 800	38	,,	,,	,,	1700	,,	,,	—	—	—	—	8.35 G.T. 400 miles payed out.
	5 30	3 30	4·6	382 400	39	,,	,,	,,	,,	,,	,,	—	—	—	—	Niagara payed out 410 miles at 8.20 G.T.
	5 53	4 0	5·0	384 700	40	,,	,,	1700	11	2½	,,	—	—	—	—	9.0 G.T. Set forward.
	6 23	4 30	4·2	388 —	35	,,	,,	,,	,,	,,	,,	—	—	—	—	9.40 G.T. Niagara signalled, 420 miles payed out.
	6 53	5 0	5·0	391 400	41	,,	,,	,,	,,	,,	2	—	—	—	—	9.45 G.T. 410 miles payed out.
	7 23	5 30	5·0	395 —	40	,,	,,	,,	,,	,,	,,	—	—	—	—	9.45 G.T. Decreased speed of engine to 33 revols.
	7 53	6 0	5·0	398 25	39	,,	,,	,,	,,	12½	1	—	—	—	—	10.0 G.T. Do do. 30 do.
	8 23	6 30	4·8	401 400	39	,,	,,	,,	,,	10	3½	—	—	—	—	In this watch from 4 to 8 a.m. all has gone well.
	8 53	7 0	5·0	405 600	42	,,	,,	,,	,,	11	3	—	Splice	—	—	Weather fine, but still a swell on. S. CANNING.
	9 30	7 30	4·6	409 —	39	,,	,,	,,	,,	12	,,	—	—	—	—	11.0 a.m. G.T. Niagara payed out 430 miles.
	10 0	8 0	5·0	412 400	41	30	,,	,,	,,	11	2	121st	—	66 miles since 8 p.m., July 31.	—	12.20 a.m. G.T. Wind freshening and weather thickening. 2 weights taken off brakes, making total 1866.
	10 30	8 30	4·5	415 800	39	,,	,,	,,	,,	,,	,,	—	—		—	12.50 a.m. G.T. Squall with rain; going 5½ by log; eased ship's engine 2. 440 miles payed out from Niagara at 12.40.
	11 0	9 0	,,	418 200	38	,,	,,	,,	,,	12½	,,	—	—		—	*All right at end of this watch from 8 to noon.*
	11 30	9 30	4·5	421 400	39	,,	,,	,,	,,	8	—	—	—		—	CHARLES T. BRIGHT.
																2.15 p.m. G.T. 440 miles payed out.
																2.30. Niagara payed out 6 0 miles.
																3.10. Vertical angle paying with the rise of the ship from 11 to 17½°.
																3.30 G.T. 450 mil s payed out.
	12 0	10 0	4·8	424 850	37	,,	,,	,,	,,	,,	,,	—	—	—	—	3.40 G.T. Niagara payed out 450 miles.
	12 30	10 30	4·8	427 200	,,	,,	,,	,,	,,	,,	,,	—	—	—	—	3.45 G.T. Eased speed of engines to 27 revols.
	1 0	11 0	4·4	430 400	38	28	1866	,,	,,	14	3	—	—	—	—	4 0 G.T. Do. do. 28 do.
	1 30	11 30	4·5	433 800	39	30	,,	,,	,,	,,	,,	8	—	—	—	4.41 G.T. Increased engine 2 revolutions. Took in fore-topsails.
																5.0 G.T., Niagara payed out 470 miles.
	—	12 0	,,	434 800	38	,,	,,	,,	,,	,,	,,	9	—	126 since noon, July 31.	333 from Friday.	5.20 G.T. Increased engines 2 revolutions.
	2 10	12 30	5·0	440 900	42	31 full speed.		,,	,,	,,	,,	9	—			In this watch from 12 to 4 p.m. there has been heavy squalls of wind and rain; much swell, and ship pitching heavily. All well at end of watch. S. CANNING.
	2 40	1 0	5·2	442 600	44	*4th grade.* 1203		1550	13	11	—	—	—	—	—	
	3 10	1 30	5·0	445 200	40	,,	,,	,,	,,	13	11	—	—	—	—	5.35 p.m. G.T. Splice went out.
	3 40	2 0	5·3	448 400	43	,,	,,	,,	,,	,,	5	—	—	—	—	6.30 p.m. G.T. Niagara signalled 480 miles out.
	4 10	2 30	5·1	453 —	39	23	,,	1000	1300	,,	6	—	—	—	—	6.30 p.m. G.T. 470 miles payed out.
	4 40	3 0	5·2	456 200	40	23	,,	1300	1700	12½	8	—	—	—	—	7.20 p.m. G.T. Screw increased 2 revolutions.
	5 10	3 30	5·5	459 300	44	27	,,	,,	1600	,,	6	—	—	—	—	7.40 p.m. G.T. 30 miles run since noon by Massey's log, taken up in consequence of getting foul of common log.
	5 40	4 0	,,	463 —	48	25	,,	1200	,,	15	,,	—	—	—	—	In this watch much difficulty experienced from excessive pitching of the ship.
	6 10	4 30	4·8	468 —	42	,,	,,	,,	1500	,,	8	—	—	—	—	CHARLES T. BRIGHT
	6 40	5 0	4·8	470 —	42	,,	,,	,,	,,	,,	,,	—	—	—	—	7.40 G.T. Niagara payed out 480 miles.
	7 10	5 30	4·8	475 —	43	,,	,,	,,	,,	,,	7	—	—	26 since noon.	—	9.10 G.T. 490 miles payed out.
	7 40	6 0	,,	478 600	42	27	,,	,,	,,	,,	10	—	—	—	—	9.40 G.T. Niagara payed out 500 miles.
	8 8	6 30	5·2	480 750	42	,,	,,	,,	,,	12½	4	—	—	—	—	In this watch, from 4 to 8 p.m., there has been heavy sea; long swell and wind; ship still pitching heavily. S. CANNING.
	8 40	7 0	5·5	485 500	44	,,	,,	,,	,,	,,	,,	—	—	—	—	10.45 G.T. Increased ship 2 revolutions.
	9 7	7 30	,,	489 —	44	,,	,,	,,	,,	13	4	—	—	—	—	11.25 G.T. (9·45 A.T.) in hauling in common log, found with Massey's log, which came in with it.
	9 37	8 0	5·0	493 —	45	,,	,,	,,	,,	,,	,,	104	—	—	—	10.40 G.T. Niagara, 510 miles.
	10 10	8 30	5·0	497 900	45	30	1866	1300	1300	13°	44	—	—	—	—	11.50 G.T. Ditto, 520 ,,
	10 40	9 0	4·8	501 300	40	,,	,,	,,	,,	,,	,,	—	—	—	—	12 G.T. Eased engine 2 revolutions, and shortly after 2 more.
	11 10	9 30	5·0	505 —	44	32	,,	,,	,,	,,	,,	—	—	—	—	1.30 G.T. Niagara payed out 530.
	11 40	10 0	5·0	509 —	44	,,	,,	,,	,,	,,	,,	—	—	—	—	During the watch, from 8 to midnight, there has been a tremendous swell on, and the ship has rolled and pitched excessively. CHARLES T. BRIGHT.
																AUGUST 2.
	12 0	10 30	5·0	512 300	47	,,	1866	,,	1600	,,	10	—	—	—	—	2.58 a.m. G.T. 530 miles payed out.
	12 40	11 0	4·8	516 400	42	27	,,	,,	1500	,,	5	—	—	—	—	3 a.m. G.T. Niagara payed out 540 miles.
	1 10	11 30	5·0‡	519 100	46	,,	,,	,,	1400	,,	,,	—	—	—	—	4.15 a.m. G.T. 540 miles payed out.
	1 40	12 0	Log out of order.	522 200	42	,,	,,	,,	,,	,,	4	—	—	—	—	4.30 a.m. G.T. Niagara payed out 550 miles.
																5.35 a.m. G.T. Ditto ditto 560 ,,
																5.40 a.m. G.T. 560 miles payed out.
August 2	2 10	12 30	5·0	526 —	42	27	1866	,,	1400	13	4	—	—	—	—	From midnight to 4 a.m., squalls of wind and rain, with heavy swell. Ship pitching and rolling heavily; towards termination of watch sea getting down.
	2 40	1 0	4·8	529 650	,,	,,	,,	,,	,,	,,	,,	—	—	—	—	Reported that signals from Niagara had been getting weak; made preparations for picking cable at 5.30 G.T.; reported signals right again; at termination of watch all going well.
	3 10	1 30	5·0	533 300	43	,,	,,	,,	,,	,,	,,	—	—	—	—	S. CANNING.
	3 40	2 0	5·0	536 800	42	,,	,,	,,	,,	,,	2	—	—	—	—	
	4 10	2 30	5·0	539 500	44	,,	,,	,,	,,	,,	,,	—	—	—	—	Roll of ship 7 deg. to 12 deg. at commencement of watch; reported from electrical cabin that signals, which had been weaker since midnight, had failed altogether shortly before end of last watch, but had afterwards returned, though remaining weaker than originally.
	4 40	3 0	5·0	543 0	41	,,	,,	,,	,,	,,	5	—	—	—	—	
	5 10	3 30	5·0	546 900	42	,,	,,	,,	,,	,,	,,	—	—	—	—	
	5 40	4 0	5·0	550 0	43	,,	,,	,,	,,	,,	,,	—	—	—	—	6.5 a.m. G.T. Splice payed overboard.
	6 10	4 30	4·8	550 700	43	,,	,,	,,	,,	,,	6	92	Splice	—	—	6.30 a.m. G.T. Signals with rain.
	6 40	5 0	4·5	553 300	38	,,	,,	,,	1500	,,	,,	—	—	—	—	6.40 a.m. G.T. Increased speed of ship 2 revols.
	7 10	5 30	4·2	562 600	39	29	,,	,,	,,	,,	5	—	—	—	—	6.50 a.m. G.T. Niagara payed out 570.
	7 40	6 0	4·8	566 0	39	31	,,	,,	,,	,,	,,	—	—	—	—	7.10 a.m. G.T. Increased speed of ship 2 revols.
	8 10	6 30	4·8	569 400	42	,,	,,	,,	,,	,,	,,	—	—	—	—	7.30 a.m. G.T. Signals in electrical room very much better for the last half-hour.
	8 40	7 0	5·2	572 700	41	29	,,	,,	,,	,,	,,	—	—	—	—	7.45 a.m. G.T. Signals still continuing to improve.
	9 10	7 30	5·5	576 1200	40	27	,,	,,	,,	,,	8	—	—	—	—	

* But varying much with the pitching of the ship. † Varying with motion of stern caused by heavy swell. ‡ Log out of order.

A. T. No. 16.

APPENDIX No. 16—*continued.*

Log of the successful Paying-out the Atlantic Cable on Board the Agamemnon—*continued.*

DATE.	Greenwich Time.	Ship's Time.	By Ship's Log. Speed	Cable payed out.	No. of Revolutions of Machine per Minute. No. of Revolutions of Screw per Minute.	Dip. upon Wheel.	Weight in Brakes.	Indicated Strain. Lowest.	Indicated Strain. Highest.	Angle of Cable. Vertical.	Angle of Cable. Horizontal.	Flakes payed out of Coil.	Splices and Lappings.	Distance by Massey's Log.	Distance made good at Noon.	REMARKS.
	H. M.	H. M.	Knots.	Mls. Fths.												H.M.
Aug. 2	9 40	8 0	4·8	578 300	38	27	1866	1200	1500	18	6	86	—	101 since noon yesterday.	—	8.20. Niagara payed out 580 miles; hoisted foretopsail, and reduced engines 2 revolutions.
	10 10	8 30	„	581 —	41	„	„	„	„	„	5	—	—			9. 5. Reduced 2 revolutions of engine on account
	10 40	9 0	5·5	584 400	40	„	„	„	„	„	„	—	—	N.B. Massey's log has been fouled twice.	—	of squall coming up; 9.15, reduced 2 more, breeze freshening now 25; 9.20, increased 2 again.
	11 10	9 30	„	588 —	43	„	„	„	„	„	„	—	—			9.30. 590 miles payed out from Niagara.
	11 40	10 0	6·0	593 600	44	„	„	„	„	„	„	—	—			During this watch the ship has rolled and pitched very much; signals from Niagara at close of watch reported as stronger than ever.
P.M.	12 10	10 30	5·8	596 300	45	„	„	„	1200	„	„	—	—	—	—	CHARLES T. BRIGHT.
	12 40	11 0	„	600 —	„	„	„	„	„	„	„	—	—	—	—	10.5 a.m. G.T. 580 miles payed out.
	12 55	11 30	5·2	602 100	44	„	„	„	„	„	„	—	—	—	—	10.30 a.m. G.T. Niagara payed out 600 miles.
	1 24	12noon	5·5	605 —	45	„	„	„	„	„	„	—	—	126 miles since yesterday.	127 miles since yesterday.	11.27 a.m. G.T. 590 miles payed out.
	1 55	12·35	5·5	610 —	46	„	„	„	1400	„	„	—	—			12.10 p.m. Niagara payed out 610 miles.
	2 25	1 0	5·5	614 700	45	„	„	„	„	„	„	—	—	—	—	12.40. 620 miles payed out.
	3 25	2 0	5·4	622 —	44	„	„	„	„	„	„	—	—	—	—	Squally during the first part of this watch, from 8 a.m. to noon, since then finer weather and sea becoming more calm. All right at end of watch.
	3 55	2 30	5·2	625 500	42	„	„	„	„	„	„	—	—	—	—	S. CANNING.
	4 22	3 0	*	629 —	44	27	1866	1200	1500	12½	4	—	—	—	—	1.40. G.T. No signals for three intervals of 10 minutes from the Niagara reported; variable currents, but not the pre-arranged signals; may be only earth currents.
	5 55	3 30	5·5	633 500	45	„	„	„	„	„	„	—	—	—	—	1.50. All right again from Niagara; 620 miles
	5 25	4 0	5·2	636 500	44	„	„	„	„	„	„	—	—	—	—	miles payed out by her; unmistakeably good signals coming now.
	5 55	4 30	5·0	639 200	43	„	„	„	„	„	3	—	—	—	—	2.5. Cut the upper end of the deck coil at union with the main hold coil by Professor Thomson's
	6 25	5 0	„	642 100	„	„	„	„	„	„	15	2	—	—	—	desire, and joined the main hold *only* to the
	6 55	5 30	4·8	646 —	„	„	„	„	„	„	12½	„	—	—	—	circuit.
	7 25	6 0	5·0	649 150	42	„	„	„	„	„	„	70	—	—	—	3.10. G.T. Set foretopsail.
	7 55	6 30	5·0	653 800	43	„	„	„	„	„	„	—	—	—	—	Lost additional amount of slack by having to change our course to avoid the American ship "Chieftain," which was steering direct for us.
	8 25	7 0	5·0	657 —	42	28	„	„	„	„	3	—	—	—	—	4.50. G.T. 640 miles payed out by Niagara.
	8 55	7 30	5·6	661 —	47	„	„	„	„	„	„	—	—	—	—	All well at end of watch from noon to 4. Was ther finer. Ship's motion much less.
	9 22	8 0	5·5	663 500	42	„	„	„	„	„	„	—	—	46 since noon.	—	CHARLES T. BRIGHT.
	9 55	8 30	5·0	669 ·680	44	„	„	„	„	„	2	—	—	Total run 511.	—	5.10 p.m. G.T. Niagara payed out 650 miles.
	10 25	9 0	5·5	672 —	42	„	„	„	„	„	„	—	—	Distance from Valencia 204.	—	6.6 p.m. G.T. 660 miles payed out.
	10 55	9 30	5·5	675 400	42	27	„	„	„	12	„	—	—			6.15 p.m. Squall of wind, with rain, reduced speed of engines to 23 revolutions.
	11 25	10 0	5·8	678 50	42	„	„	„	„	„	0	—	—			6.30. Increased speed of engines to 27 revolutions.
	11 55	10 30	5·0	682 —	41¼	„	„	„	„	„	„	—	—			6.37. Niagara payed out 670 miles.
	12 25	11 0	5·4	685 150	42	„	„	„	„	„	„	—	—	—	—	6.30 G.T. Paid out 680 miles.
	12 55	11 30	5·8	688 600	43	„	„	„	„	„	„	—	—	—	—	A few squalls in this watch; at other times sea calmer. All right at end of watch.
August 3	1 25	12 0	5·4	691 400	40	„	„	„	„	„	„	60	—	—	—	S. CANNING.
	1 55	12 30	5·2	696 600	40	„	„	„	„	„	„	—	—	—	—	7·27. G.T. Increased speed of ship 2 revolutions.
	2 25	1 0	5·3	699 500	41	„	„	„	„	„	„	—	—	—	—	8.30. G.T. Niagara payed out 670 miles.
	2 15	1 30	5·4	703 500	40	„	„	„	„	„	„	—	—	—	—	8.35. G.T. Increased speed of ship 2 revolutions.
	3 25	2 0	5·3	706 200	„	25	„	„	„	„	„	—	—	—	—	8.50. Decreased speed 2 revolutions. Squall coming up, took in foretopsail.
	3·55	2 30	5·0	709 500	36	„	„	„	„	„	„	—	—	—	—	All right at end of watch.
	4 25	3 0	4·6	713 800	„	29	„	„	„	„	„	—	—	—	—	CHARLES T. BRIGHT.
	4 55	3 30	5·4	716 600	42	27	„	„	„	„	„	—	—	—	—	9.40 G.T. Niagara payed out 680 miles.
	5 23	4 0	5·4	720 200	40	„	„	„	„	„	„	—	—	—	—	10 G.T. 690 miles payed out.
	5 55	4 30	5·5	723 500	42	„	„	„	„	„	„	—	—	—	—	10.47 G.T. Wind freshening, eased engines 22 revolutions.
	6 25	5 0	5·6	726 500	„	„	„	„	„	„	„	—	—	—	—	11.34. G.T. 680 miles payed out.
	6 55	5 30	5·6	730 —	„	„	„	„	„	„	„	—	—	—	—	11.10. Niagara payed out 690 miles.
	7 25	6 0	5·0	733 300	„	„	„	„	„	„	„	—	—	—	—	AUGUST 3.
	7.55	6 30	5·0	736 400	42	27	1866	1200	1500	12	0	—	—	—	—	12.30 a.m. G.T. Niagara payed out 700 miles.
	8 23	7 0	5·2	740 *	—	„	„	„	„	„	„	—	—	—	—	1.8 a.m. 680 miles payed out.
	8 55	7 30	5·2	744 —	45	„	„	„	1400	12½	5	—	—	—	—	8 to 12 p.m. Everything has gone on satisfactorily in this watch. Electrical signals reported good.
	9 28	8 0	5·2	748 400	45	„	„	„	„	„	„	45	—	113 since noon yesterday, 229 miles from Valencia.	—	S. CANNING.
	9 55	8 30	5·0	753 100	43	27	„	„	„	„	1	„	—			2.10 a.m. G.T. Niagara signalled 710 miles payed out.
	10 25	9 0	5·0	756 800	43	„	„	„	„	„	„	—	—			3.50. Niagara payed out 720 miles;
	10 55	9 30	5·0	760 —	44	„	„	„	„	„	3	—	—			4.30. Ship rolling very much. Wind got round to N.W., and sea heavy, slow swell from S.W. setting in.
	11 55	10 30	5·0	767 400	42	„	„	„	„	12½	0	—	—			4.40. A barque bearing down upon our beam end, starboard side, Niagara fired guns, and caused her to alter her course. Increased speed 2 revolutions to clear her.
	12 25	11 0	5·2	770 600	„	„	„	„	„	„	„	—	—	—	—	5.35. Niagara 730 miles.
	12 40	11 30	5·6	773 —	48	„	„	„	„	„	„	—	—	Massey's log fouled.	134·2	All well during watch ending 4 a.m.
	1 10	12 0	5·6	776 300	„	„	„	„	„	„	„	—	—	—	—	CHARLES T. BRIGHT.
	1 37	12 30	5·0	779 200	42	„	„	„	„	„	„	—	—	—	—	6.55. G.T. 730 miles payed out.
	2 7	1 0	5·0	782 100	44	29	„	1500	„	„	1	—	—	—	—	7.10. Niagara payed out 740 miles.
	2 45	1 30	5·0	785 900	44	„	„	„	„	„	2	—	—	—	—	8.25. G.T. Payed out 740 miles.
	3 15	2 0	5·2	790 350	44	„	„	„	„	„	„	—	Splice	—	—	8.35. Niagara payed out 750 miles.
	3 45	2 30	5·0	793 900	42	„	„	„	1400	„	0	—	—	—	—	All right at close of watch, ending noon; weather fine, but heavy swell on. C. T. BRIGHT.
	4 15	3 0	4·6	797 600	40	„	„	„	„	„	„	—	—	—	—	1.30 a.m. G.T. 780 miles payed out from Niagara.
	4 45	3 30	5·0	800 200	„	„	„	„	„	„	„	—	—	—	—	1.50. 780 miles payed out.
	5 15	4 0	4·8	803 —	42	‡	„	„	„	„	31	—	—	—	—	2.0. Increased speed of engines, 2 revolutions.
	5·45	4 30	5·4	809 200	44	„	„	„	„	„	„	—	—	28·2 since noon, at 5 p.m. s.s. changed log to starboard side.	—	2.50. Niagara payed out 790 miles.
	6 15	5 0	5·4	811 500	41	„	1466	nil.	nil.	„	1	—	—			3.11. 790 miles payed out.
	6 45	5 30	†	815 —	42	„	„	—	—	13	1	—	—			3.14. Splice went out.
	7 15	6 0	5·0	818 800	42	31	„	—	—	14	0	—	—			4.30. Niagara payed out 800 miles.
	7 45	6 30	5·4	820 900	41	„	„	—	—	„	0	—	—			4.35. 800 miles payed out.
	8 15	7 0	5·4	824 —	40	32	„	—	—	„	0	—	—			5.10. Engines increased to full speed, 4th grade; at end of this watch, everything going well, 4 p.m. S. CANNING.
	8 45	7 30	5·5	827 500	39	„	„	—	—	„	0	—	—			6.0 a.m. 810 miles payed out from Niagara.
	9 15	8 0	5·5	831 —	37	„	„	—	—	„	0	22	—	43 since noon.	—	6.2. 810 miles payed out.
	9 45	8 30	„	834 —	36	„	„	—	—	„	0	—	—			6.10. Speed of paying out diminished from 45 to 38 without any apparent reason; ship going 5.4 as before; log hove twice; reduced strain by lifting weights to keep speed in same proportions as before: took off two weights, also reduced weights to 1656; 6.15, took off two more weights, reducing to 1466; all well during this watch from 4.0 to 6.0. CHARLES T. BRIGHT.
	10 15	9 0	5·0	838 500	34	„	1066	—	—	„	„	—	—	—	—	
	10 45	9 30	5·0	841 —	38	„	„	—	—	„	„	—	—	—	—	
	11 45	10 30		846 700	38	„	„	—	—	„	„	—	—	—	—	

* Log not taken; turning ship to clear American vessel.
† Log out of order. ‡ Full speed on this grade.

APPENDIX No. 16.—*continued.* APP. No. 16.

Log of the successful Paying-out the Atlantic Cable on Board the Agamemnon—*continued.*

DATE.	Greenwich Time.	Ship's Time.	Ship's Speed by Log.	Cable payed out.	No. of Revolutions of Machine per Minute.	No. of Revolutions of Screw per Minute.	Weight in lbs. upon Brakes.	Indicated Strain. Lowest.	Indicated Strain. Highest.	Angle of Cable. Vertical.	Angle of Cable. Horizontal.	Flakes payed out of Coil.	Splices and Lappings.	Distance by Massey's Log.	Distance made good at Noon.	REMARKS.
	H. M.	H. M.	knots.	Mls. Fths.												
																P.M.
Aug. 3	12 45	11 30	5·0	853 300	34	31	1066	Nil.	Nil.	14	0	—	—	—	—	7.30 p.m. G.T. Niagara payed out 820 miles.
	1 15	12 0	5·0	856 300	38	31	1066	,,	,,	14	0	—	—	—	—	7.37 p.m. G.T. 820 miles payed out.
																9.8 p.m. 830 ,, ,,
																There has been no alteration in this watch; all has gone right; electric signals reported perfect. S. CANNING.
																8.0 p.m. All the brakes off the machine being kept running by the tension of the cable, which is forming an easy curve from the stern.
																10.0 p.m. 4 moveable weights taken off weight on brakes 1,066 lbs., 10.30. 840 miles from Niagara; 850 miles out of Niagara at 12.10 a.m., G.T.
																All right during this watch from 8 p.m. to midnight. CHARLES T. BRIGHT.
																AUGUST 4.
Aug. 4	1 45	12 30	5·5	859 400	39	,,	,,	,,	,,	,,	,,	—	—	—	—	1.40 a.m. G.T. Niagara payed out 860 miles.
	2 15	1 0	5·5	862 800	42	,,	,,	,,	,,	,,	,,	—	—	—	—	1.55 a.m. G.T. 865 miles payed out.
	2 45	1 30	5·7	866 100	41	,,	,,	,,	,,	15	1	—	—	—	—	3. 0 a.m. G.T. Niagara in 200 fathoms water.
	3 15	2 0	5·0	869 600	40	,,	,,	,,	,,	,,	,,	—	—	—	—	3 20 a.m. G.T. 870 miles out.
	3 45	2 30	4·6	872 800	34	,,	,,	,,	,,	18½	2	—	—	—	—	3.15 a.m. G.T. Shortened sail.
	4 15	3 0	4·8	875 900	36	,,	,,	,,	,,	,,	,,	—	—	—	—	5.40 a.m. G.T. Niagara payed out 870 miles.
	4 45	3 30	4·6	878 800	35	31	1066	,,	,,	20	2	—	—	—	—	5.53 a.m. G.T. Passed bight from main hold to deck coil.
	5 15	4 0	Nil.	879 900	24	Nil.	,,	,,	,,	,,	,,	—	—	—	—	All right during this watch. S. CANNING.
	5 45	4 30	4·0	882 000	34	28	,,	,,	,,	24	,,	—	Splice	—	—	5.26 a.m. G.T. Splice went out.
	6 15	5 0	5·0	886 200	36	30	,,	,,	,,	16	0	—	—	—	—	5.40 a.m. G.T. 880 miles payed out.
	6 45	5 30	—	889 200	,,	,,	,,	,,	,,	,,	,,	—	—	—	—	5.50 a.m. G.T. Niagara payed out 880 miles.
	7 15	6 0	4·8	892 900	33¾	,,	,,	,,	,,	14	,,	—	—	583½	—	7.35 a.m. G.T. Do. Do. 890 miles.
	7 45	6 30	4·7	895 500	,,	28	,,	,,	,,	,,	4	—	—	since 8 P.M.	—	7.30 a.m. G.T. Eased the screw two revolutions, the upper deck coil coming out very dry.
	8 10	7 0	4·8	898 630	33	,,	,,	,,	,,	,,	,,	—	—	—	—	8.25 a.m. G.T. 900 miles payed out.
	8 40	7 30	—	901 300	34	,,	,,	,,	,,	,,	,,	—	—	—	—	8.40 a.m. G.T. Niagara payed out 900 miles.
	9 10	8 5	4·4	904 100	35	26	,,	,,	,,	,,	,,	—	—	—	—	All right at termination of watch.
	9 40	8 30	4·2	907 —	36	25	,,	,,	,,	24	0	—	—	—	—	CHARLES T. BRIGHT.
	10 10	9 0	4·0	910 —	39	,,	,,	,,	,,	22	,,	—	—	—	—	10.10 a.m. G.T. Niagara payed out 910 miles.
	10 40	9 30	4·0	912 800	31	,,	,,	,,	,,	,,	,,	—	—	—	—	10.10 a.m. G.T. 910 miles payed out.
	11 10	10 0	4·0	915 100	30	,,	,,	,,	,,	20	,,	—	—	—	—	11.50 a.m. G.T. Niagara payed out 920 miles.
	11 45	10 30	4·0	918 0	33	,,	,,	,,	,,	21	,,	—	—	—	—	12 5 a.m. G.T. 920 miles payed out.
																12.42 a.m. G.T. Increased strain 200 lbs.
																All going well at end of watch. S. CANNING.
	12 10	11 0	4·0	920 200	31	,,	,,	,,	,,	,,	,,	—	—	—	—	1.9. p.m. G.T. Splice went out.
	2 57	12 noon	4·0	924 800	28	,,	1266	,,	,,	16	,,	—	—	—	—	Niagara 930 miles out at 3 p.m. G.T.
	2 30	1 30	4·0	—	29	,,	,,	,,	,,	,,	,,	—	—	—	—	Agamemnon 930 miles out at 3 p.m. G.T.
	3 45	2 45	5·1	939 —	,,	,,	,,	,,	,,	15	,,	—	—	—	—	Niagara 940 ,, 4 p.m. G.T.
	4 45	3 45	5·0	945 750	30	,,	,,	,,	,,	,,	,,	—	—	—	—	Agamemnon 940 ,, 4 p.m. G.T.
	5 27	4 30	5·0	948 400	31	,,	,,	,,	,,	,,	,,	—	—	—	—	All right during this watch. H. CLIFFORD.
	5 55	5 0	4·8	950 750	30	,,	,,	,,	,,	,,	,,	—	—	—	—	5.40 p.m. G.T. Niagara payed out 950 miles.
	6 25	5 30	4·7	952 900	30	,,	,,	,,	,,	,,	,,	—	—	—	—	5.48 p.m. G.T. 950 miles payed out.
	6 55	6 0	4·7	954 800	29	,,	,,	,,	,,	,,	,,	—	—	—	—	During this watch all has gone on satisfactorily. S. CANNING.
	7 20	6 35	4·6	959 900	31	,,	,,	,,	,,	,,	,,	—	—	—	—	7.20. Niagara 970 miles out.
	8 45	7 45	5·0	965 600	29	,,	1266	,,	,,	,,	,,	—	—	—	—	7.30. Agamemnon 960 ditto.
	9 27	8 30	4·5	969 400	31	29	,,	,,	,,	,,	,,	—	—	—	—	All right *during this watch*.—H. CLIFFORD.
	9 55	9 0	4·4	971 900	28	,,	,,	,,	,,	18	,,	—	—	—	—	9.10 p.m. G.T. *Niagara* payed out 960 miles.
	10 20	9 30	4·3	974 —	29	,,	,,	,,	,,	,,	,,	—	—	—	—	9.38 p.m. G.T. Passed bight from upper deck coil to drop deck; engines reduced 24 revolutions.
	10 50	10 0	4·3	976 800	29	,,	,,	,,	,,	21	0	—	—	—	—	9.34. 970 miles payed out.
	11 25	10 30	4·3	979 —	28	,,	,,	,,	,,	26	,,	—	—	—	—	10.30. Niagara payed out 990.
	11 55	11 0	4·5	981 800	29	24	,,	,,	,,	,,	,,	—	—	—	—	11.30. Increased speed of engines 2 revolutions.
																11.40. 980 miles payed out.
																11.37. Ship going 4.5 knots.
																During this watch everything has gone on satisfactorily. S. CANNING.
August 5	12 25	11 30	4·7	984 100	29	,,	,,	,,	,,	,,	,,	—	—	—	—	AUGUST 5.
	12 55	12 0	4·7	986 400	30	,,	,,	,,	,,	22½	0	—	—	—	—	9.10 a.m. G. T. Signal from Niagara 1000 miles payed out. Agamemnon payed out 1000 miles at 3.30, G. T. Entering Valencia Bay at 3.45.
	2 20	12 0	3·5	993 3	—	all off.	,,	,,	,,	,,	,,	—	—	—	—	4.10 a.m. G.T. Niagara payed out 1013 miles.
	4 55	4 0	5·0	1004 —	31	24	,,	,,	,,	27½	0	—	—	—	—	7.0 G.T. Anchored in Double Bay.
	5 25	4 30	4·0	1007 300	30	,,	,,	,,	,,	,,	,,	—	—	—	—	CHARLES T. BRIGHT.
	6 25	5 30	4·0	1012 300	30	,,	,,	,,	,,	29	,,	—	—	—	—	

App. No. 16. Atlantic telegraph.

Appendix No. 16—*continued.*

Letter enclosing Copy of the Engineer's Log, U.S. Steamer "Niagara," off Sandy Hook, August 17, 1858.

Log. of the U.S. steamer "Niagara."

Gentlemen,

I have the honour to enclose a copy of the engineer's log, which contains every particular of any importance connected with the paying out of the telegraph cable from this ship, and requires no explanation further, than the great per-centage of loss, from the time of making the splice to the next day at noon, was undoubtedly caused to an extent by the ship not running directly on her course. As for that day, there was a difference of 16½ miles between distance run by observation and patent log; while for the remaining part of the voyage they nearly coincided. Also, that the speed of the ship noted per hour must be considered strictly correct, as it is not possible to log accurately by the ordinary means.

Nearly all the stores were landed at the Telegraph House, Bay of Bull's Arms, as they would be of much more service there than any other disposition which could have been made of them.

There is now remaining on board about 60 miles of cable manufactured by Glass, Elliot, and Co., during the present season, and about 20 miles of the cable recovered from Valentia Bay, most of which is not suitable for use.

The cable, machinery, and few articles now on board, will be disposed by the direction of Mr. Field.

It is almost needless for me to say that every person connected with the undertaking has been untiring in their efforts to bring about so gratifying a result as the successful laying down of the Atlantic telegraph cable, and that Captain Hudson and all the officers have made any and every sacrifice to further the great work.

Accept my congratulations,

And believe me, &c.

(Signed) W. E. Everett.

To the Directors of the Atlantic Telegraph Company, 22, Old Broad Street, London.

U. S. Steamer "Niagara."

At Sea. Thursday, July 29, 1858.

Hour	Dynamometer. Strain.	Brake Strain.	Angle of Rope. Hor.	Angle of Rope. Ver.	Rotometer.	Amount per hour by Rotometer.	Speed of Ship.	Remarks.
A.M.					K. F.	K. F.	K F.	
1	—	—	—	—	—	—	—	From 8 to meridian.
2	—	—	—	—	—	—	—	At 10·20 Stern of ship being secured to stern of "Agamemnon" by hawser, commenced veering out cable to the "Agamemnon" to splice. At 10·30 had veered out 300 fathoms. (Signed) M. Kellogg.
3	—	—	—	—	—	—	—	
4	—	—	—	—	—	—	—	
5	—	—	—	—	—	—	—	
6	—	—	—	—	—	—	—	
7	—	—	—	—	—	—	—	From meridian to 4 p.m.
8	—	—	—	—	—	—	—	
9	—	—	—	—	—	—	—	At 1 p.m. hawser was let go and ship steaming a-head slowly, and commenced paying out the cable. At 2·54 p.m. 10 miles of cable paid out. Smooth sea. (Signed) M. Kellogg.
10	—	—	—	—	0 200	0 200	—	
11	—	—	—	—	—	—	—	
12	—	—	—	—	—	—	—	
	Lat. 52° 09' North. Long. 33° 29' West.		Knots run by ship, Knots cable paid out,		Excess of cable paid out, Per-centage of loss,			From 4 to 6 p.m.
P.M.								
1	—	—	—	—	—	—	—	Smooth sea; at 4·40 p.m. 20 miles of cable paid out. (Signed) J. Follansbee.
2	2,000	1,500	20 S.	25	4 800	4 600	2 2	
3	2,050	1,500	19 S.	21	13 600	5 813	2 6	From 6 to 8 p.m.
4	2,050	,,	18 S.	20	18 900	5 300	2 6	Smooth sea and light breeze on port beam. At 7·30 p.m., had paid out 30 miles of cable. At 7·50 continuity reported to have ceased. Ship's speed reduced. (Signed) M. Kellogg.
5	,,	,,	19 S.	26	24 900	6 000	4 2	
6	,,	,,	,,	15	27 261	5 218	4 6	
7	,,	,,	,,	,,	32 505	5 241	4 4	
8	,,	,,	,,	,,	37 800	5 304	4 4	From 8 to 12.
9	2,000	,,	,,	20	42 200	5 413	2 0	At 9·11 p.m. continuity restored. At 9·35, 40 miles of cable paid out. At 10·45 p.m. 50 miles of cable paid out. Distance run as midnight by patent log, 41 miles. (Signed) J. Follansbee.
10	,,	1,900	,,	20	45 580	5 380	3 6	
11	,,	,,	,,	18	51 2 0	5 713	4 0	
12	,,	,,	,,	19	56 800	5 581	4 2	

At Sea. Friday, July 30, 1858.

A.M.					K. F.	K. F.	K. F.	
1	2,050	1,900	,,	,,	62 381	5 713	4 2	From midnight to 4 a.m.
2	2,050	1,900	,,	,,	68 250	5 753	4 6	Very light breeze on port quarter, and smooth sea. At 12·24 a.m. 60 miles of cable paid out. At 2·12 a.m. 70 miles of cable paid out. At 3·55 a.m. first-hand paying out forward coil on spar deck, and commenced on forward tiers on berth deck. At 4·10 a.m. 80 miles of cable paid out. (Signed) M. Kellogg.
3	2,050	1,900	,,	,,	73 803	5 553	4 2	
4	2,050	1,900	11 S.	14	79 —	5 213	4 2	
5	2,050	1,900	10 S.	16	83 210	6 240	4 6	
6	2,050	1,900	9 S.	14	91 630	6 420	5 2	From 4 to 8 a.m.
7	2,050	1,900	8 S.	14	98 150	6 521	5 4	Light fair breeze, smooth sea. At 5·45 a.m. 90 miles of cable paid out. At 7·17 a.m. 100 miles of cable paid out. At 8 a.m. had run 81¼ miles by patent log. (Signed) J. Follansbee.
8	2 0 0	1,500	2 S.	14	101 730	6 580	5 4	
9	2,050	1,500	10 S.	12	110 940	6 170	5 —	From 8 to meridian.
10	2,050	1,900	10 S.	12	117 201	6 313	5 2	At 8·31 a.m. 110 miles of cable paid out. At 10·27 a.m. 120 miles of cable paid out; distance by patent log the last 24 hours 105¾/10 miles. At 11·55 a.m. had paid out 130 miles of cable; light wind aft and smooth sea. (Signed) M. Kellogg.
11	2,050	1,890	6 S.	12	123 800	6 601	6 —	
12	2,050	1,890	8 S.	12	131 910	8 100	7 4	
	Lat. 51° 20' N. Long. 34° 45' W.		Knots run by ship, 85. Knots cable paid out, 131 9/10. Depth of water, 1,550 to 1,575.		Excess of cable paid out, 2,900 Per-centage of loss, 48.			From meridian to 4 p.m.
P.M.								
1	2 050	1 800	8 S.	12	138 720	6 823	5 4	At 1·10 p.m. 140 miles of cable paid out. At 2·36 p.m. 150 miles of cable paid out. At 4·02 p.m. 160 miles of cable paid out; distance run since noon by patent log at 4 p.m. 23¼/10 knots. (Signed) J. Follansbee.
2	2 000	1 800	9 S.	11	145 720	6 970	6 —	
3	2 000	1 800	9 S.	12	152 800	7 010	6 —	
4	2,000	1,800	9 S.	11	159 800	7 —	6 4	From 4 to 6 p.m.
5	2,000	1 8 0	10 S.	12	166 500	6 673	6 —	At 5·30 p.m. 170 miles of cable paid out; strong wind and moderate sea aft. (Signed) W. Kellogg.
6	2,100	1 800	10 S.	12	173 307	6 813	6 2	
7	2,050	1 800	8 S.	12	179 761	6 465	6 —	From 6 to 8 p.m.
8	2 050	1 800	10 S.	12	186 210	6 503	5 6	Fresh breeze and moderate sea on port quarter. At 7·03 p.m. 180 miles of cable paid out. (Signed) J. Follansbee.
9	2,100	1 800	,,	,,	192 700	6 390	5 —	
10	2 100	1,800	,,	,,	199 210	6 513	5 4	From 8 to 12.
11	2 100	1,800	,,	,,	205 800	6 609	6 —	Distance run by patent log at 8 p.m. since noon 46¾/10 miles. At 8·34 p.m. 190 miles of cable paid out. At 10·08, 200 miles; at 11·39, 210 miles. Wind and sea moderating. Wind changed from port quarter to port beam. (Signed) M. Kellogg.
12	2,100	1,800	,,	,,	212 330	6 513	6 2	

APPENDIX No. 16.—*continued.*

App. No. 16. — Atlantic telegraph. — Log of the U.S. steamer "Niagara."

U. S. STEAMER "NIAGARA."

AT SEA. Saturday, July 31, 1858.

Hour.	Dynamometer Strain.	Brake Strain.	Angle of Rope. Hor.	Angle of Rope. Ver.	Rotometer.	Amount per hour by Rotometer.	Speed of Ship.	Remarks.
A.M.			°	°	K. F.	K. F.	K. F.	
1	2,050	1,800	—	—	218 800	6 500	5 7	From midnight to 4.
2	2,050	1,800	—	—	225 300	6 500	5 7	Fresh wind and moderate sea on port quarter. At 10 a.m. 220 miles of cable paid out. At 2·35 a.m. 230 miles of cable paid out. Distance run by patent log since yesterday noon until 4 a.m., 90 4/10 miles.
3	2,050	1,800	10 S.	13	232 824	6 963	6 0	
4	2,030	1,800	4 „	13	239 900	7 076	6 0	
5	2,030	1,800	„	12	246 600	6 713	6 1	(Signed) J. FOLLANSBEE.
6	2,030	1,800	„	„	252	6 —	5 4	
7	2,050	1,800	„	11	258 300	6 300	5 46	From 4 to 8 a.m.
8	2,000	1,800	5 S.	12	264 200	6 313	6 0	At 3·31 a.m. 230 miles cable paid out. At 7 a.m. 260 paid out; light breeze on port bow. Sea moderating. At 8 a.m. 113 4/10 miles run by patent log since yesterday noon. (Signed) M. KELLOGG.
9	2,040	1,800	9 „	10	268 750	6 550	6 1	
10	2,050	1,800	2 „	12	271 0	6 250	5 7	
11	2,040	1,800	8 „	10	278 170	6 170	5 7	From 8 to meridian.
12	2,050	1,800	2 „	13	284 730	7 560	6 4	At 8 44 a.m. 270 miles of cable paid out. At 10·20 a.m. 280 miles of cable paid out. At 11·01 a.m. 290 miles of cable paid out. Distance by patent log since noon yesterday, 127 4/10 miles. (Signed) J. FOLLANSBEE.
					291 0			

Lat. 51° 05′ N. Knots run by ship, 137. Excess of cable paid out, 22 843.
Long. 38° 29′ W. Knots cable paid out, 159 843. Per-centage of loss, 17.
Depth of water, 1,657 to 2,250

From meridian to 4 p.m.

At 1·14 p.m. 300 miles of cable paid out. At 2·35 p.m. 310 miles of cable paid out; light head breeze and moderate sea. At 4·02 p.m. 320 miles of cable paid out. (Signed) M. KELLOGG.

P.M.								
1	2,050	1,800	8 S.	12	298 400	6 633	5 4	From 4 to 6 p.m.
2	2,000	1,750	5 S.	12	305 600	7 200	5 6	Very light breeze on starboard bow, with but little sea. At 5·36 p.m. 330 miles of cable paid out. Commenced orlop circle at 5·40 p.m.
3	2,050	1,800	6 S.	12	311 900	6 300	6 0	(Signed) J. FOLLANSBEE.
4	2,050	1,800	8 S.	12	319 700	7 813	6 0	
5	2,075	1,800	10 S.	10	326 189	6 493	5 7	From 6 to 8 p.m.
6	2 075	„	„	10	332 070	5 903	5 4	At 7·12 p.m. 340 miles of cable paid out; distance run by patent log since meridian, 41 3/10 miles; light head breeze and moderate sea.
7	2,075	„	9 „	12	338 600	6 530	5 6	
8	2 030	„	7 S.	12	345 200	6 653	5 0	From 8 to midnight.
9	2,050	„	4 S.	12	351 670	6 420	5 5	At 8·45 p.m. 350 miles of cable paid out. At 10·16 p.m. 360 miles of cable paid out. At 11·46 p.m. 370 miles of cable paid out, (Signed) J. FOLLANSBEE.
10	2,030	„	straight	12	358 180	6 510	6 0	
11	2,000	„	5 S.	10	365 580	6 770	5 7	
12	2,000	„	9 S.	12	371 460	6 623	5 7	

AT SEA. Sunday, August 1, 1858.

A.M.								
1	2,075	1,800	—	—	378 300	6 858	6 3	From midnight to 4 a.m.
2	„	„	—	—	385 500	7 200	6 4	At 1·13 a.m. 380 miles of cable paid out. At 2·36 a.m. 390 miles of cable paid out. At 3·59 a.m. 400 miles of cable paid out; fresh breeze on starboard bow and moderate sea. (Signed) M. KELLOGG.
3	„	„	4 N.	12	392 500	7 470	5 6	
4	„	„	2 „	10	402 100	7 213	5 6	
5	2,000	„	10 „	10	407 300	7 200	6 0	From 4 to 8 a.m.
6	„	„	10 „	„	414 610	7 100	6 0	
7	„	„	10 „	„	421 500	6 903	6 2	Moderate breeze on starboard beam. At 5 a.m. patent log gave since noon yesterday, 99 miles. At 5·20 a.m. 410 miles of cable paid out. (Signed) J. FOLLANSBEE.
8	„	„	11 „	12	428 210	6 723	6 4	
9	2,075	„	10 „	„	434 300	6 090	6 0	
10	2,100	„	12 „	„	440 700	6 400	6 0	From 8 to meridian.
11	„	„	13 „	„	447 300	6 600	6 0	At 8·09 a.m. 420 miles of cable paid out. At 9·34 a.m. 440 miles of cable paid out. At 11·18 a.m. 450 miles of cable paid out; strong breeze and moderate sea on starboard beam. Ship rolling considerably; distance by patent log from meridian to meridian 141 2/10 miles. (Signed) M. KELLOGG.
12	„	„	11 „	11	456 400	8 613	6 0	

Lat. 50° 39′ N. Knots run by ship, 145. Excess of cable paid out, 19,683.
Long. 42° 55′ W. Knots cable paid out, 164 683. Per-centage of loss, 14.
Depth of water, 1,950 to 2,424.

From meridian to 4 p.m.

Strong breeze and heavy sea on starboard beam. At 12 30, 460 miles of cable paid out. At 1·55 p.m. 470 miles paid out. At 3·05 p.m. commenced paying out fore hold-coil. (Signed) J. FOLLANSBEE.

P.M.								
1	2,030	1,800	10 N.	12	463 700	7 500	6 3	From 4 to 6 p.m.
2	2,030	1,800	11 „	13	470 800	6 903	„ 4	At 4·45 p.m. 490 miles of cable paid out. Strong wind and heavy sea on starboard beam; ship rolling heavily. (Signed) M. KELLOGG.
3	2,050	1,800	10 „	12	477 303	6 723	„ —	
4	2,050	„	11 „	13	484 310	7 016	„ —	From 6 to 8 p.m.
5	2,050	„	70 „	12	491 800	7 490	„ —	Fresh breeze and moderate sea forward of starboard beam. At 6·05 p.m. 500 miles of cable paid out. At 7·32 p.m. 510 miles of cable paid out; distance since noon by patent log, 46½ miles. (Signed) J. FOLLANSBEE.
6	2 050	„	70 „	14	499 100	7 313	„ —	
7	2,000	„	11 „	12	506 455	6 923	„ —	
8	2,030	„	9 „	„	513 510	7 485	„ —	From 8 to midnight.
9	2,050	„	„	„	521 200	7 703	„ —	At 8 51 p.m., 520 miles of cable paid out. At 10·10 p.m. 530 miles paid out. At 11·35 p.m., 540 miles paid out. Wind moderate, heavy sea, ship rolling badly. (Signed) M. KELLOGG.
10	2,100	„	„	„	529 500	7 300	„ —	
11	2,075	„	„	„	535 800	7 300	„ —	
12	2,030	„	„	„	543 100	7 313	„ —	

AT SEA. Monday, August 2, 1858.

A.M.								
1	2,030	1,800	11 N.	13	550 420	7 320	5 4	From midnight to 4 a.m.
2	2,050	„	10 „	14	557 760	„ 340	6 4	Wind and sea moderate. At 12·56 a.m. 550 miles of cable paid out. At 2·18 a.m. 560 miles cable paid out. At 3·38 a.m. 570 miles paid out. Patent log, at 4 a.m. from noon yesterday, 106½ miles. (Signed) J. FOLLANSBEE.
3	2 030	„	11 „	12	565 160	„ 443	„ 4	
4	2 030	„	„	„	571 840	„ 640	„ „	
5	2,000	„	15 „	„	580 700	„ 873	„ „	
6	2,075	2,100	14 „	18	587 „	„ 300	5 6	From 4 to 8 a.m. At 4.53 a.m. 580 miles paid out. At 5·40, commenced on new cable, which being very dry, some difficulty was experienced in the running of the coil. At 6·21 a.m. 590 miles paid out. At 7·47 a.m. 600 miles paid out. Light breeze on starboard beam. Moderate sea, ship rolling considerably. (Signed) M. KELLOGG.
7	2,150	„	12 „	„	594 800	6 813	4 6	
8	2 150	„	„	13	601 600	7 100	„ 4	
9	2,200	2,300	10 „	„	608 750	„ 150	5 —	From 8 to meridian.
10	2 200	„	8 „	„	615 „	„ 030	6 —	At 9·11 a.m. 610 miles of cable paid out. At 10·25 a.m. 620 miles of cable paid out. At 11·40 a.m. 630 miles paid out. Patent log, at noon, 141 3/10 miles. (Signed) J. FOLLANSBEE.
11	2,200	„	10 „	12	623 850	8 070	„ 4	
12	2,150	2,100	8 „	„	633 850	9 6.3	5 2	From meridian to 4 p.m.

Lat. 49° 52′ N. Knots run by ship, 154. Excess of cable paid out, 23 150.
Long. 49° 27′ W. Knots cable paid out, 177.150. Per-centage of loss, 13.
Depth of water, 1,600 to 2,385.

At 1·50 p.m. 640 miles paid out. At 2·03 p.m. 650 miles paid out. At 3·16 p.m. 660 miles paid out. Light breeze on starboard beam, ship rolling considerably. (Signed) M. KELLOGG.

P.M.								
1	2,000	1,800	2 N.	10	641 400	7 861	6 4	From 4 to 6 p.m.
2	1,900	1,907	—	„	649 640	8 240	7 1	Light breeze and moderate sea forward of starboard beam. At 4·42 p.m. 670 miles paid out. At 6·09 p.m. 680 miles paid out.
3	1,850	1,800	—	„	657 900	260	„ „	(Signed) J. FOLLANSBEE.
4	1,850	1,800	5 N.	12	665 200	7 313	6 7	
5	1,900	1,800	straight	„	671 960	6 760	„ „	From 6 to 8 p.m.
6	1,857	„	9 N.	„	678 840	„ 943	5 4	At 7·40 p.m. 690 miles of cable paid out. Distance by patent log, since meridian, 47 7/10 miles. (Signed) M. KELLOGG.
7	1,857	„	4 „	11	685 500	„ 373	„ 4	
8	1,857	„	5 „	12	692 300	„ 813	6 —	
9	1,870	„	Could not see	„	699 030	„ 743	5 4	From 8 to midnight.
10	1,875	„	the cable	„	705 180	„ 150	6 —	Light breeze and moderate sea forward starboard beam. At 9·09 p.m. 700 miles of cable paid out. At 10·46 p.m. 710 miles paid out.
11	1,925	„	over the stern.	„	711 430	„ 240	„ 5	
12	1,875	„	„	„	717 550	„ 130	„ 6	(Signed) J. FOLLANSBEE.

APPENDIX No. 16—*continued.*

App. No. 16. Atlantic telegraph. Log of the U.S. steamer "Niagara."

U. S. STEAMER "NIAGARA."

AT SEA. Tuesday, August 3, 1858.

Hour.	Dynamometer Strain.	Brake Strain.	Angle of Rope. Hor.	Angle of Rope. Ver.	Rotometer.	Amount per hour by Rotometer.	Speed of Ship.
A.M.			°	°	K. F.	K. F.	K. F.
1	1,975	1,950	—	—	723 500	5 993	5 4
2	1,850	1,700	—	—	730 300	6 813	6 0
3	1,850	1,700	—	—	736 800	6 500	6 0
4	1,850	"	—	12	742 800	6 —	5 4
5	1,775	1,600	10 N.	"	749 300	6 493	5 6
6	1,600	"	8 "	"	755 640	7 260	6 0
7	1,650	"	8 "	"	762 590	6 963	5 4
8	1,600	"	8 "	11	770 510	6 963	6 0
9	1,450	1,450	—	12	775 100	5 993	4 4
10	1,450	1,350	5 "	13	780 800	5 700	5 0
11	1,200	1,200	—	11	787 100	6 313	3 0
12	1,200	1,200	—	10	795 300	8 003	5 0

Lat. 49° 17′ N. Knots run by ship, 147. Excess of cable paid out, 14·763.
Long. 49° 23′ W. Knots cable paid out, 161·763. Per-centage of loss, 10.
Depth of water, 742 to 1,827.

P.M.			°	°	K. F.	K. F.	K. F.
1	1,200	1,200	8 S.	12	801 880	6 880	6 4
2	"	"	8 S.	"	808 270	" 390	6 —
3	"	"	4 S.	"	814 700	" 430	6 4
4	"	"	4 S.	11	821 400	" 700	7 —
5	"	"	—	"	828 200	" 813	6 4
6	below 1,200	"	—	12	838 790	" 990	6 2
7	below 1,200	"	10 S.	12	841 370	" 393	6 —
8	below 1,200	"	4 S.	13	848 400	" 643	6 2
9	1,000	1,000	—	—	855 000	7 400	7 6
10	900	"	—	—	861 400	6 100	5 6
11	1,000	"	—	—	864 504	3 300	3 2
12	"	"	—	—	870 800	5 213	6 2

From midnight to 4 a.m.

At 12·28, 720 miles paid out. At 1·57 a.m., 730 miles paid out; distance by patent log, from 8 a.m. to 4 a.m. 43 1/10 miles. At 3·30 a.m. 740 miles paid out. Light breeze on starboard beam and heavy swell; ship rolling moderately. (Signed) M. KELLOGG.

From 4 to 8 a.m.

Light breeze and moderate sea forward, starboard beam. At 5·5 a.m. 750 miles paid out. At 6·29 a.m. 760 miles paid out. At 7·36 a.m., 770 miles paid out. (Signed) J. FOLLANSBEE.

From 8 to meridian.

At 8·26 a.m., cable all paid out from the forward coil, and commenced on the after circle in ward-room. At 9·51 a.m., 780 miles of cable paid out. At 11·25 a.m. 790 miles paid out. Distance run by patent log, 134¾ miles. (Signed) M. KELLOGG.

From meridian to 4 p.m.

At 12·44 p.m. 800 miles paid out. At 2·15 p.m. 810 miles paid out. At 3·47 p.m. 820 miles paid out. Distance by patent log, 24 4/10 miles. At 5·15 p.m. 830 miles paid out. (Signed) J. FOLLANSBEE.

From 6 to 8 p.m.

Nearly calm and smooth sea during the watch. At 6·47 p.m. 840 miles paid out. Distance at 8 p.m., since noon, by patent log, 30 3/10 miles. (Signed) J. FOLLANSBEE.

From 8 to midnight.

At 8·15 p.m. 850 miles paid out. At 9·10 p.m. 860 miles of cable paid out. At 9·30 p.m. received signal from "Agamemnon" that she was in sounding. At 12, 870 miles paid out. Distance run by patent log, 15 7/10 miles. (Signed) M. KELLOGG.

AT SEA. Wednesday, August 4, 1858.

A.M.					K. F.	K. F.	K. F.
1	1,100	1,000	Too dark to see the cable.		876 410	6 410	6 2
2	1,100	1,000			883 020	6 623	6 4
3	1,100	1,000	8 S.	13	889 140	6 120	6 —
4	1,000	1,000	4 S.	12	895 910	6 770	6 —
5	900	900	4 S.	11	901 —	5 703	5 6
6	500	500	—	11	906 800	5 890	5 6
7	900	900	—	10	913 500	6 713	6 2
8	900	900	—	11	920 —	6 513	6 2
9	850	800	straight	13	926 630	6 630	6 4
10	850	800	6 S.	13	933 910	7 270	6 6
11	800	800	6 S.	12	941 —	7 113	7 —
12	800	800	"	12	941 —	— "	" 4

Lat. 48° 17′ N. Knots run by ship, 146. Excess of cable paid out, 8 360.
Long 52° 49′ W. Knots cable paid out, 154·360. Per-centage of loss, 6.
Depth of water, less than 200 fathoms.

P.M.			°	°	K. F.	K. F.	K. F.
1	800	800	4 S.	12	937 000	7 253	7 —
2	800	800	—	15	964 860	7 860	7 —
3	1,000	1,000	—	13	969 580	4 883	4 —
4	800	800	—	12	973 000	3 5·3	3 —
5	800	800	2 S.	11	976 820	3 820	3 6
6	800	800	straight	11	983 000	6 253	5 6
7	600	600	—	—	991 100	6 040	5 6
8	600	600	—	—	994 700	3 600	2 6
9	400	400	—	—	998·8 270	4 683	1 6
10	400	400	—	—	1,001 080	2 733	2 6
11	400	400	—	—	1,005 410	4 260	3 —
12	400	400	—	—	1,009 600	4 190	3 —

From midnight to 4 a.m.

Nearly calm and smooth sea. At 1·38 a.m. 880 miles of cable paid out. At 3·03 a.m. 890 miles of cable paid out. Patent log at 4 a.m. 21 6/10 miles from noon yesterday. (Signed) J. FOLLANSBEE.

From 4 to 8 a.m.

At 4·48 a.m. 900 miles of cable paid out. At 6·25 a.m. 910 miles of cable paid out. At 8 a.m. 920 miles of cable paid out; distance by patent log from meridian, 115 7/10 miles. (Signed) J. KELLOGG.

From 8 to meridian.

At 9·29 a.m. 930 miles of cable paid out. At 10·51 a.m. 940 miles of cable paid out. At 12·05 a.m. 950 miles of cable paid out. Patent log at noon, for 24 hours, 142 miles. (Signed) J. FOLLANSBEE.

From meridian to 4 p.m.

At 1·25 p.m. 960 miles of cable paid out. At 3·10 p.m. 970 miles of cable paid out. Patent log, from meridian to 4 p.m., 21 6/10 miles. (Signed) M. KELLOGG.

From 4 to 6 p.m.

At 4.50 p.m. changed from ward-room to quarter-deck coil, in order to cut out a fault which had been discovered yesterday. At 5·10 p.m. 980 miles of cable paid out. (Signed) J. FOLLANSBEE.

From 6 to 8 p.m.

At 6·41 p.m., 990 miles of cable paid out. Distance by patent log, since meridian, 41 1/10 miles. (Signed) M. KELLOGG.

From 8 to midnight.

At 9·28 p.m., 1,000 miles of cable paid out. At midnight had run since noon, by patent log, 58 miles. At 12·06 1,010 miles of cable paid out. (Signed) J. FOLLANSBEE.

AT SEA. Thursday, August 5, 1858.

A.M.			°	°	K. F.	K. F.	K. F.
1	400	400	—	—	1,013 600	4 000	—
2	—	—	—	—	1,016 600	3 000	—
3	—	—	—	—	—	—	—
4	—	—	—	—	—	—	—
5	—	—	—	—	—	—	—
6	—	—	—	—	—	—	—
7	—	—	—	—	—	—	—
8	—	—	—	—	—	—	—
9	—	—	—	—	—	—	—
10	—	—	—	—	—	—	—
11	—	—	—	—	—	—	—
12	—	—	—	—	—	—	—

From midnight to 4 a.m.

At 1·43 a.m. ship came to anchor off Telegraph House, Bay of Bull's Arm. At 3·30 a.m. coiled 1¼ miles of cable on quarter-deck, preparatory to it e end being taken on shore. End of cable was landed on shore at 7·15 a.m. in "Niagara's" boats. (Signed) M. KELLOGG.

Distance by patent log, since meridian, 64 miles. Total distance run since making splice, 882 miles. Total amount of cable paid out, 1,016 miles 600 fathoms. Total per-centage of cable paid out over distance run, 15¾. Total Amount paid out from "Agamemnon," as per signal, 1,010 miles. During the day three miles cable extra was sent on shore, at the request of Mr. de Sauty, for future use.

APPENDIX No. 16—*continued.*

APP. No. 16. Atlantic telegraph. Log of "Agamemnon," unsuccessful trip, 1858.

Log of paying out ATLANTIC CABLE from AGAMEMNON. Unsuccessful Trip in 1858.

Date.	Greenwich Time. H.	M.	Ship's Time. H.	M.	Speed of Ship by Log. Miles.	No. of Revolutions of Screw per Minute.	No. of Revolutions of Machine per Minute.	Cable payed out. Mil.	Fath.	Indicated Strain in pounds.	Weight upon Brake in pounds.	Angle of Cable. Vert.	Hor.	Flakes payed out.	Splices and Lappings payed out.	Latitude.	Longitude.	Distance run by Massey.	Distance made good at Noon.	REMARKS.
	2	55	—		2·0	—	22	—	600	—	—	Went on board the Niagara, and found that the accident had been owing to a want of tension in the cable entering the machine, which allowed it to get out of the leading-on groove, and into the second groove of same drum. In trying to replace it it was allowed to get off the drum on the outside, where it was caught by a large projecting lever for working the scrapers, and broken. W. THOMSON.								12.25 ship's time, 2.36 Greenwich time, splice finished, lowering process, veering out until 200 fathoms payed out; 2.55, steaming slowly away.
June 26	3	30	—		—	—	30	2	870	1800*	1666									
	3	35	—		—	ceased		3	250	2500	,,									
	7	35	Splice lowered, and 150 fathoms payed out.									—	—	—	—	—	—	—	—	
	7	50	Started, P.M.		2·0	—	23	—	—	—	1666							Massey's Log set.	—	Three and a half miles to be deducted from the amount of cable registered as payed out, having been lost in the first instance.
	8	0	5	45	1·8	—	26	4	650	1998	,,	—	—	—	—	—	—	—	—	
	8	30	6	15	2·0	—	23	7	140	1530	2366	—	—	3¼	—	—	—	—	—	
	9	0	6	45	2·0	13	25	9	400	1838	,,	25	—	—	—	—	—	—	—	
	9	30	7	15	2·0	13	21	10	200	1838	,,	26	—	—	—	—	—	—	—	
	10	15	8	0	2·0	13	19	13	400	1838	,,	30	—	—	—	—	—	—	—	
	10	40	8	20	2·8	16	20	15	—	1838	,,	20	20	—	—	—	—	—	—	10.45. Screw increased to 18.
	11	10	9	0	3·0	22	27	16	600	1838	,,	17	8	—	—	—	—	—	—	11.7. ,, ,, 22.
	11	45	9	20	3·5	26	30	19	100	1838	,,	16	5	—	—	—	—	—	—	Weights increased to 2466.
June 27	A.M. 12	12	10	0	4·0	28	31	21	200	1838	2466	16	5	—	—	—	—	—	—	11.55. Machine decreased to 26 revolutions per minute.
	12	43	10	20	4·0	28	33	24	—	1838	,,	15	4	—	—	—	—	—	—	12.5. Screw increased to 28.
	1	11	11	0	4·0	28	32	26	600	1838	,,	10¼	4	—	—	—	—	—	—	
	1	41	11	20	4.0	28	31	29	300	1838	,,	13	—	—	—	—	—	—	—	
	2	11	midnt. 12	0	4·0	28	32	32	—	1838	,,	13	—	16	—	—	—	—	—	All going well; gave up charge to Mr. Canning. (C. T. BRIGHT.
												Vert.	Hor.							Revolutions of screw increased to 24 at 3 p m.
	2	42	12	20	4·2	28	36	34	380	1838	,,	15	—	—	—	—	—	—	—	
	3	12	1	0	4·0	24	30	38	70	1838	,,	14	4	—	—	—	—	—	—	
	3	42	1	20	4·0	24	26	41	300	2330	,,	—	—	—	—	—	—	29½	—	
			Reported continuity broken at 3.44 a.m. No variation in dynamometer. B. H.																	
			At 3.44 reported by Professor Thomson want of continuity, and in his opinion that the cable had parted. Maximum strain on the cable, 2450 lbs., cable having payed out easily and without the slightest appearance of hitch or accident, at after consultation with Mr. Bright and Professor Thomson, put stopper on cable by stern wheel, to enable us to cut cable as near the machinery as possible for Professor Thomson to test for distance of point of accident from ship. After holding on for a few minutes the cable parted by stern wheel, there being a breeze on at the time, which made it difficult to keep cable perpendicular over the stern of the ship. SAMUEL CANNING.																	
			At 3.25, Greenwich time, when signals had been coming from the Niagara regularly each minute, the galvanometers came to zero, instead of indicating the last of the reversals. During the next ten minutes the operations were continued on board this ship on the regular plan, but the incisions of the instruments corresponded to earth about the middle of the cable, and after 3.40 the signals due from the Niagara did not come. These indications leave no doubt but that some point of the conductor near the middle must quite suddenly have lost insulation. After the part which had been paid out from the Agamemnon was cut away, the part of the circuit remaining on board was tested and found perfect. W. THOMSON.																	
June 28	9	45	7	35	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Made splice and lowered out 150 fathoms, 50 being used for bringing round cable for splice and lowering.
	10	17	8	0	2·9	12	26	1	506	1838	2266*	25	8	—	—	At time of making splice. 52 6	33 8	—	—	Engine going 14 revolutions, could not go slower on account of wind being aft.
	10	45	8	30	2·0	14	33	4	200	1838	2266	21	8	—	—	—	—	—	—	Ship pitching. 11.55 p.m., weights on break reduced to 2066 lbs.
	11	15	9	0	2·8	14	32	6	900	1838	,,	21	5	—	—	—	—	—	—	12.25 p.m., strain hardly sufficient to draw out cable at 3½ knots, ship going 2·8.
June 29	A.M. 12	15	13	6	2·5	14	26	11	500	1838	2266	23½	5	—	—	—	—	—	—	12.59 a.m., screw increased to 26 revolutions per minute, at 12.0 gave up charge to Mr. Canning, all going well during above time. (Signed) CHARLES T. BRIGHT.
	12	43	10	20	2·8	16	24	13	500	1838	2266	23	10	—	—	—	—	—	—	
	1	15	11	0	3·5	20	30	15	900	1838	,,	16	5	—	—	—	—	—	—	
	1	45	0	0	4·0	26	33	19	600	2046	,,	14	1	—	—	—	—	—	—	
	2	15	12	0	4·0	26	33	21	450	1748	,,	15	1½	—	—	—	—	—	—	
	2	45	12	30	3·6	26	33	24	190	2530	,,	14	1½	—	—	—	—	—	—	
	3	15	1	0	3·6	26	33	26	660	2426	,,	14	,,	—	—	—	—	—	—	
	3	45	1	30	3·5	—	32	29	506	1852	,,	15	,,	—	—	—	—	—	—	During this time there was no alteration in the speed of the ship or the strain on the cable. Electrical signals reported perfect. (Signed) S. CANNING.
	4	15	2	6	3·6	—	32	31	660	2025	,,	15	2	—	—	—	—	—	—	
	4	45	2	20	3·5	27	30	34	730	2025	,,	,,	,,	—	—	—	—	—	—	
	5	15	3	0	3·5	—	32	37	100	2025	,,	,,	,,	—	—	—	—	—	—	
	5	45	3	20	3·5	—	30	39	900	2025	,,	,,	4	Dark coil	—	—	—	—	—	
	6	15	4	0	3·5	25	20	42	0	2025	,,	,,	,,	60th	—	—	—	—	—	
	6	45	4	30	3·8	28	34	45	200	2426	2266	12	1	flake off.	—	—	—	—	—	
	7	15	5	0	4·0	28	31	47	950	2185	,,	12½	1	—	—	—	—	—	—	6.30. weight on brakes increased to 2366 pounds.
	7	45	5	30	4·0	28	33	50	650	2185	,,	12¼	½	—	—	—	—	—	—	
	8	15	6	0	4·0	28	33½	53	506	2185	,,	14	½	—	—	—	—	—	44½	8.54, 100 pounds added to brakes.
	8	43	6	20	4·1	28	36	56	440	2113	,,	12	2	—	—	—	—	—	—	
	9	15	7	0	4·2	28	31	59	340	2125	2466	14½	4	—	—	—	—	—	—	9.20, 200 pounds removed from brakes.
	9	23	7	8	4·0	28	32	60	0	2426	2266	,,	,,	—	—	—	—	—	—	
	9	41	7	27	4·0	28	31	61	800	2426	,,	14	3	—	—	—	—	—	—	All right during above time. (Signed) CHARLES T. BRIGHT.
	10	15	8	0	3·8	28	32	64	800	2026	,,	14	1	—	—	—	—	—	52	
	10	45	8	30	3.7	26	32	67	500	2053	,,	14	2	—	—	—	—	—	—	10.40 G.T., hoisted fore trysail, and gib. Reduced engine to 26 revolutions.
	11	15	9	0	4·0	26	34	70	400	2325	,,	14	3	—	—	—	—	—	—	11.3, hoisted foresail.
	11	45	9	30	4·5	26	36½	73	280	2026	,,	13	2½	—	—	—	—	—	—	12.0, increased weight on lever brake to 2466 pounds. Depth of water increasing.
	P.M. 12	15	10	0	4·5	25	37	76	408	2292	2466	13	2½	—	—	—	—	—	—	1.8, increased speed of engine to 28 revolutions.
	12	45	10	30	4·3	26	36	79	540	2125	,,	12½	2	—	—	—	—	—	—	

* Eased by hand-wheel.

APP. No. 16. Atlantic telegraph. Log of "Agamemnon," 1858.

APPENDIX No. 16—*continued.*

LOG of paying out the ATLANTIC CABLE from AGAMEMNON—*continued.*

Date.	Greenwich Time. H. M.	Ship's Time. H. M.	Speed of Ship by Log. Miles.	No. of Revolutions of Screw per Minute.	No. of Revolutions of Machine per Minute.	Cable payed out. Mil. Fath.	Indicated Strain in pounds.	Weight upon Brakes in pounds.	Angle of Cable. Vert.	Angle of Cable. Hor.	Flakes payed out.	Splices and Lap-plugs payed out.	Latitude. D. M.	Longitude. D. M.	Distance run by Massey.	Distance made good at Noon.	REMARKS.
	p.m.	a.m.															
	1 15	11 0	4·0	26	38	82 600	2129 / 2173	2466	14¼	2½	—	—	—	—	—	—	1.26, increased weight on brakes to 2,666 pounds, as machine was going 40 revolutions per minute.
		p.m.															
	1 45	11 30	4·5	22	39½	86 —	2222 / 2266	2666	11¾	1½	—	—	—	—	—	—	1.40, speed of screw reduced 2 revolutions, in consequence of cable vibrating.
	2 15	12 0	—	24	—	— —	—	,,	—	—	—	—	—	—	—	—	
		noon															
	2 8	12 0	3·8	24	34	88 150	2180 / 2222	2666*	14¾	4½	67	—	52 12	31 11	70 9	73	1.47, speed of screw reduced 2 revolutions.
																	2 p.m., increased speed of screw to 24 revolutions per minute.
																	During the watch between 8 a.m. and noon, the speed of the engine was often altered, on account of the variableness of the force of the wind. (Signed) S. CANNING.
		p.m.															
	2 18	12 10	4·5	24	40	89 500	2203 / 2248	,,	,,	,,	—	—	—	—	—	—	
	2 56	12 40	3·5	26	36	92 —	2248 / 2292	,,	15½	1½	—	—	—	—	—	—	Set brakes up a little, and increased number of revolutions of screw.
	3 25	1 10	3·5	26	34	95 500	2292 / 2337	,,	13½	1½	—	—	—	—	—	—	
	3 40	1 30	3·8	28	36	96 500	2292 / 2337	,,	,,	,,	—	—	—	—	—	—	3.7, decreased speed 2 revolutions, the log giving 4¼; good breeze freshening.
	4 0	1 49	3·8	30	38	98 500	2292 / 2337	,,	13	,,	—	—	—	—	—	—	
	4 20	2 10	4·0	,,	,,	100 700	2337 / 2381	,,	,,	—	—	—	—	—	—	—	2·55, increased speed 2 revolutions. (Signed) CHARLES T. BRIGHT.
	4 40	2 30	4·6	28	40	102† 700	2337 / 2381	,,	,,	3	—	—	—	—	—	—	
	4 50	2 40	4·6	28	40	104† 400	2337 / 2381	,,	11	1	—	—	—	—	—	—	
	5 10	3 0	4·2	30	,,	106 100	2337 / 2381	,,	14	1	—	—	—	—	—	—	
	5 25	3 15	4·4	,,	39	— —	2381 / 2425	2866	12	1½	—	—	—	—	—	—	200 pounds additional put on brakes not fully set up.
	5 50	3 40	4·2	,,	,,	110 600	2425 / 2469	,,	—	—	—	—	—	—	—	—	During the watch from noon to 4 p.m. all has gone well. Electrical signals reported all right. (Signed) CHARLES T. BRIGHT.
	6 10	4 0	,,	,,	,,	112 380	2469 / 2513	,,	14	4	80	—	—	—	70 9	—	7.10, 200 pounds removed from brakes.
	6 40	4 30	4·0	,,	38	115 —	2513 / 2557	,,	13	1¼	—	—	—	—	18 8†	—	During this watch, from 4 p.m. to 6 p.m., all has gone well.—H. C.
	7 15	5 6	4.2	,,	36	119 500	2513 / 2557	2666	11	2	—	—	—	—	89 7	—	
	7 45	5 30	4·5	,,	38½	122 700	2513 / 2557	,,	12½	3½	—	—	—	—	—	—	Electrical signals reported perfect: S. CANNING.
	8 ‡	5 45	4·8	,,	36	124 60	2513 / 2557	,,	12½	3	—	—	—	—	—	—	
	8 30	6 15	4·8	,,	36	127 600	2513 / 2557	,,	12½	4½	91	—	—	—	—	—	Rotometer when started after repairing 6.30 a.m., 8.45 o'clock, 41,484. Indicator 129,405.
	9 0	6 50	4·8	,,	37	130 500	2513 / 2557	,,	,,	—	—	—	—	—	—	—	
	9 30	7 20	5·0	,,	38	133 500	2513 / 2557	,,	12	,,	—	—	—	—	—	—	9·45, in the 24 hours, since starting, payed out 135 miles 450 fathoms.
	9 45	7 35	5·0	,,	36	135 450	2513 / 2557	,,	12	1	—	—	—	—	—	—	
	10 16	8 0	5·0	,,	37	138 —	2513 / 2557	,,	13	1	97	a lapping at 97 flake.	—	—	70 9† 89 2	—	During the watch from 6 p.m. to 8 p.m. all well. (Signed) CHARLES T. BRIGHT.
	10 40	8 30	4·4	,,	34	141 300	2513 / 2557	,,	12	1	—		—	—	—	—	
	11 10	9 0	4·0	28	32	144 150	2513 / 2557	,,	12	—	—	—	—	—	110 1	—	11·20 p.m., eased screw 2 revolutions.
	11 40	9 30	3·0	20	22	146 556	2513 / 2557	,,	12	—	—	—	—	—	—	—	11·35, eased again to 20.
																	11.45, cable parted 6 fathoms over the ship's stern. The speed of the ship had been reduced to enable us to pass more readily the b'ght from the deck coil to the coil in the main hold. The strain indicated by dynamometer at the time of the cable parting being 2,200 pounds, the number of revolutions of the screw being 20, and of the machine 24 per minute. (Signed) S. CANNING.
																	The speed of the ship reduced; the dynamometer indicated 2,500 pounds on the cable parting, varying from 1,500 to 2,200 pounds previously. C. MOORE.

* Brakes slack. † Run since noon. ‡ Index hand for miles does not point quite true.

APPENDIX No. 16—*continued.*

App. No. 16. Atlantic telegraph. Result of testings on the Newfoundland end.

RESULT of the TESTINGS on the NEWFOUNDLAND END of the ATLANTIC CABLE ACCORDING to INSTRUCTIONS from C. F. VARLEY, ESQ.—OCTOBER 1859.

October 22nd, 1859.

Resistance Test.—Zinc to Line.

	Deflection. °
Galvanometer A needle at	0
„ 1 cell short circuit	32 L.
Cable current	2 R.
12 cells into line	80½ L.
Through resistance = 14½⅛ cable	80½ L.
24 cells into line	87 L.
Through resistance = 12½⅛ cable	87 L.
Cable current	5 R.

	Deflection. °
Galvanometer B needle at	2 R.
„ 1 cell short circuit	3 L.
	0°
12 cells into line	35 L.
Through resistance = 14½⅛ cable	35 L.
24 cells into line	59 L.
Through resistance = 10½⅛ cable	59 L.
48 cells into line	75 L.
Through resistance = 6 ⅝ cable	75 L.
Cable current	3 R.

Resistance Test.—Copper to Line.

	Deflection. °
Put 24 cells copper to line for one hour to drive up resistance of cable to maximum.	
Galvanometer needle A at	0
„ 1 cell short circuit	32 L.
Cable current	0 L.
12 cells copper to line	6 L.
Through resistance = 1773 miles cable	6 L.

	Deflection. °
24 cells copper to line	20 L.
Through resistance = 1134 miles cable	20 L.
36 cells copper to line	30 L.
Through resistance = 1134 miles cable	30 L.
48 cells copper to line	44 L.
Through resistance = 910 miles cable	44 L.
Cable current	½ L.

October 23rd.

Differential Tests.

	Deflection. °
12 cells zinc to line and through resistance = 21¾ miles cable	0
24 cells zinc to line and through resistance = 14¾ miles cable	0
48 cells zinc to line and through resistance = 10½ miles cable	0
24 cells copper to line for one hour.	
12 cells copper to line and through resistance = 1404¾ miles cable	0
24 cells copper to line and through resistance = 1023¾ miles cable	0
48 cells copper to line and through resistance = 767 miles cable	0
24 cells zinc to line for one hour to bring cable to original state.	

Return current Tests.

	Deflection. °
12 cells zinc to line Charge, 5 seconds - return current	50 L.
24 cells zinc to line Charge, 5 seconds - return current	50 L.
48 cells zinc to line Charge, 5 seconds - return current	55 L.

Return Current Tests with varying Resistances.

	Deflection. °
12 cells zinc to line. 5 seconds charge return current 0 resistance	0 L.
„ 1 Varley's resistance	5 L.
„ 2 „ „	7 L.
„ 5 „ „	20 L.
„ 10 „ „	32 L.
„ 20 „ „	43 L.
„ 30 „ „	49 L.
„ 50 „ „	53 L.
„ 100 „ „	55 L.
„ 200 „ „	56 L.
„ 400 „ „	57 L.
„ infinite resistance	57 L.

	Deflection. °
24 cells zinc to line 5 seconds charge return current 0 resistance	0
„ 1 Varley's resistance	10 L.
„ 2 „	18 L.
„ 5 „	30 L.
„ 10 „	41 L.
„ 20 „	49 L.
„ 30 „	53 L.
„ 50 „	55 L.
„ 100 „	57 L.
„ 200 „	58 L.
„ 400 „	59 L.
„ infinite resistance	59 L.
48 cells, zinc to line, 5 seconds charge. return current 0	0
„ 1 Varley's resistance	10 L.
„ 2 „	20 L.
„ 5 „	32 L.
„ 10 „	43 L.
„ 20 „	49 L.
„ 30 „	53 L.
„ 50 „	55 L.
„ 100 „	58 L.
„ 200 „	59 L.
„ 400 „	59 L.
„ infinite resistance	59 L.
48 cells copper to line for one hour.	

Resistance Test.

	Deflection. °
12 cells copper to line	4 L.
Through resistance = 1903¾ miles cable	9 L.
24 cells copper to line	7 L.
Through resistance = 1903¾ miles cable	18 L.
48 cells copper to line	8 L.
Through resistance = 1903¾ miles cable	25 L.

App. No. 16.
Atlantic telegraph.
Result of testings on the Newfoundland end.

Appendix No. 16—*continued.*

October 24th.

Resistance Test.—Zinc to Line.

	Deflection. °		Deflection. °
Galvanometer A needle at	0	Galvanometer B needle at	2 R.
,, 1 cell short circuit	34 L.	,, 1 cell short circuit	3 L.
Cable current	0		0
12 cells into line	81 L.	12 cells into line	32½ L.
Through resistance = 12$\frac{1}{19}$ cable	81 L.	Through resistance = 12$\frac{1}{19}$ cable	32½ L.
24 cells into line	87 L.	24 cells into line	55½ L.
Through resistance = 10$\frac{1}{19}$ cable	87 L.	48 cells into line	55½ L.
		Through resistance = 6$\frac{4}{19}$ cable	74 L.
Cable current	6 R.	Cable current	0

Resistance Test.—Copper to Line.

Put 48 cells copper to line, to drive up resistance of cable to maximum.

Galvanometer A needle at zero.

	Deflection. °
,, 1 cell, short circuit	34 L.
Cable current	0 L.
12 cells copper to line	4 L.
Through resistance = 1903$\frac{3}{4}$ miles cable	6 L.
24 cells copper to line	5½ L.
Through resistance = 1903$\frac{3}{4}$ miles cable	13 L.
36 cells copper to line	7½ L.
Through resistance= 1903$\frac{3}{4}$ miles cable	12½ L.
48 cells copper to line	10½ L.
Through resistance = 1903$\frac{3}{4}$ miles cable	20½ L.
Cable current	0

Put 48 cells zinc to line for 1 hour.

Differential Tests.

	Deflection. °
12 cells zinc to line and through resistance = 17 miles cable	0
24 cells zinc to line and through resistance = 11$\frac{1}{4}$ miles cable	0
48 cells zinc to line and through resistance = 9 miles cable	0
48 cells copper to line for one hour.	
12 cells copper to line and through resistance = 1903$\frac{3}{4}$ miles cable	0
24 cells copper to line and through resistance = 1903$\frac{3}{4}$ miles cable	50 L.
48 cells copper to line, and through resistance = 1903$\frac{3}{4}$ miles cable	63 L.
48 cells zinc to line for 1 hour to bring cable to its original state.	

Return Current Tests.

	Deflection. °
12 cells zinc to line, Charge, 5 seconds - return current	55 L.
24 cells zinc to line, Charge, 5 seconds - return current	57 L.
48 cells zinc to line, Charge, 5 seconds - return current	57 L.

Return Current Tests with varying Resistances.

	Deflection. °
12 cells zinc to line 5 seconds charge.	
Return current 0 resistance	0
,, 1 Varley's resistance	8 L.
,, 2 ,,	16 L.
,, 5 ,,	28 L.
,, 10 ,,	37 L.
,, 20 ,,	44 L.
,, 30 ,,	49 L.
,, 50 ,,	51 L.
,, 100 ,,	54 L.
,, 200 ,,	56 L.
,, 400 ,,	56 L.
,, infinite resistance	58 L.
24 cells zinc to line, 5 seconds charge.	
Return current 0 resistance	0
,, 1 Varley's resistance	12 L.
,, 2 ,,	20 L.
,, 5 ,,	35 L.
,, 10 ,,	45 L.
,, 20 ,,	49 L.
,, 30 ,,	51 L.
,, 50 ,,	55 L.
,, 100 ,,	58 L.
,, 200 ,,	59 L.
,, 400 ,,	59 L.
,, infinite resistance	60 L.
48 cells zinc to line, 5 seconds charge.	
return current 0	0
,, 1 Varley's resistance	12 L.
,, 2 ,,	22 L.
,, 5 ,,	36 L.
,, 10 ,,	46 L.
,, 20 ,,	52 L.
,, 30 ,,	54 L.
,, 50 ,,	56 L.
,, 100 ,,	59 L.
,, 200 ,,	60 L.
,, 400 ,,	60 L.
,, infinite resistance	61 L.

App. No. 16. Atlantic telegraph. Result of lettings on the Newfoundland end.

October 25th.

Resistance Test.—Zinc to Line.

	Deflection.		Deflection.
Galvanometer A needle	0	Galvanometer B needle	2 R.
1 cell short circuit	33½ R.	1 cell short circuit	2 L.
Cable current	0		0
12 cells into line	79½ L.	12 cells into line	22½ L.
Through resistance = 21$\frac{1}{24}$ cable	79½ L.	Through resistance = 31$\frac{1}{12}$ cable	22½ L.
24 cells into line	87 L.	24 cells into line	57 L.
Through resistance = 16$\frac{9}{24}$ cable	87 L.	Through resistance 8$\frac{1}{24}$ cable	57 L.
		48 cells into line	72½ L.
		Through resistance = 6$\frac{5}{48}$ cable	72½ L.
Cable current	3 R.	Cable current	0

Resistance Test.—Copper to Line.

	Deflection.
Put 48 cells copper to line for 1 hour, to drive up resistance of cable to maximum.	
Galvanometer A needle at zero.	
1 cell short circuit	33 L.
Cable current	½ L.
12 cells copper to line	8½ L.
Through resistance = 1647$\frac{1}{2}$ miles cable	8½ L.
24 cells copper to line	12½ L.
Through resistance = 1903$\frac{1}{2}$ miles cable	13½ L.
36 cells copper to line	18 L.
Through resistance = 1903$\frac{1}{2}$ miles cable	20 L.
48 cells copper to line	20 L.
Through resistance = 1903$\frac{1}{2}$ miles cable	25 L.
Cable current	½ L.
48 cells zinc to line for one hour.	

Differential Tests.

	Deflection.
12 cells zinc to line and through resistance = 16$\frac{1}{2}$ miles cable	0
24 cells zinc to line and through resistance = 11$\frac{1}{4}$ miles cable	0
48 cells zinc to line and through resistance=8$\frac{1}{4}$ miles cable	0
48 cells copper to line for one hour.	
12 cells copper to line, and through resistance = 1903$\frac{1}{2}$ miles cable	57
24 cells copper to line, and through resistance = 1903$\frac{1}{2}$ miles cable	62
48 cells copper to line, and through resistance = 1903$\frac{1}{2}$ miles cable	75
48 cells zinc to line for 1 hour, to bring cable to its original state.	

Return Current Tests.

	Deflection.
12 cells zinc to line.	
Charge, 5 seconds - return currents	57 L.
24 cells zinc to line.	
Charge, 5 seconds - return current	59 L.
48 cells zinc to line.	
Charge, 5 seconds - return current	59 L.

Return Current Tests with varying Resistances.

			Deflection.
12 cells zinc to line, 5 seconds charge.			
return current	0	resistance,	0
,,	1	Varley's resistance	11 L.
,,	2	,,	20 L.
,,	5	,,	36 L.
,,	10	,,	45 L.
,,	20	,,	52 L.
,,	30	,,	55 L.
,,	50	,,	56 L.
,,	100	,,	59 L.
,,	200	,,	60 L.
,,	400	,,	60 L.
,,	infinite resistance		60 L.
24 cells zinc to line, 5 seconds charge.			
return current	0	resistance,	0 L.
,,	1	Varley's resistance	13 L.
,,	2	,,	24 L.
,,	5	,,	40 L.
,,	10	,,	47 L.
,,	20	,,	54 L.
,,	30	,,	55 L.
,,	50	,,	57 L.
,,	100	,,	59 L.
,,	200	,,	60 L.
,,	400	,,	60 L.
,,	infinite resistance		60 L.
48 cells zinc to line, 5 seconds charge.			
return current	0	resistance,	0
,,	1	Varley's resistance	15 L.
,,	2	,,	25 L.
,,	5	,,	40 L.
,,	10	,,	47 L.
,,	20	,,	53 L.
,,	30	,,	57 L.
,,	50	,,	58 L.
,,	100	,,	59 L.
,,	200	,,	59 L.
,,	400	,,	61 L.
,,	infinite resistance.		61 L.

(Signed) H. SAUNDERS.

REPORT on the STATE of the ATLANTIC CABLE in TRINITY BAY, by CROMWELL F. VARLEY and J. KELL.

Report on the state of the Atlantic cable in Trinity Bay, by C. F. Varley and J. Kell.

The Atlantic Telegraph Company,
St. John's, Newfoundland, July 3, 1860.

After repeated attempts to raise the cable, by grapnelling, in order to test its electrical condition, and with a view to haul it at New Perlican, as instructed by the Board, we regret having to report that although we have on many occasions been able to raise the bight, and so get on board at different times pieces of cable, in all amounting to about seven miles, we have invariably found it broken again a few miles off.

The log, which Captain Kell will furnish, will give the details of the proceedings in full.

The weather up to the 12th of June had been so bad that grapnelling operations were impracticable, and even while writing is cold and unsettled, the season here having been unusually late and boisterous.

On the 17th of June water was frozen in the pails during the night; the noon following, however, was oppressively hot. Cold, dense fogs, and strong winds have been frequent ever since.

The plan of operations was as follows:—

On the 12th of June Captain Kell succeeded in fishing up and buoying the end, after recovering three quarters of a mile of cable.

Appx. No. 1. Report on the state of the Atlantic Cable of Trinity Bay, by C. F. Varley and J. Kell.

On the 14th operations were resumed, and three miles and a half of cable recovered, when the old fault, the fracture, already reported to you by Captain Kell, came on board. A fog buoy was anchored at the spot where the same came up.

On the 20th, Mr. Varley arrived at New Perlican, and Captain Kell also. A consultation was immediately held on this and the following day, when it was unanimously agreed that the assistance of a steamer and extra men should be obtained. Captain Kell having returned, started for Bull's Arm, and Mr. Varley left for St. John's on the 21st.

On the 22nd, the cable was hooked in 30 fathoms and parted both ways, the bight and a short piece of cable coming on board.

23rd.—The cable was hooked again, but parted when within 15 fathoms of the surface, as it had done on several previous occasions.

26th.—Mr. Varley arrived in a steam tug, having, during the previous night, on his passage from St. John's, encountered such a heavy gale that they had to take shelter.

27th.—The steamer left Rix Harbour in a dense fog and stiff breeze, to examine buoys in position. While out the weather suddenly cleared and moderated, and the sea calmed by the drenching rain. Grappelling was resumed in 114 fathoms. The cable was hooked several times, and, with one exception, parted before reaching the surface. Care was taken to buoy the spot the moment the cable broke, and by grappelling from one quarter to one half mile east of the buoy, we hoped to succeed in raising the bight; and did at last get it on board. On testing the cable towards Ireland, it was found to be broken at a very short distance from the vessel, three quarters of a mile of cable being recovered before it parted again at a weak place.

28th.—The wind and sea too high for working. A fresh consultation was held as to the best mode of proceeding, and it was resolved to go further out at once, hoping thereby to avoid the rocky ground, and the bad state of the cable.

29th.—Grappelling was resumed three miles further east. Soon after getting to work the weather suddenly changed, and the sea ran so high that we had to run into harbour.

30th. Re-commenced operations with steamer and boats, in calm but densely foggy weather. The fog just cleared up long enough to determine our position. The "Industry" was accordingly anchored in 130 fathoms as a beacon, and grappelling performed by the steamer. The cable was hooked at least three times—and probably more during the day, but broke before reaching the surface. At last a bight came on board, the cable at this spot being unusually good for about 30 yards. The outer end was found to be broken about 200 yards off. About two miles of the inner end were recovered, when it parted again, at a weak place, where there was nothing but the gutta-percha covered wire left; this, however, was just able to bring the cable to the surface when it snapped before it could be secured by a stopper. The point where we last grappelled this day was a little east of a straight line joining Tickle Point and Copper Island in 140 fathoms water. Although mud is shown on the charts, there are most unquestionably rocks also, as was too plainly indicated by the state of the cable, rock weed and sea animalcules adhering to and surrounding it in many places, showing that it had been suspended clear of the bottom. The cable was invariably hauled in by hand, to avoid unnecessary strain. The recovered cable varied in condition very much, and what is most important is, that even those portions which came out of the black mud were so perished in numerous patches, that the outer covering parted on board during the process of hauling in, and but for the dexterity and courage of the men in seizing hold of it beyond the break, where the iron wires stuck out like highly sharpened needle points, we should not have known so much of its condition. In a word, it was evidently sometimes embedded in mud, sometimes on small stones, sometimes half imbedded, and sometimes wholly exposed over rocks, as was apparent from the condition of the outer covering. The iron wires in many places often appeared sound, but on minute inspection were found eaten away and rotten; the sewing was also observed. In some places the iron wires were coated with metallic copper, and much eaten, they having most probably raised upon copper ore, for there are veins of it in Trinity Bay. The gutta percha and copper wire, are, however, in as good condition as when laid down. The general rugged, mountainous, and rocky character of the surrounding land eventually extends below the surface of the water. The unevenness of the soundings and condition of the cable indicate this most plainly. We accordingly decided, upon leaving the neighbourhood of Bull's Island altogether, as the cable in its present state at that part of the bay will not repay the cost of recovery. We agreed simultaneously to attempt to raise the cable off Heart's Content, and ascertain its condition there, this being the most promising part of the bay, as the information we have been able to collect.

Accordingly, on the 1st of July, we sailed to New Perlican, and made preparations to start at 8 a.m. on Monday morning.

On the 2nd of July sailed from New Perlican at 7 a.m., and grappelled for the cable in a smooth sea. At 7 a.m. we hooked in 143 fathoms water in a straight line joining the north point of St. John's harbour and the south point of New Perlican, at a distance from the latter of about six miles. Having taken bearings, grappelling was resumed half a mile nearer to the entrance of Trinity Bay, and the cable again and again hooked, each time about half a mile north of the previous run. The cable was during the day hooked at least four times on bottom more. It sometimes lifted off the ground before parting as much as 40 fathoms, sometimes only 15; in no instance did it come near the surface of the water. On two occasions the iron strands of the cable left most unmistakable impressions on the grapnel, and iron rust, resembling that usually found on the cable, adhered to its claws. The bottom consisted of green mud and light-coloured clay, the latter very compact, and, in consistency, not much unlike the blue clay of London. Some parts of the bottom were of stone. Having found it quite impossible to raise the cable, we concluded, after careful consideration, to make a last but hopeless trial off Break Heart Point, at the mouth of Trinity Bay, and, if unsuccessful, to take the steamer and run to St. John's to avoid further expense.

On July 3rd the steamer sailed from New Perlican at 6·6 a.m., and reached Break Heart Point a little before 4 a.m. We grappelled for the cable from about six miles and a half off in 165 fathoms water to within one mile and a half of the point where the water was still over 100 fathoms. We did not succeed in finding it, and had to leave on the Atlantic roll setting into the bay was so heavy and the current running out so strong, that we could not possibly have raised it to the surface, but only have determined its position. It is quite possible that the cable was hooked without being perceived by us, owing to the depth of water, and to the fact that the cable, especially where laid over stone, is very rotten. At six miles out the bottom consisted of clay, covered by a thin stratum of mud, the same as that off New Perlican. At about four and a half or five miles off the bottom appeared to consist of stones, and this continued to within one mile and a half of the "point," where the water was very deep.

These portions of the recovered cable that were wrapped with tarred yarn were sound, the tar and hemp having preserved the iron wires bright and free from rust. This will be further reported on when the pieces of recovered cable have been more closely examined.

It is with deep regret that we have to inform you that it has been necessary to abandon the cable.

(Signed) CROMWELL F. VARLEY,
Electrician to the Electric and International and the Atlantic Telegraph Company.
(Signed) JOHN KELL.

To the Chairman and Directors
of the Electric Telegraph Company.

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